Do Market Conditions Affect Preferences? Evidence from Experimental Markets with Excess Supply and Excess Demand

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June, 2000

Keywords: Experiment, Unbalanced Markets, Cooperation, Gift-exchange

JEL Classifications: A13, C91,C92, D43, J41

This paper is part of the EU-TMR Research Network ENDEAR (FMRX-CT98-0238). The authors thank Isabel Busom, David Cooper, Philippe Polomé, Jim Warnicke, and Shmuel Zamir for helpful comments, Brit Grosskopf and Carles Solà for their help in running the experiments and David Rodríguez for very able research assistance. Financial support from the Spanish DGCICYT (PB93-0679, PB94-0663-C03-01 and PB98-0465) is gratefully acknowledged. Charness also gratefully acknowledges support from the MacArthur Foundation.

Abstract: We study whether people's preferences in an unbalanced market are affected by whether they are on the excess supply side or the excess demand side of the market. Our analysis is based on the comparison of behavior between two types of experimental gift exchange markets, which vary only with respect to whether first or second movers are on the long side of the market. The direction of market imbalance could influence subjects' motivation, as second movers, *workers*, might react differently to favorable actions by first movers, *firms*, in the two cases. Our data show strong deviations from the standard game-theoretic prediction. However, we only find secondary treatment effects. First movers are not more generous when they are in excess supply and second movers do not respond less favorably when they are in excess demand. Competition has only minor psychological effects in our data.

1. INTRODUCTION

Gift exchange markets, in the Akerlof (1982) sense, have been employed as experimental representations of labor markets with variable effort and of goods markets with variable quality. Issues related to cooperative behavior play a prominent role in this form of market. The analysis of these markets, first studied experimentally by Fehr, Kirchsteiger and Riedl (1993), has shown that behavior usually deviates substantially from simple own-payoff maximization. Yet some of the motivational underpinnings of the remarkable behavior observed in these experiments has not been fully explored.

Our aim in this paper is to study whether market conditions affect people's preferences. More specifically, we investigate to what extent the state of competition, as defined below, affects the patterns of gift exchange. By the state of competition we refer to the relationship between the number of firms and the number of workers; in the gift exchange markets we study, this relation determines the degree of excess supply or demand for labor. We believe that the psychology of competition is an important economic issue. If the state of competition had a significant effect on motivation, this result would affect the very basis of how economists think about markets, since it would show that a specific feature of the economic environment has an effect on people's preferences.

One can describe the basic sequence of events in an experimental gift exchange market in the following manner: There are two types of agents (*firms* and *workers*) participating in the market, and the number of firms may or may not be equal to the number of workers. First, firms make wage offers in a one-sided auction and workers have the opportunity of accepting them; in

the standard case, workers cannot make counter-offers.[1] After a worker has accepted a firm's offer, the two parties become matched and the wage (and so the worker's base income) cannot be changed. A firm can only be matched with one worker and *vice versa*. Workers then choose effort levels, where there is no requirement that effort be greater than the lowest possible level. Higher wages yield lower monetary payoffs for firms and higher ones for workers, while higher effort levels have the reverse effect on payoffs. It is customary to conduct experimental sessions with multiple periods of the gift-exchange market just described. However, no identification of the other person in a match is possible, so that each interaction is considered to be a separate event.

The standard game-theoretic prediction is that workers will invariably choose minimum effort, since this choice is dominant in a pecuniary sense; in anticipation of this, firms will only make the lowest possible wage offer. Evidence from both our study and others, however, shows substantial deviations from the equilibrium prediction.[2] What drives the observed behavior? One possibility is that behavior is induced purely by concerns about the distribution of outcome payoffs. It could, however, also be affected by other circumstances; when evaluating a situation, people may not only be motivated by the payoffs at the outcome but also by a variety of other factors surrounding the act of choice. The focus of our study is very much related to the more general theme that preferences may depend not only on the outcomes that follow from certain choices, but also on information concerning the *process* leading to these outcomes. Such non-outcome information may matter because it offers inferences about the intentions or disposition behind the actions of others.

[1] Falk and Fehr (1999) study the case where workers can make counter-offers.

