Does Fairness Prevent Market Clearing? An Experimental Investigation

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DOES FAIRNESS PREVENT MARKET CLEARING?
AN EXPERIMENTAL INVESTIGATION*

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This paper reports the results of an experiment that was designed to test the impact of fairness on market prices. Prices were determined in a one-sided oral auction, with buyers as price-makers. Upon acceptance of an offer, sellers determined the quality of the good. Buyers offered prices that were *substantially* above the market-clearing level and expected sellers to respond with high quality levels. This expectation was, on average, confirmed by the behavior of sellers. These results provide, therefore, experimental support for the fair wage-effort theory of involuntary unemployment.

I. INTRODUCTION

Fairness is an elusive term. Yet, in actual business practice the parties involved in a transaction seem to refer quite frequently to the notion of fairness. This observation had already been made by Marshall [1925], who wrote: ". . . the phrase is constantly used in the market place; it is frequent in the mouths both of employers and the employed; and almost every phrase in common use has a real meaning, though it may be difficult to get at." In a recent study Blinder and Choi [1990] asked nineteen managers whether they and their workers would perceive a wage reduction to take advantage of labor market slack as (a) completely fair, (b) acceptable, (c) unfair, or (d) very unfair. While three managers considered this as an irrelevant question, all but one of the remaining sixteen said that such a wage cut is unfair or very unfair. Moreover, all but one believed that their workers would consider this wage cut as unfair or very unfair. In their study of community standards of fairness for the setting of prices and wages, Kahneman, Knetsch, and Thaler [1986] infer rules of fairness for conduct in the market from their interviewees' answers to hypothetical questions. Their empirical findings confirm the existence of such rules.

However, what people say is one thing, but what they actually do may be quite a different thing. The possible divergence between

revealed opinion and actual behavior raises the question of to what extent fairness considerations affect the behavior of people in markets. In several papers Akerlof [1982] and Akerlof and Yellen [1988, 1990] have put forward the view that fairness-oriented behavior of workers may lead to involuntary unemployment. Based on psychological and sociological theories [Adams, 1963; Homans, 1961] and on experimental evidence,[1] they postulate a positive relationship between work effort and wages. Because of this relatiqnship, it may be profitable for employers to pay wages above the market-clearing level.

There is by now overwhelming evidence that experimental markets converge toward the competitive equilibrium under rather weak conditions. This raises the question whether equity and fairness effects, which have been observed by psychologists in nonmarket experiments, will survive in a competitive market [Smith 1991]. To our knowledge, the fair wage-effort hypothesis has not been tested in the context of a competitive experimental market. Our two-stage market experiment may be viewed as such a test of Akerlof and Yellen's efficiency wage approach. In the first stage of the experiment, a one-sided oral auction took place in which employers and workers could exchange one unit of labor time. In the second stage, workers chose their effort levels.

Our experiment can, however, also be interpreted as a stylized version of certain kinds of goods or service markets. In many markets, the price of the good or the service is fixed before the good is produced or the service is rendered. If the quality of this good or service cannot be completely specified in the contract, or if the quality is not verifiable by third parties, a similar problem arises as in the labor market.

The sellers in our experiment had no pecuniary incentive to raise the quality of the good above the exogenously given minimum. Therefore, if self-interested buyers expect sellers to be pure money maximizers, that is, if they expect sellers *always* to choose the minimum quality, they have no reason to pay prices above the market-clearing level. Contrary to this prediction, however, most buyers tried to induce sellers to choose higher quality levels by offering them high ("fair") prices for the good. Buyers offered prices that were on average *substantially* (by more than 100 percent) above the market-clearing level and expected sellers to reciprocate, that is, to respond with "fair" quality levels. This

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1. See Mowday [1991] for a recent survey.

expectation was *on average* confirmed by the behavior of sellers, although buyers' attempts have also been frustrated in specific cases. As the experiment was repeated, there was no tendency for prices to converge toward the market-clearing level.

II. THE EXPERIMENTAL DESIGN

The fair wage-effort hypothesis should be distinguished from the shirking version of the efficiency wage hypothesis [Bowles, 1985; Fehr, 1984, 1986; Shapiro and Stiglitz, 1984; Stoft 1982].[2] In the latter, wage variations are accompanied by different degrees of punishment for shirking. A wage increase raises the costs of shirking because if the worker is detected shirking and is fired, he will lose more money. The fair wage-effort hypothesis stipulates, however, that wage increases raise workers' effort levels even in the absence of any increase in the penalty for shirking.

In Fehr, Kirchsteiger, and Riedl [1992] we designed an experimental test of the shirking version of the efficiency wage hypothesis. To test experimentally for the validity of the fair wage-effort hypothesis, we set up the following two-stage game. The first stage was a one-sided oral auction with employers as bidders which lasted three minutes. At this stage employers made wage proposals, but they had no opportunity to choose the worker with whom they traded because every worker could accept every offer. Employers' wage offers had to be multiples of five.[3] If a worker accepted an offered wage p, a binding contract was concluded, and stage 1 was finished for both the worker and the employer. If an employer's bid was not accepted, he was free to change his bid, but the new bid had to be higher than the previous highest bid (possibly from other employers) which had not yet been

2. In the following, we frame our presentation in labor market terms because, by referring to other labor market theories, the logic of our argument can be most easily illustrated. The labor market terms, however, can be easily replaced by equivalent goods market terms. Instead of, e.g., fair wage-effort hypothesis, we could also use the term fair price-quality hypothesis. In fact, our experiment was framed in goods market terms. Neither in the instructions, nor in our oral explanations, did we use terms like employer, worker, labor market, effort, etc. Instead, we spoke of buyers, sellers, goods, conversion rates, etc. The reason for this is that we conjecture that fairness considerations are more prevalent in labor markets than in goods markets because, in general, the social interaction between firms and their representatives, on the one hand, and workers, on the other hand, is more intensive than the interaction between buyers and sellers in goods markets. Therefore, agents in our experiment would have been more likely to exhibit fair behavior if they had identified with employers and workers.