Our investigation in this paper is part of our more general interest in a broader theme: the experimental analysis of the effects of non-outcome information on behavior. Current research in the area is directed toward delineating more specific features of this behavior. It is at this point clear that in many situations people are influenced by others' payoffs at the outcome. However, it remains to be seen to what degree explanations of observed behavior need consider non-outcome information pertaining to the circumstances surrounding the act of choice. Note that it would be difficult to carry out this kind of analysis on the basis of field data alone, since in natural environments it will be unusual to find data with the desired variations in the non-outcome information. By contrast, experiments make it possible to generate this kind of evidence in a systematic manner.

Our results suggest that gift exchange behavior is not substantially affected by market imbalance. We do not find significant differences in wages or effort levels chosen with differing directions of market imbalance, although we do find some secondary effects across treatments. Motivation appears to be largely independent of this one specific feature of market participation. In our final section we present a discussion of the manner in which our results mesh with other findings concerning the effects of non-outcome information on choice.

2. BACKGROUND AND MOTIVATION

Why should the existence of unequal numbers of firms and payoffs influence subjects' motivation in this context? The conjecture that motivates our study is the possibility that workers

[2] Other recent evidence of this includes Fehr, Gächter and Kirchsteiger (1997), Fehr, Kirchsteiger and Riedl (1998) and also Hannan, Kagel and Moser (1999).

perceive a higher wage to be the result of different motivations on the firms' side: inspired by "generosity" (when there is an excess of workers) or "competitive pressures" (when there is an excess of firms). To the extent that perceptions of employer motivations vary depending on the nature of market imbalance, we might observe differential effort choices.

Information about the state of competition may be useful for inferring others' intentions because it pertains to the opportunities that others' have in the market. In his review of the connections between psychology and economics, Rabin (1998) discusses the relationship between opportunities and the attribution of intentions. He states that: "When motivated by reciprocal altruism, for instance, people differentiate between those who take a generous action by choice and those who are forced to do so." Whether people are "forced to be generous" may depend on the situation in which they find themselves.

Bowles (1998) presents a detailed survey of the literature on the different ways in which institutions may affect values, tastes and personalities. One of the several issues he discusses is closely related to the effect of market imbalance on the motivation of market participants. He states: "...or to take another example, there are significant differences in the personality effects on participants in markets which clear in equilibrium and those which do not, and in those markets which do not clear, for people on the short side of the market (whose advantageous positions may allow them to make take it or leave it offers) and those on the long side of the market, some of whom are simply excluded from the exchange process, while others fear losing the transactions they have secured."[3]

[3] For a more general discussion of the effects of participation in markets on preferences see also Lane (1991).

The possible influence of non-outcome information on choices has recently been introduced into a number of theoretical models of motivation. Sen (1997) provides a general discussion of the influence that the act of choice may have on behavior and suggests that relevant factors can be classified as either *chooser dependence* or *menu dependence*.[4] Differences in characteristics of decision-makers reflect chooser dependence, while the possible impact of foregone opportunities (or of social information) relates to menu dependence. Any effect on behavior from the type of market imbalance may be seen as a form of menu dependence.

Models of interdependent preferences differ with respect to whether motivation is affected by non-outcome information. Rabin (1993) presents models of reciprocal altruism, in which beliefs about intentions can affect behavior in two-person normal-form games. In contrast, Bolton and Ockenfels (2000) and Fehr and Schmidt (1999) propose models in which individual motivation is increasing in one's financial reward and decreasing in disparities among payoffs, but does not depend on other circumstances. Charness and Rabin (1999) respond by offering experimental evidence on the effects of forgone alternatives on choice, as well as a model of "quasi-maximin" social preferences (a combination of utilitarian and Rawlsian preferences). In this model, one's willingness to sacrifice to allocate the quasi-maximin allocation to someone depends on the extent to which that person is believed to be acting in consonance with this social ideal.

Previous experimental studies have looked at non-outcome information of different types. One line of work has investigated whether the decision made at a given choice set is influenced by the nature of the process that led to this choice set. For example, several papers vary whether a

[4] Sen's classification is a useful organizing tool, although it may not easily cover all ways in which non-outcome information may affect behavior.

self-interested party or some external mechanism determines the choices available to the subsequent player. This relates to Sen's concept of chooser dependence. Blount (1995) and Offerman (1998) find evidence that behavior in sequential games is affected by the process leading to the available alternatives. Charness (1996a) is perhaps the most closely related to our work here, since it is based on experiments with gift exchange games. He finds that what he refers to as attribution of volition has a significant effect on behavior when wages are relatively low.