3. Instead of introducing a commission fee, we imposed this restriction. It enabled workers to earn a small amount of money at marginal trades.

accepted. After three minutes the market was closed, and those parties who did not succeed in trading earned zero profits for this period. At the second stage, workers had to choose their effort *anonymously*; i.e., their choice was revealed only to "their" employer. Moreover, their choice was completely unconstrained in the sense that there were no sanctions associated with it.

In total, we organized four experimental sessions. In all sessions we deliberately created an excess supply of workers. In three, sessions we had nine workers and six employers, and in one session there were eight workers and five employers.[4] In each session there was at least one trial period with only the first stage in order to allow agents to become acquainted with the one-sided oral auction. Stage 1 and stage 2 together constituted one period, and to allow for learning effects, we had twelve periods in each session. A session lasted approximately two hours, and on average an experimental subject earned 296 Austrian Schillings (AS 296 ~ £14,5 ~ $25) per session.[5]

Before the beginning of a session, each subject had to choose a card. If there was an S on the card, she/he was a seller (worker), if a B, she/he was a buyer (employer). Workers and employers were located in different rooms. During the experiment communication took place by means of a telephone. Four supervisors were engaged in each session: two in the employers' room and two in the workers' room. In each room one supervisor transmitted the price (acceptance) and effort messages over the telephone. While price messages were public knowledge, the information about effort choices was coded. It was known only to the two parties involved. In addition, employers and workers did not know the identity of their trading partners. These information restrictions have been chosen to exclude group pressure effects on effort choice and to reduce strategic spillovers between periods as much as possible. Moreover, we wanted to rule out the possibility of hidden side payments between parties after the experiment.

TABLE I
$m(e)$-Schedule

e	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
$m(e)$	0	1	2	4	6	8	10	12	15	18

Although we think that in real labor processes group pressure effects and repeated game effects play an important role in enhancing cooperation and developing fairness ties among workers (employers), as well as between workers and employers, we wanted to isolate pure one-period-fairness effects. In this respect, the conditions were most unfavorable for fairness effects to occur. It was, for example, impossible for employers (workers) to reward the past action of a specific worker (employer) because they did not know the identities of their trading partners.

Let e_j denote the effort of worker j, and let p_j be the wage for one unit of labor time accepted by j. The monetary effort costs for workers were given by the increasing function,

$$(1) \qquad m = m(e), \qquad m(e_{min}) = 0.$$

Schedule (1) was the same for each worker. $e_{min} > 0$ represented the minimum effort level; it was associated with zero effort costs. The monetary costs of providing one unit of labor time were c, and the total monetary payoff of worker j, who received a wage of p_j and provided effort of e_j, was given by

$$(2) \qquad u_j = p_j - c - m(e_j).$$

The payoff of employer i whose worker has chosen an effort of e_i was determined by

$$(3) \qquad \Pi_i = (v - p_i)e_i.$$

ve_i may be interpreted as the revenue of the employer; one unit of effort produces v units of output which is sold for a price of one. The assumption that wage costs vary also with the level of e_i has been made to rule out the possibility of losses for employers. In all four experimental sessions we fixed $v = 126$, $c = 26$; whereas the $m(e)$-schedule is given by Table I.

Although it is difficult to give an exact definition of fairness,[6] the term necessarily involves some comparison between the gains of transactors. Therefore, if an agent is motivated by fairness

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6. In Fehr [1991] an attempt is made in the context of ultimatum games.

considerations, his actions are based not only on his own gains, but also on the gains of other parties. Our experiment aimed at detecting whether agents exhibit intrinsic fairness preferences, and whether they base their actions on the expectation that others act fairly. To allow for this possibility, we avoided any use of terms like fairness, equity, etc., but we informed all workers and employers about v, c, the cost of effort schedule $m(e)$, and about the number of employers and workers. We also told them that everybody knows these parameters.[7]

III. PREDICTIONS WITH MONEY-MAXIMIZING AGENTS

Early research [Smith 1964] about the effects of one-sided oral auctions has shown that if only buyers are allowed to make price quotations, the mean price per trading period tends to converge to prices which are slightly above the competitive market-clearing price.[8] The fact that mean prices are only slightly above the competitive price has been confirmed by a study of Plott and Smith [1978] for the multiple-unit case: in both of the oral bid experiments they conducted, the market-clearing price was $0.60.[9] After seven periods the mean price in one experiment was $0.6065; in the other it was exactly 0.60. In a more recent extensive study [Walker and Williams, 1988] of one- and double-sided auctions, the authors could not find significant differences in mean prices across trading institutions. Mean prices in bid- offer- and double-auctions tended to converge to the competitive equilibrium price. It seems that the results provided by Walker and Williams are now accepted as showing that oral bid markets converge to the competitive equilibrium, and that the process of convergence is not different from

7. To make sure that the experimental subjects are capable of computing their own monetary payoffs, and the payoffs of their (unknown) trading partners, they had to solve a problem before the start of the trial period. The experiment did not start until all subjects had solved this problem correctly.