Another type of non-outcome information that may matter is the nature of any foregone opportunities. For instance, in sequential games people may evaluate the intentions behind others' previous moves by taking into consideration the outcomes of alternative courses of action that other players could have taken but didn't. Bolton, Brandts and Ockenfels (1998) present some results from simple sequential dilemma games and find that forgone opportunities do not affect behavior significantly. On the other hand, Brandts and Solà (1998) and Falk, Fehr, and Fischbacher (1999) study behavior in games akin to the ultimatum mini-game and find definite evidence that the likelihood of an offer being rejected is affected by the options that were not exercised. In these studies, it is the attribution of negative intentions to the proposer that may have an impact on responders' behavior.[5]

Cason and Mui (1998) investigate the influence of information about the behavior of others in the same context on an individual's behavior. In their data, social information does not have strong effects on behavior. Brandts and Charness (1999) analyze whether subjects' evaluation of a given outcome is influenced by whether that outcome was reached after a truthful

or an untruthful statement by another subject; the results indicate that subjects react differently to the two types of statements.[6]

3. EXPERIMENTAL DESIGN AND PROCEDURES

To disentangle the possible effects of market imbalance from other potential influences we need an appropriate experimental design. This section motivates and presents the design and the procedures that we used in our experiment.

3.1. A simple dilemma game in a market environment

Gift-exchange games can be envisioned as two-player dilemma games that are played in a sequential fashion. A dilemma game is characterized by the following features: all players have a dominant strategy and certain joint deviations from dominant strategy play lead to both players receiving a higher payoff than if both play their dominant strategy. We can describe sequential play of a dilemma game in terms of gift exchange. In the beginning, the first player chooses a certain gift or contribution level. After seeing this, the second player decides the degree to which he returns the gift.[7] These games have a unique subgame-perfect Nash equilibrium where both players choose the minimum gift level.

Embedding a game of this type in a market environment with competition, as in previous experimental work, does not alter the straightforward prediction of game-theoretic analysis. As will be explained below, in an unbalanced market context some agents will not be matched, but

[5] The evidence about contributions in dilemma games presented in Bolton, Brandts and Ockenfels (1998) is clearly inconsistent with the reward of good intentions. In contrast, the punishment of bad intentions may explain a portion, albeit a rather small one, of observed behavior.

[6] Note that the first mover is a self-interested party in all of the studies referred to in these last two paragraphs.

this does not affect the pecuniary incentive structure of subjects given that they have been matched.

In our experiments, we use the following simple symmetric and linear payoff functions:

$$FI = 10 - w + 5e \qquad (1)$$

$$WI = 10 - e + 5w, \qquad (2)$$

where FI and WI refer (respectively) to firm income and worker income, w denotes the wage and e the effort level. The range of possible wage and effort levels is restricted to integers between 0 and 10, inclusive.[8] Each unit of income was worth 5 pesetas ($1≅ 150 pesetas, at that time).

The symmetry and the linearity of the payoff structure are the two crucial features of our design. As stated above our objective is to study in which way subjects' behavior is affected by varying exclusively the number of participants on the two sides of the market, as well as the ratio between them. The symmetry of the payoff functions is necessary to ensure that the impact of our treatment variable can be studied in isolation.[9] It implies that, apart from issues of market imbalance, the only difference between the incentives of the two players is caused by the fact that one of them chooses first and the other chooses second. It also makes it possible to think of a situation with *n* firms and *m* workers as symmetric to the case of *m* firms and *n* workers.

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[7] In the context of a public good game these gifts can be seen as contribution levels.

[8] This payoff function is a slight modification of the standard linear public good payoff function, which for the two-player case can be written as: $I_i=(E_i-C_i) + p(C_i+C_j)$ i different from j, where I_i is individual i's income, E_i is i's endowment C_i and C_j are the contributions and $p < 1$ is the marginal per capita return. The only difference from the standard case is that here the payoff a player obtains from his own contribution to the public good is different than the payoff he gets as a result of the other's contribution, i.e. p_i is different from p_j and $p_I < 1$.

[9] An asymmetric representation could be easily introduced in a subsequent experiments.

The linearity of our payoff function helps isolate the impact of the market imbalance on behavior by making the marginal effect of effort independent of the wage. If we instead had a payoff structure in which the marginal effect of the effort level diminished with the wage level there would be a possible confounding factor. Lower effort/wage ratios at higher wages might then be the result of either a motivational effect or a diminishing transformation rate.[10]

Another important feature of our design is that the information available to participants was the same in both treatments. All wage offers were public information both for firms and for workers, while the effort supplied in a particular match was only known to the two parties in the match.[11]

It is easy to verify that with these features the standard subgame-perfect equilibrium prediction does not depend on whether there are more firms or more workers in the market. In the second stage workers have no financial incentive to exert any effort. Given this expected behavior, the subgame-perfect equilibrium notion predicts that firms offer the lowest possible wage or do not make any offer. As a consequence, all agents obtain a payoff of 10, independently of their type and of the existence and type of market imbalance, i.e. the situation that arises in the case in which there is no gift exchange at all is not favorable to either side of the market.

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[10] Here the wage is not a pure one-to-one transfer, unlike the payoff design in Fehr, Kirchsteiger and Riedl (1993) and its successors. For our purposes, however, the crucial feature of the gift exchange game, from a conceptual point of view, is the sequential structure of the game and the fact that joint deviations can lead to common gains. Since we wished to maintain these two features and, at the same time, introduce symmetry, it was not possible to keep the one-to-one transfer aspect of the payoff structure. One can think of our design as representing the case where gifts are more valuable to the recipient than to the donor.

[11] An antecedent of the work we present here is Fehr, Kirchler, Weichbold and Gächter (1998). They compare behavior in gift exchange markets with excess supply to behavior in a bilateral gift exchange condition. However, they use an asymmetric non-linear payoff function in both treatments and information about others' wage offers is different across the two treatments. Given these features, their data can not be used for our purposes.

3.2. The conduct of our experimental sessions

We conducted a total of eight experimental sessions, four with the *excess supply of labor* (hereafter, ESL) treatment and four with the *excess supply of firms* (hereafter, ESF) treatment. There were twenty participants in each of the sessions. In the ESF sessions 8 subjects had the role of *employees* (workers) and 12 had the role of the *employers* (firms), while in the ESL treatment there were 12 employees and 8 employers. The experimental sessions took place in Barcelona between June and October 1998, at the Universitat Pompeu Fabra. Subjects were recruited using announcements in university buildings.

At the beginning of each experimental session all the participants were gathered in a room and the instructions were read to them, while they read along.[12] During this time subjects could ask public questions about the procedures. Then subjects were randomly assigned to one of the roles and employers and employees were seated in different rooms. Each period consisted of two stages: Stage 1 of each period consisted in a one-sided oral auction following Fehr, Kirchsteiger and Riedl (1993). Employers made wage offers and these offers were written on the blackboards of both rooms.[13] Firms that had not made a wage offer received a payoff of 10; this gave them the same payoff than if they had made a wage offer of zero and had then been matched with a worker who chose a zero effort level.

To accept an offer an employee had to raise his hand and state which of the outstanding offers he accepted. In Stage 2, each employee wrote his effort level on his record sheet. This

[12] The appendix contains a copy of the instructions. With the exception of the payoff function they follow quite closely those of Fehr, Kirchsteiger and Riedl (1993).

information was then communicated exclusively to the corresponding employer. We excluded the possibility of workers rejecting wage offers. Our trading rules specified that, after the wage-offer stage of a period was over, workers who had not accepted a wage would be randomly assigned to the firms whose offers were still outstanding. In an analogous way, our rules stipulated that a firm that had not made a wage offer would be randomly assigned to outstanding workers at a wage of 0. We believe that these rules add to the desired symmetry of our design. At any rate, in our sessions it was actually never necessary to assign subjects randomly according to the rules just described.

There were ten *market periods* in each session.[14] At the end of the period all participants calculated their period-payoff. Subjects were paid privately at the end of the session; in addition to experimental earnings, each participant received 500 pesetas as a show-up fee.

In the experiments we used the labor market presentation of the situation. Although the frame may affect behavior, it should have no bearing on our results, since the frame was held constant across our two treatments.[15]

4. RESULTS

In a general sense, we are interested in identifying any type of treatment effect. As mentioned in the introduction, we do, however, have a specific conjecture about how the type of market imbalance may affect behavior: workers will tend to be more generous in ESL than in

[13] We used telephone technology to communicate the offers to the other room.