8. Smith reports the results of two experimental sessions. Session 1 had six periods; ten sellers and ten buyers each of whom could trade one unit per period. Session 2 lasted five periods with fourteen buyers and fourteen sellers as participants. In both sessions the competitive price was $2.10; the highest redemption value was $3.15, whereas the lowest marginal cost was given as $1.20. From charts 5 and 6 in Smith [1964] we infer that in either session the average price in the last two periods was well below $2.25 and above $2.10.

9. For each trader the marginal costs of the first units sold were strictly smaller than $0.60 (marginal costs of the first unit was 0.26 (0.28) in experiment 1 (2)). The redemption values of the first units bought were strictly above 0.60 (for the first unit 0.92 (0.94) in experiment 1 (2)). Thus, the competitive price involved positive profits for each trader.

convergence in oral double auctions or oral offer markets [Plott 1989, p. 1126].

Suppose for a moment that all agents in our two-stage experiment are money maximizers, and that this fact is common knowledge. Since, first, effort is costly for the workers and, second, they cannot be punished for providing a low effort, they have no pecuniary incentive to choose $e > e_{min}$. It follows that rational employers will, irrespective of their wage offers, expect e to be equal to e_{min}. At $e = e_{min}$ the opportunity cost of accepting a job is given by $c = 26.$ Remember that wage offers have to be multiples of five and that there is an excess supply of workers. Under these conditions, the competitive equilibrium wage is equal to 30. Notice further, that, since everybody knows that workers will always choose e_{min}, the addition of a second stage to the oral bid auction does not change the bidding and acceptance incentives relative to a familiar one-stage auction. In view of the above cited previous research, one should expect, therefore, wages to converge toward 30. In the following we call the outcome $[p = 30, e = 0.1]$ the standard prediction, and the above reasoning that leads to this prediction the standard theory.

At a wage of 30, workers earn $\tau \equiv 30 - c = 4$ AS. We interpret τ as a compensation for a real input, namely, the effort of active participation. Therefore, we say that workers who trade at $p = 30$ receive their reservation wage (in case that $e = 0.1$), while if some trading workers receive $p > 30$, nontraders are involuntarily rationed because they would strictly prefer to be a trader at $p > 30$. More generally (i.e., for $e \geq 0.1$), nontraders are involuntarily rationed, if some workers received $p_j > 30 + m(e_j)$.

IV. THE FAIR WAGE-EFFORT HYPOTHESIS

In this section we introduce the hypotheses that will be used to test the validity of the fair wage-effort theory. In contrast to standard theory, the fair wage-effort hypothesis stipulates that the effort level e depends positively on the wage and that, therefore, employers may be willing to pay a wage greater than the market-clearing wage. This leads to

HYPOTHESIS 1. The effort level is increasing in the wage.

Of course, the situation in the experiment is very unfamiliar to the subjects. Therefore, one cannot expect them to act in a fully

rational way from the beginning. In particular, since employers do not know to what extent workers will exhibit reciprocal behavior, they may not offer the appropriate wage at the beginning. But when the game is repeated, the price of labor should converge to the market-clearing wage if fairness plays no role. However, if workers behave according to Hypothesis 1, wages above the market-clearing level should be observed. This leads to

HYPOTHESIS 2. Average wages in the experiment are considerably greater than the market-clearing wage $c + \tau$. Furthermore, if the game is repeated, the average wage does not converge to $c + \tau$.

Combining Hypotheses 1 and 2 gives us

HYPOTHESIS 3. The average effort per period is above e_{min}. If the game is repeated, it does not converge to e_{min}.

To investigate Hypothesis 2, we define an "average relative overpayment" variable r. This variable is just the difference between the average wage of a period p^0 and the market-clearing wage divided by the surplus:

$$(4) \qquad r = (p^0 - c - \tau)/(v - c).$$

If Hypothesis 2 is true, r should be considerably greater than zero and should exhibit no tendency to converge to zero. To test Hypothesis 1, we fit the regression[10]

$$(5) \qquad e = \alpha + \beta \cdot p + \mu,$$

where μ is white noise. If β is significantly greater than zero, Hypothesis 1 cannot be rejected. Regression (5) does not take into consideration that fairness notions may vary substantially across workers. There may be different types of workers with respect to their idea of what is a fair response to a certain wage. Of course, in "real world" situations, which are repeated many times, the individuals' conception of fairness may converge toward a common norm. But this could not happen in our experiment because workers did not know the effort levels chosen by other workers (see Section II). Therefore, a worker had no opportunity to adjust his

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10. All regressions reported in this paper are estimated with OLS. But we also run two-sided censored Tobit regressions, to take into account the nonlinearity of the wage-effort relation. Furthermore, we estimated log-linear regression models with OLS and two-sided censored Tobit. Because the main results of these different specifications do not differ from the OLS estimation of the linear model, only the results of the latter are reported in Section V.

individual fairness notion to that of the others. A natural way to tackle this problem is to use dummy variables d_i for the workers to run the following regression:

$$(6) \qquad e = \sum_i \gamma_i d_i + \beta p + \mu.$$

To support Hypothesis 1, β again has to be significantly greater than zero. To test for the significance of behavioral differences among workers, we also computed the Wald-statistic for the null hypothesis that all estimated γ_i are equal to the estimated α-coefficient of regression (5).

It is also possible that effort varies systematically across periods. In order to test for this effect, we use period dummies p_t which have value one if the relevant observation is made in period t and value zero otherwise. This leads to[11]

$$(7) \qquad e = \sum_t \theta_t p_t + \beta p + \mu.$$

To test whether at least some θ_t differ significantly from α, we again computed the Wald-statistic.

Finally, we take a look at efficiency. If worker j sells his labor time to employer i, the sum of their payoffs is given by

$$(8) \qquad G_{ij} = \pi_i + u_j = e_j(v - p_i) + p_i - c - m(e_j).$$

As already mentioned, the standard theory predicts that the wage equals $(c + \tau)$ and that workers choose $e_{min} = 0.1$. Therefore, the joint monetary gain predicted by standard theory is rather small. It is given by

$$(9) \qquad G_s = e_{min}(v - c - \tau) + \tau = 0.1*96 + 4 = 13.6.$$

Note that G_s is substantially below the maximum total gain that could be achieved by an employer-worker pair. Therefore, if all agents are money maximizers, there is a conflict between individual and collective rationality. Fairness considerations may, however, help the subjects resolve this conflict at least partly.[12]

As a measure of the efficiency of the transaction between i and j, we use

$$(10) \qquad f_{ij} = G_{ij}/G_s.$$

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11. In general, one should, of course, use a model with dummies for workers and periods. However, this would lead to an intolerable increase in the number of dummies relative to the number of observations.

12. For the effects of fairness on efficiency in an ultimatum game, see Güth, Ockenfels, and Wendel [1991].

TABLE II
THE WAGE-EFFORT RELATION

Wage	Average observed effort level	Median observed effort level
30–44	0.17	0.1
45–59	0.18	0.2
60–74	0.34	0.4
75–89	0.45	0.4
90–110 ,	0.52	0.5

Because $m(e_{min}) = 0$, the highest possible G is nearly 100 (when the wage is 125). Therefore, f_{ij} will be somewhere between one and seven.

V. EXPERIMENTAL RESULTS

On the whole, the experimental results are rather favorable for the fair wage-effort hypothesis.[13] The lowest price observed was 30 (one time in the tenth period of the third session), the highest 110 (one time in the seventh period of the first session). The average price taken over all observations in the four sessions was 72, the average relative overpayment was 0,42. On average, employers gave workers 42 percent of the surplus, although there were more workers than jobs. On the other hand, workers chose all possible effort levels from 0.1 to 1. While e_{min} was chosen in 44 of the 276 cases (16 percent of the cases), e_{max} was chosen in six cases. The average effort chosen by the workers was 0.4 — four times as high as predicted by standard theory.

To check Hypothesis 1, we have constructed Table II. It shows the observed average and median effort levels of workers in response to a wage within a certain interval.

As can be seen, average and median effort increased with the wage. The positive relationship between wages and effort is confirmed by Figure I which is based on the observations of all four experimental sessions. It shows that the graph of regression (5) is sloping upward. In addition, it exhibits the average effort levels[14]

13. The data set is available from the authors upon request.
14. Due to the discreteness of our data, different workers chose the *same* effort level at a particular wage several times. As a consequence, many observations are not visible in a two-dimensional figure of effort-wage data points. Such a figure would, therefore, provide a misleading picture of the evidence. To avoid this problem, we plotted the average effort.
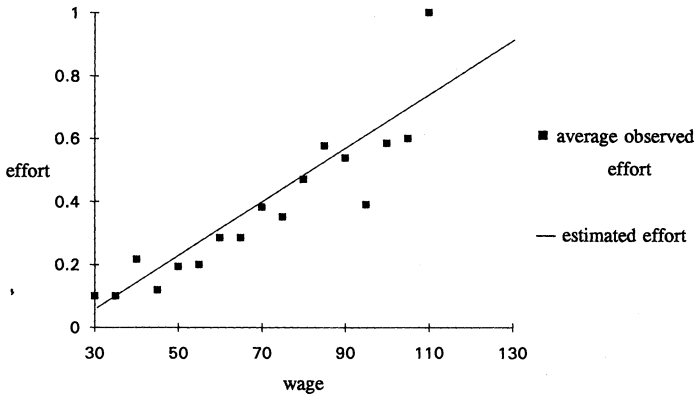
FIGURE I
The Wage-Effort Relation

that were associated with different wages. The pattern of these data points is also clearly sloping upward.

In Table III we present the results of regression (5) with the whole data set as well as with the data of the individual sessions and with the last period observations of all sessions.

Notice that the β-coefficient is positive and highly significant in all regressions. In Table IV the results of regression (6) are listed.