[14] We used a multi-period procedure to follow standard practice. While there are 10 periods in each session, the anonymity should eliminate direct reputation-building. However, a worker knows that she might be anonymously re-matched with a firm, so that dynamic considerations may be relevant. *A priori* it is not clear why these potential dynamic effects should be different across treatments.

ESF. A second conjecture about workers' behavior, based on previous evidence, is that in both treatments effort and wage levels will be positively related.

According to the standard view of economic behavior, effort levels will invariably be zero. This prediction represents the strong null hypothesis. In experiments, however, one has to allow for the presence of decision error. Although decision error should not be systematic, here the "error" can only go in one direction, as it is possible for effort levels to take on positive values, but not negative ones. Decision error can be conceptualized in more than one way. For example, one could presume that subjects just make random errors in their decisions. Alternatively, Anderson, Goeree and Holt (1998) posit that relatively costly mistakes are less likely. Given the linear structure of our payoff function, incorporating this second conception of errors into the standard prediction leads to the following (weaker) null hypothesis, composed of two elements:

H_0: (i) Effort levels are independent of wage levels.
(ii) Effort levels are the same under ESF than under ESL.

Costs of deviations are the same under ESF and under ESL; in neither treatment do these costs depend on the actions of others.

Our two alternative hypotheses can be formulated as follows:

H_{A1}: Effort levels are increasing in wage levels,

and

H_{A2}: Effort levels are higher under ESL than under ESF.

With respect to the second alternative hypothesis we will look at effort levels both overall and separately for different wage levels.

[15] For evidence of framing effects see Pillutla and Chen (1999).

Before we present our evaluation of the hypotheses pertaining to workers' effort choices, we present a brief analysis of firm behavior. As mentioned in section 3.1., it is clear that for the case of purely self-interested preferences there should be no treatment effects on firms' behavior. However, for the case in which preferences have a social component it is not clear how wages would be affected by the treatments. Under ESL firms' wage offers are the outcome of the interaction between firms' and workers' motivations. Under ESF, however, the effects of motivational factors are confounded by the presence of firms' incentives to enter into a match. For this reason we do not present any specific hypotheses about firm behavior. Our aim is to present a complete picture of behavior in our experimental markets; as will be seen below, observed firm behavior will facilitate the interpretation of worker behavior.

4.1. Analysis of firm behavior

Figure I presents the wage distribution separately for both treatments, aggregated over sessions and periods. In general terms, the differences between the two distributions are not striking. If we take the wages paid by firms in ESL (the treatment in which they are on the short side of the market) as the baseline, wages under ESF are not clearly higher. However, the higher incidence of zero wages and the somewhat lower incidence of wages equal to 10 in ESL, shown in figure I, might be a indication of a possibly lower average wages for that treatment. Another feature of the distributions for both treatments that we wish to highlight at this point is the high proportion of wages at the highest possible level for both treatments.

A different perspective on firms' behavior is given by Figure II, which shows the average wages over time for both treatments. As in Figure I, the differences between treatments appear to

be quite small throughout the periods. In this presentation one gets the impression that in the first part of the session average wages are higher for ESF than for ESL, while for the second part it is the other way around. Next we discuss whether the impressions one obtains from Figures I and II have any statistical validity.

The statistical analysis of data from gift exchange experiments like the ones we conducted is a delicate matter. Due to the interaction between subjects across periods we only have, in the strict sense, one statistically independent observation per session. In what follows we base our analysis mainly on statistically independent information. At some points we also report other types of tests, if we judge them to be informative.

Table 1 presents average wages for all eight sessions, both for complete sessions and for the first and second part of the sessions.[16] The results of the (one-tailed) permutation tests we performed show that the differences in average wages between the two treatments are not significant at anything close to conventional levels for the three ways of organizing the data.[17] While it could be argued that a high degree of variation across the individual sessions might be swamping any treatment effect, we point out that a significance level of .014 can be attained with only eight observations, even with the less powerful Wilcoxon rank-sum test.

The information contained in Table 2 is aimed at examining the statistical significance of the disparity (observed in Figure I) between the frequency of offers at the lowest and highest wage. It presents the proportions of wages equal to zero as well as the proportion of wages equal to ten for all eight sessions, both for complete sessions and for the first and second part of the sessions.

[16] The overall average wage is 7.45 for ESL and 7.35 for ESF.

Using the permutation test we again fail to find differences between these two specific features of the wage distribution.