Table IV confirms the results of Table III with respect to the sign and significance of β. Furthermore, the adjusted R^2s of Table IV are approximately two times the R^2s of Table III. As the

TABLE III
RESULTS OF REGRESSION (5): $e = \alpha + \beta p + \mu$

	N	α	$t(\alpha)$	β	$t(\beta)$	R^2
S1-4	276	−0.18	−3.1	0.0078	9.6	0.25
SL1-4	23	−0.6	−2.2	0.0129	3.5	0.34
S1	72	−0.27	−2.8	0.0076	6.2	0.34
S2	72	−0.34	−2.3	0.0111	5.4	0.28
S3	72	−0.14	−1.6	0.0066	4.9	0.25
S4	60	−0.38	−1.7	0.0113	3.9	0.19

S#: Session#.
SL1-4: Results of the estimation with the last period data of all sessions.
N: number of observations.
t(): t-value of the relevant coefficients.
R^2: Adjusted coefficient of determination.

TABLE IV

RESULTS OF REGRESSION (6): $e = \sum_{i=1}^{n} \gamma_i d_i + \beta p + \mu$

	N	n	β	$t(\beta)$	R^2	W-st	prob
S1-4	276	35	0.0076	10.8	0.6	275	0.000
S1	72	9	0.0067	5.9	0.61	56.9	0.000
S2	72	9	0.0081	5.4	0.65	81.3	0.000
S3	72	9	0.0072	6.3	0.51	45.1	0.000
S4	60	8	0.0118	4.4	0.38	25.0	0.002

S#: Session#.
  N: Number of observations.
  n: Number of workers.
  $t(\beta)$: t-value of the β-coefficient.
  R^2: Adjusted coefficient of determination.
  W-st: Wald-statistic for the hypothesis that all γ_i are equal to α (the relevant α can be found in Table III).
This statistic is CHI(n-1) distributed.
  prob: Significance level of W-st.

Wald-statistic shows, the hypothesis that all γ_i are equal to α has to be rejected for the whole data set as well as for the data of each individual session, even at a significance level of 1 percent. This allows us to conclude that the intercept of the effort-wage relation differed across workers.

The results of regression (7) are shown in Table V. The β-coefficient is again always significantly positive, but the adjusted R^2s are lower than those of regression (5) (compare Table V with Table III). Furthermore, the hypothesis that all θ_t are equal to α

TABLE V

RESULTS OF REGRESSION (7): $e = \sum_{t=1}^{12} \theta_t p_t + \beta p + \mu$

	N	β	$t(\beta)$	R^2	W-st	prob
S1-4	276	0.0077	8.9	0.23	5.3	0.93
S1	72	0.008	5.5	0.29	5.5	0.94
S2	72	0.0117	4.4	0.28	10.2	0.61
S3	72	0.005	2.5	0.16	3.9	0.98
S4	60	0.0139	4.2	0.18	9.9	0.62

S#: Session#.
  N: Number of observations.
  $t(\beta)$: t-value of the β-coefficient.
  R^2: Adjusted coefficient of determination.
  W-st: Wald-statistic for the hypothesis that all θ_t are equal to α (the relevant α can be found in Table II). This statistic is CHI(11) distributed.
  prob: Significance level of W-st.

cannot be rejected for any regression at a significance level of 60 percent (!). This indicates that workers do not behave significantly differently in different periods.

Tables III–V reveal evidence in favor of the hypothesis that the effort decision of the workers depends positively on the wage, and on their fairness notion but not on the timing. To examine Hypothesis 2, we take a look at the evolution of the average relative overpayment per period, r, over time.

As can be seen from Figure II, r is significantly greater than zero in all periods of all four experimental sessions. Furthermore, r does not converge toward zero, although each session took twelve periods. In session 1, r was decreasing in the eleventh and twelfth periods. But such a decrease can also be seen in the fourth and
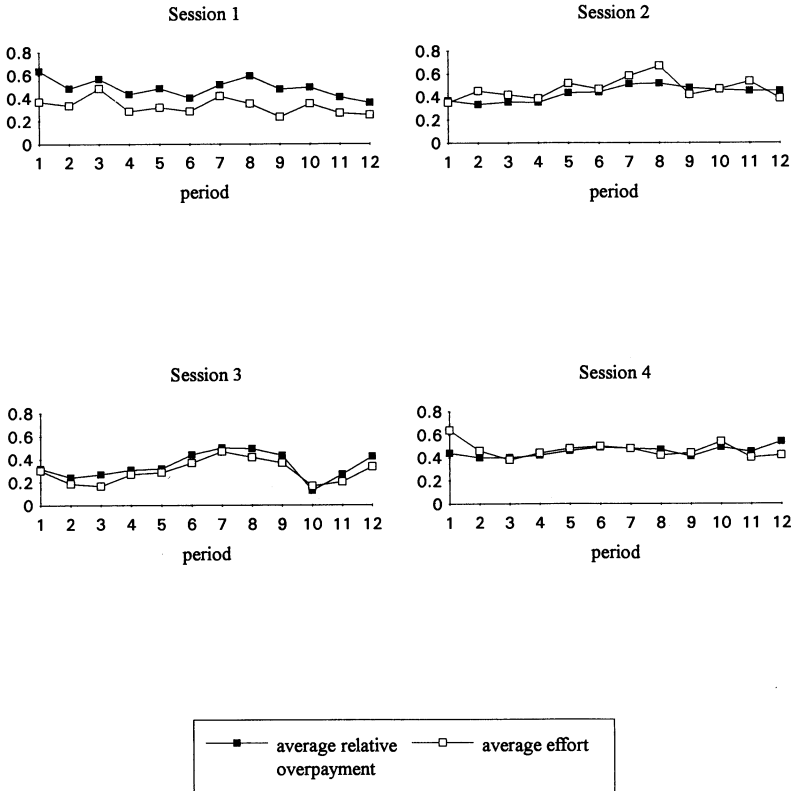

FIGURE II
Average Relative Overpayment and Average Effort per Period

sixth periods, each followed by an increase in r. In session 2, r was nearly constant over the last periods. In session 4, r was increasing in the last period. In session 3 a sharp decrease of r in the ninth and tenth periods was followed by a similar sharp increase of r in the last two periods. Moreover, except for session 1 the value of r in the last period *exceeds* its value in the first five periods. On the whole, one cannot conclude from the data whether the price converges to a particular positive level, but it clearly does not converge to the market-clearing level. Therefore, Hypothesis 2 seems to be supported by the data.

Figure II also shows the evolution of the average effort per period over time. Contrary to the standard prediction, average effort exceeds e_{min} in all periods of all four sessions. Figure II also confirms once more that wages and effort are positively related. As with average wages, there seems to be no convergence toward the standard prediction. Therefore, Hypothesis 3 is supported by the data.

The payment of high wages and the associated positive effort response of the workers increased the efficiency of almost all transactions considerably. The efficiency measure f_{ij} was one in only one case, as predicted by standard theory. On average, f was 4.5, and in eight cases it was even higher than six. Thus, reciprocal behavior increased the overall gains by several hundred percent.

VI. INTERPRETATION AND OBJECTIONS

In our view, the above results indicate that fairness effectively prevented the wage from decreasing to the market-clearing level. Firms expected that paying high wages will, on average, induce workers to reciprocate, i.e., to choose high effort levels. When employers made their wage offers, they took into account that the effort levels of workers depended, contrary to the standard prediction, on the wage. This hypothesis is not only based on our experimental results, but is also supported by two additional observations.

First, since we deliberately created an excess supply of workers, which was explained to all agents from the very beginning, employers knew that workers were willing to accept virtually any offer above the market-clearing wage. Moreover, during the experiment this fact was established over and over again. Competition among workers was extremely intense, and the most difficult task of the supervisor in the workers' room was to detect which subject

cried "accepted" first. Most wage bids were accepted after fractions of a second. Employers very quickly noticed that their bids were accepted immediately. This was also true for wages well below average wages, for example, in the tenth and eleventh periods of the third session. Furthermore, this conclusion was confirmed by the workers' answers to a questionnaire. When asked for the reason why they did not trade, they answered in only ten of the 144 cases that they had received no acceptable offers. In all other cases, the offers acceptable to them had already been accepted by other workers. Almost always, the "unemployed" were simply not fast enough; faster workers snatched the jobs away from them. Obviously, the unemployed would have preferred to get a job; their unemployment was involuntary. Despite the existence of involuntary unemployment, most employers did not try to offer wages that were (relative to average wages) closer to the market-clearing level.

This fact leads us directly to the second observation. At the end of an experimental session, a questionnaire was given to each employer. When asked about the motives for their price quotations, most employers answered that they tried to get a high effort from "their" worker by offering a relatively high price. Two employers answered that they tried to share equally with "their" workers. One answered that initially he tried to pay a low wage but that he later changed his behavior and paid a high wage to get a higher effort. Only one of 23 employers said that he tried to pay a low wage in all periods.

Employers' hope that fairness considerations will induce workers to choose a high effort in response to high wages has been justified. This can also be seen from the answers the workers gave when asked at the end of the experiment for their motives in making their effort choice. Two workers answered that they tried to equalize their gains with the profits of their employers. Twenty-nine of the 35 workers answered that their effort decisions were dependent on the wages they received. The higher the wage, the greater was their effort. Only six workers answered that they simply tried to get a high gain when choosing their effort. But even the average effort chosen by these six workers was about 0.25, considerably greater than e_{min}.

Since each experimental session lasted twelve periods, one might suspect that our data are generated by a desire to gain a reputation. Workers might have reasoned that if they provide a high effort to employer j in the present period, they will receive a high wage offer from employer j in future periods. If it had been

possible to profit in this way from a reputation as a high-effort worker, our interpretation would indeed be questionable. Yet, our experimental procedures did not allow individual subjects to gain such a reputation. Remember that workers and employers were located in different rooms, and that employers (workers) could neither choose their trading partners, nor identify their trading partner. Moreover, the effort choice of workers was revealed neither to other workers, nor to the other employers. It was, therefore, impossible for experimental subjects to reward the past action of a specific trading partner.

The claim that reputation formation is unlikely to be the driving force behind the deviation from the standard prediction can also be supported by the regularities of the data. Toward the end of the game, the pecuniary value of a good reputation becomes increasingly lower, and in the last period workers can gain nothing from a high effort. Thus, if reputation formation were the major motivational factor, we should have observed declining effort levels toward the end, and furthermore, in the twelfth period the average effort should have been close to e_{min}. But this prediction is invalidated by the effort data exhibited in Figure II, and by the results of our regression. Notice that the data of each session do not support the hypothesis that the wage-effort relation differs across periods (see Table V), and that a significant positive relation could also be established with the data of the twelfth period (see SL1-4 in Table III).