In summary, the first-movers of our experimental markets do not appear to be affected by whether they are in excess supply or in excess demand.

4.2 Analysis of worker behavior

The evidence presented until now shows that there is a very strong tendency for gift-giving in both treatments. It remains to be seen to what extent these gifts are returned. Figure III presents average effort for the different feasible wage levels for both treatments, aggregated over all four sessions of the respective treatments.[18] Wages and effort levels appear to be positively related; we will refer to this pattern of behavior as *reciprocal actions*.[19] To provide some statistical validity for reciprocal actions we used the Page test on the basis of session level data. For each session we computed the mean effort level for four wage ranges: 0 to 5, 6 to 8, 9 and 10.[20] For both treatments separately, we can reject the null hypothesis of no relation between wage and effort levels in favor of the alternative of an increasing relation at the 1% level.[21] In accordance with previous results we can reject portion (b) of the null hypothesis in favor of H_{A2}.

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[17] In contrast to the Wilcoxon rank-sum test, the permutation test also takes into account the differences between the data for the two treatments. For a discussion of the use of the permutation test in experimental economics see Davis and Holt (1993).

[18] A wage level of two was never observed under ESF.

[19] Note that this is not necessarily reciprocity in the sense of the rewarding of favorable actions. Outcome-based models predict that an employee would make the same effort choice if a random process had chosen the same wage for the employee.

[20] At the session level we do not always have observations for each wage level. For this reason, we group the data into wage ranges.

[21] For a reference to the Page test see Siegel and Castellan (1988). It tests the hypothesis that k matched groups are the same versus the alternative hypothesis that the groups are ordered in a specific sequence.

We also computed the Spearman rank correlation coefficient for both treatments using each match as a data point. For ESL the value of the coefficient is .475, and for ESF it is .503. Each coefficient is based on 320 observations and is significant at p=.001. We also computed individual correlation coefficients: 2/3 of these were larger than .45 and significantly different from zero, at least at the 10% level. Another 15% were larger than .25, although not statistically significant. The relation at the session level is, hence, the reflection of broad-based use of reciprocal actions at the individual level. Note, however, that the tests of the rank correlation coefficients are based on the questionable assumption of the independence of observations.

With respect to treatment effects, at first sight we do not observe generally higher effort levels for ESF. However, simple inspection suggests the presence of two possible non-anticipated effects. First, the largest differences correspond to the intermediate wages, 5 to 8. Second, the rate at which effort levels increase with wages appears to be larger for ESF than for ESL. We return to these issues below, after analyzing the evidence related to our main hypotheses.

The observed pattern points to the existence of some type of interdependent preferences.[22] However, the information shown in Figure III does not directly reveal to what extent the deviations from the standard prediction made both sides of the market better off, a kind of situation we will refer to as *cooperative gains*. It is possible that effort levels were not high enough to compensate firms for offering positive wages. Data that exhibited a pattern of reciprocal

[22] Note that, given the linearity of the payoff structure presented in section 2, the behavior we observe can not be accommodated by a formulation based on linear altruism and/or warm glow, since these motivations do not generate the interdependence of actions that we observe.

actions without cooperative gains would not be easy to interpret, since firms would be earning less than at the zero wage level.

Figure IV shows average firm income FI and average worker income WI per wage level for both treatments. Given that the income is 10 at wage = 0, it can be seen directly that, for both treatments, there are increasing cooperative gains over a range of values of the wage. In addition, it is true for both treatments that those firms that offer the highest wage obtain the highest firm income. If we combine this fact with the very high frequency of the highest wage, it is clear that in our game subjects are able to obtain considerable cooperative gains. Workers, who move second, obtain a considerably larger share in every instance; worker income is actually very similar across treatments for all wage levels.[23]

As in Figure III there appear to be some differences between the two treatments. However, from a number of viewpoints these differences don't appear to be large. The proportion of wage offers that obtain cooperative gains is 87% for ESL and 92% for ESF. Focusing on the highest possible cooperative gain, it turns out that in the ESL (ESF) treatment 49% (60%) of the wage offers correspond to a wage equal to 10; if we include wages of 9 then the percentages jump to 60% (67%). There are several ways of looking at the attained efficiency level.[24] One can, for instance, look at efficiency gains at a wage of ten: they are 77% for ESL and 80% for ESF. Another measure is given by the efficiency gains, averaged over all matches: they are 53% for ESL and 64% for ESF.