In our experiment employers could not suffer losses. A frequent (Darwinian) argument in discussions about fairness in the market place is that, in the long run, fair behavior will be driven out of the market. It might seem, therefore, that the impossibility to experience losses represents a severe restriction. Yet, if workers behave reciprocally, paying high (fair) wages is not profit reducing but profit increasing. In the previous section we mentioned that subjects were able to increase considerably the efficiency of the transactions, relative to the standard prediction. Employers reaped part of this efficiency gain. The average profit per period of employers in all four sessions was AS 18.9, while according to the standard prediction they should earn only AS 9.6. In session 1 (session 2) average profits per period amounted to AS 13.4 (AS 23.9); in session 3 (session 4) they were AS 16.2 (AS 22.8).

A further objection to our interpretation of the data could be that experimental subjects might want to seem fair to the experimenters, but not to each other. For several reasons we think that

this interpretation is unlikely to account for the empirical regularities. First, the subjects were not students of ours. To them we were strangers, just like the other subjects. Second, they were recruited with the help of an information sheet in which any use of terms like fairness, equity, etc. was avoided, whereas we explicitly noted that they could earn "a considerable amount of money." To earn money seemed to be a major motivation for their participation. Third, in pure market experiments (without "our" second stage), it has been observed that a rapid convergence toward the competitive equilibrium occurs in a wide variety of circumstances. Smith [1976, 1980, 1982] reports that even in the case of similarly strong rent asymmetries as in our experiment,[15] and even if all agents were completely informed about these asymmetries, convergence could be observed. Although in the competitive equilibrium of these experiments, buyers (sellers) earned the whole rent (zero profits), buyers still did not want to seem fair in front of the researcher. Sellers, however, who "believed that it is 'fair' for trading profits to be shared between buyers and sellers (tried) to resist price decreases more vigorously than when they do not know what constitutes such a fair price" [Smith 1976, p. 278]. In accordance with Smith, we think that the *combination* of an extreme rent asymmetry, with complete information about payoffs, opens the way "for 'equity considerations' to modify self-interest choices" [1976, p. 278]. But, whereas in a one-stage market experiment sellers' 'equity considerations' are only temporarily successful in preventing a fall in the price, the addition of the second stage renders them much more powerful. Our results indicate that the effort decision is a very effective weapon for sellers, because it renders a high wage policy profitable for the buyers.

VI. CONCLUDING REMARKS

On the whole, our results provide evidence for the validity of the fair wage-effort hypothesis. But, of course, some questions remain open. The experiment is designed to test experimentally the "pure" fair wage-effort hypothesis. Therefore, our experimental design does not include features that are important in "real world" labor relations. In reality, employers and workers have the possibility to decide whether they want to stay together or not. In fact, most employment relations exhibit some durability. Long-term

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15. Recall that if the standard outcome prevails, the whole rent is reaped by the firms.

interaction among workers, and between workers and employers, is likely to enhance the development of social ties that may well increase the fairness and reciprocity effects relative to the one-shot game. Of course, in long-term relations reputation effects and purely pecuniary incentives for high effort levels will in general exist. Until now, however, little is known about the impact of these long-term effects on the outcomes of experimental markets.

In reality, firms also have some possibilities to control the effort of their employees. In most cases, they have no perfect control, but there is some probability that "lazy" workers will be punished, e.g., by losing their jobs. It is not clear how the possibility of punishing low effort levels will interact with fairness. On the one hand, it may weaken the impact of fairness. On the other hand, fairness considerations may then be applied to the severity and kind of punishment, too. If the latter is the case, it can be shown theoretically [Fehr, 1991] that bonding and entrance fees may not be sufficient instruments to allow for market clearing. Whether this hypothesis can be established to hold in experimental markets, however, is left to future research.

APPENDIX: INSTRUCTIONS

The instructions given to the subjects were, of course, written in German. As already mentioned, we framed the experiments in goods market terms. The employers are called buyers, the workers are called sellers, the wage is called price, etc. Notice that buyers and sellers were located in separate rooms.

General Instructions (for both market sides)

The experiment you will participate in is part of a research project financed by the Austrian Science Foundation. It is used to analyze the decision behavior in markets. The instructions are simple, and if you read them carefully and make appropriate decisions, you can earn a considerable amount of money. At the end of the whole experiment, all the profits you have made by your decisions will be added up and paid to you in cash. The experiment you will participate in consists of two stages. In the first stage six of you act as buyers, and nine of you as sellers. In the second stage, the sellers will determine the value of the goods for the buyers (for details of the second stage see below). We have distributed two kinds of instructions—information for the buyers, and information for the sellers, respectively. This information is for private use only—you

are not allowed to reveal this information to anyone. Furthermore, you will find at the end of these instructions a second sheet (sheet 2) that is used to document your decisions. Insert your buyer or seller number there.

Specific Instructions for Sellers

At the market, a good is traded, and each seller sells the same good. A seller can sell this good to any buyer, and a buyer can buy it from any seller. The market is organized in the following way: we open the market for a trading period (a "trading day"), and each trading day lasts three minutes. Every buyer can offer a price that will be relayed to us by telephone.[16] We list these offers on the blackboard, and you can accept one of these offers. If, e.g., a price of 50 is offered and you as seller number 5 want to accept this offer, you just say: "Number 5 sells for 50." In this case, the transaction is concluded. The good is sold to the buyer who made the offer of 50. The buyer will not know your identity. He will just know that his offer is accepted. You have to note your accepted price on sheet 2.

You can sell one unit of the good on each trading day. Therefore, the trading day ends for you after the acceptance of an offer. Note also that each buyer can buy, at most, one unit of the good per trading day. Each seller may accept an offer or not, but the sellers cannot make counteroffers. After three minutes the trading day ends, and the second stage of the experiment is conducted. After this, a new trading day is opened. In total there will be twelve trading days. At the second stage of the experiment, you can fix the value the good will have for the buyers. Buyers receive a certain amount of experimental money (reselling price) from us for each unit that they have bought. This reselling price is noted in the middle of sheet 2.

The profit of a buyer (measured in experimental money) is the difference between the reselling price and the price at which he has bought the good from you. If "your" buyer has bought the good for 205 and the reselling price is 405, he makes a profit of 405–205 = 200 (measured in experimental money). How much one unit of experimental money is worth for "your" buyer depends on you. By the choice of a conversion rate, you decide how much real money "your" buyer gets from us for one unit of experimental money. If

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16. In this context "us" meant that a supervisor in the buyers' room gave the price to a supervisor in the sellers' room who publicly announced the price. The second supervisor in the sellers' room then wrote this price on the blackboard.

you choose, e.g., the rate 0.5, your buyer gets AS 100 for 200 units of experimental money. Which conversion rates you are allowed to choose, is noted on the lower part of sheet 2. You have to write down your decision on the upper part of sheet 2. Do not announce your decision publicly.

You, as a seller, have two kinds of costs: production costs and "decision costs." The latter are associated with your decision about the conversion rate. Of course, you incur costs only in case of a deal. If you do not trade on a certain day, your costs are zero for this day. Production costs are noted on the upper part of sheet 2. Decision costs depend on your choice of the conversion rate. The higher the conversion rate you decide to give to "your" buyer, the greater are your decision costs. The costs, which are associated with the conversion rate, are noted on the lower part of sheet 2.

Your profit paid in AS is given by the formula: profit = price − production costs − decision costs. If, for example, you sell your good for 175, while your production costs are 100, and you choose a conversion rate of 0.6 which leads to decision costs of 5, your profit is given by 175 − 100 − 5 = 70. Do you have any questions?

Sheet 2 (for sellers)

Seller number:
Trading day:

conversion rate	
price (1)	
production costs (2)	26
decision costs (3)	
profit (4) = (1) − (2) − (3)	

reselling price of the buyers: 126
profit of the buyer = (reselling price − price) · conversion rate

Feasible conversion rates (CR) and associated decision costs (DC)

CR	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0
DC	0	1	2	4	6	8	10	12	15	18

Specific Instructions for Buyers

At the market, a good is traded, and each seller sells the same good. A seller can sell this good to any buyer, and a buyer can buy it from every seller. The market is organized in the following way: we open the market for a trading period (a "trading day"), and each trading day lasts three minutes. As a buyer you can offer a price that must be divisible by 5, for example, prices like 15, 60, 80, 275 are allowed, but prices like 48, 67, 124, 83 are not. These offers will be announced to the sellers by us over the telephone. The sellers will not know your identity, that is, your buyer number; they will only know the price offered. If a seller accepts your offer, all buyers are informed about this acceptance. In this case, an agreement is concluded, and the good is bought by you at the offered price. During each trading day you can buy one unit of the good. Therefore, a trading day ends for you when your offer is accepted. Note also that each seller can sell one unit of the good per day at most. If your offer is not accepted, you are free to change your offer, that is, to make a new offer. But the new price you offer must be higher than all the prices that have not been accepted. Each seller may accept an offer or not, but he cannot make a counteroffer.

After three minutes the day ends, and you cannot buy any more of a good. Then the second stage of the experiment will be conducted. After this, a new trading day is opened. On the whole, there will be twelve trading days. In the second stage of the experiment, the seller who has sold the good to you on this day can fix the value that the good will have for you. You as a buyer get a certain amount of experimental money (reselling price) from us for each unit you have bought. This reselling price is noted in the upper part of sheet 2. Your profit (measured in experimental money) is the difference between the reselling price and the price at which you have bought the good. If you bought the good for 205 and the reselling price is 405, you make a profit of $405 - 205 = 200$ (measured in experimental money). How much one unit of experimental money is worth to you depends on "your" seller. By the choice of a conversion rate, he decides how much real money you receive from us for one unit of experimental money. Which conversion rates he is allowed to choose are noted on the lower part of sheet 2. If he chooses, for example, the rate 0.5, you will get AS 100 for 200 units of experimental money.

Sellers have two kinds of costs: production costs and decision costs. The latter are associated with the decision about the conversion rate. Production costs are noted in the middle of sheet

2, and decision costs on the lower part of sheet 2. As you can see from sheet 2, the higher the conversion rate "your" seller chooses, the greater are his decision costs. The profit of the sellers paid in AS is given by the formula: profit = (price − production costs − decision costs). Suppose, for example, that you have bought the good for 175. The production costs of the seller are 100, and he chooses a conversion rate of 0.6 (which is associated with decision costs of 5), the profits of "your" seller are given by 175 − 100 − 5 = 70 AS. Do you have any questions?

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