[23] Given the symmetry of our design, equality of wage and effort yields a simple benchmark for evaluating the degree to which the second movers take a larger share for themselves. If wage and effort are equal to each other, then for a level of 1 both sides earn 14. Increasing wage and effort by 1 leads to a gain of 4 for both sides. For maximum wage and effort both sides earn 50.

It is possible that treatment effects show up not so much in average behavior but rather in the evolution of reciprocal behavior over time. Figure V shows the behavior over time of the average effort level over the 10 experimental periods for each of the treatments. We do observe "decay" for both treatments. While this is in contrast to some earlier experimental results involving gift-exchange games, public goods experiments provides instances both of decay, as in Isaac and Walker (1998), and of no decay, as in Saijo and Nakamura (1995). Note, however, that for both treatments the average firm income in period 10 is still larger than the equilibrium prediction - 13.06 for ESL and 13.73 for ESF. The data presentation in Figure V suggests that, in earlier periods, both the average wage and the average effort level are higher in ESF than in ESL.

Some of the impressions suggested by Figures III, IV and V can now be verified on the basis of the information shown in Table 3. It presents average effort levels per session, both for complete sessions and for the first five and last five periods of each session. As for the average wage levels shown in Tables 1 and 2, the permutation test does not find any significant differences between treatments. Table 4 contains the data for three additional session indicators, the Spearman rank correlation coefficient, the Tobit regression slope coefficient and the average level of firm income. We feel that this last indicator is a very natural one to use, since it directly captures the consequences of possible treatment effect for firms' payoffs. The results shown in table 4 show that the treatment differences for the new set of indicators are again not statistically significant.[25]

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[24] Given the baseline earnings of 10 for both firms and workers and the maximum joint income of 100, the efficiency gains can be computed as total income in excess of 20 divided by 80, the maximum efficiency gain.

[25] Here we are using the Spearman rank correlation and the Tobit regression coefficients simply as session summary statistics and, hence, don't use the assumption that observations within a session are statistically independent from each other. For a similar use of Tobit coefficients as summary statistics see Sadiraj and Schram (1999). For both indicators we

Using the more familiar Wilcoxon test we can get a better feel of the strength our results. None of our conclusions about statistical significance would change. More importantly, the rankings for the ESF and the ESL treatments are so similar that we would need the data from at least 5 more sessions to line up perfectly for the Wilcoxon test to confer significance at the 5% level. This is true for any of the session level tests in Tables 1-4.

At this point we have finished the evaluation of our main hypotheses. We can easily reject part (i) of the null hypothesis in favor of the alternative, but cannot reject part (ii). We now turn to the analysis of the two non-anticipated features of our data mentioned above. As mentioned above, Figures III and IV suggest that the differences in behavior are smaller for the more extreme values of the wage and larger for intermediate values. It could be that a very high wage always seems generous to a worker and a very low wage always seems ungenerous, whatever the supply/demand imbalance. This would obstruct any treatment effect at the wage extremes. On the other hand, an intermediate wage level might be more open to interpretation, and we might then expect treatment effects to be more likely to manifest.

Table 5 shows average effort levels per session, separately for low, middle and high levels of wages. We again do not find any significant differences between the two treatments; even from this more differentiated perspective we can not reject part (iii) of our null hypothesis. It is, nevertheless, true that the difference is largest for intermediate levels, in agreement with the notion suggested in the previous paragraph.[26]

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encountered the difficulty that in session ESL-4 all accepted wages were equal to 10 and so we could not compute the statistics for this session.

[26] However, note from Figure I that intermediate wage offers are only a modest fraction of all wage offers.

Data from experimental gift exchange markets have been often been analyzed using Tobit regressions. This procedure assumes that the data from different matches within a session are statistically independent and is, therefore, not fully appropriate for the analysis of our data. We have, nevertheless, run various two-sided censored Tobits to explore the apparent difference between treatments in the rate at which effort levels increase with wage levels. This is done through the use of treatment dummies.

Table 6 presents the results of these regressions. Due to the existence of some decay in our data we present results both 10 periods and for only the first 7 periods. Regressions 1 to 4 are separate for ESL and ESF and can be seen as baselines; their results are consistent with a positive relation between effort and wage levels, in accordance with the non-parametric test results presented above. Regressions 5 and 6 combine both the ESL and ESF data sets, and include an ESL treatment dummy on both the intercept and the slope coefficient: ESL*constant and ESL*wage. We see that these coefficients are both statistically significant (subject to the caveat about independent observations), and more so when all 10 periods are included.[27]

These last regressions express formally the impression one gets from the inspection of Figure III. In the ESF case, the wage-effort relation can be seen as having a more negative intercept and a higher positive slope than in the ESL case. This finding indicates that there may be some dimensions to the motivation behind gift exchange that cannot be fully explored with our design. However, it does not alter our conclusion with respect to the two hypotheses that motivated our study.

5. SUMMARY AND DISCUSSION

The main features of our results that we wish to highlight in this final section are the following. Our principal finding is that in our data the treatment effects are secondary, in that whether firms or workers are on the long or short side of the market generally does not have a major impact on their behavior. Firms do not pay higher wages when there are more firms than workers and workers do not exert more effort when there are more workers than firms. We have not been able to find any significant differences in wages and in effort levels, although we have inspected our data from a variety of viewpoints. All our indicators are far from showing a significant difference and it is, hence, very doubtful that data from additional sessions would alter our conclusions.

The fact that wages are constant across the two treatments facilitates the interpretation of workers' behavior in our experiments. It allows us to separate the pure effect of the state of competition from the indirect effect that market conditions could have through their impact on the wage distribution. For a worker that is matched with a firm that has offered a certain wage level, the only difference between the two treatments is the state of competition. Our data allow us to conclude that market conditions, as an isolated factor, do not affect effort levels significantly.

One reaction to our findings about treatment effects might be the feeling that the kind of emotions that might cause market imbalance to have an effect on behavior are naturally not present, and cannot be created, in the laboratory. While this may be a reasonable conjecture, it

[27] In their analysis of experimental gift exchange markets Fehr, Kirchler, Weichbold and Gächter (1998), Fehr, Kirchsteiger and Riedl (1993) and Fehr, Kirchsteiger and Riedl (1998) also use a linear relation between effort and wage levels for their

must be evaluated in the light of some other features of our results as well as of other relevant experimental evidence. As discussed in our results section, we find very considerable deviations from the standard prediction. Our results exhibit a clear pattern of reciprocal actions, as in previous work on gift exchange. We also find that subjects are able to attain considerable cooperative gains. These statements are valid for both our treatments. The absence of treatment effects is, hence, not due to the fact that the laboratory leads to a strong adherence to the standard game-theoretic prediction.

A second way in which our evidence can be put into perspective is by relating it to the cited evidence favoring the notion that non-outcome information influences behavior. A provisional assessment of this evidence points to two patterns: First, non-outcome information tends to be more relevant when it very directly points to others' personal responsibility, as in the cases analyzed by Charness (1996) and Brandts and Charness (1999). In the terminology of Sen (1997), chooser dependence may tend to be more important than menu dependence. Second, perhaps due to a form of self-serving bias, people may react more strongly to perceived negative intentions than to perceived positive intentions.[28] Charness and Rabin (1999) find so little positive reciprocity in their games that their model does not include it.

On the basis of our interpretation of this previous evidence, the modest treatment effects we found in this paper appear to make sense. The attribution of disposition on the basis of the type of market-balance can only be based on a rather indirect channel. Perhaps the effect of individual responsibility must be quite clear, as suggested by Charness (1996b). With competitive

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Tobit regressions. They also find positive values for the wage coefficient and negative values for the constant.

[28] See Offerman (1998).

bidding, the attribution of responsibility is muted, potentially explaining why the direction of market imbalance does not seem to be a strong force in our data. According to our results, appropriate models of interdependent preferences need not take into account the effects of market imbalance on motivation.

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Table 1

	Session ESL-1 June 9, 1998	Session ESL-2 June 18, 1998	Session ESL-3 July 14, 1998	Session ESL-4 October 8, 1998	Session ESF-1 June 10, 1998	Session ESF-2 June 19, 1998	Session ESF-3 July 16, 1998	Session ESF-4 October 6, 1998	Permutation test results
Average wage in first part of session	8.275	4.125	8.675	7.025	8.816	6.85	6.266	10	P=.257
Average wage in second part of session	9.376	5.325	8.25	8.15	6.309	5.622	5.566	10	P=.729
Average wage in whole session	8.825	4.925	8.462	7.588	7.567	6.242	6.917	9.658	P=.457

Table 2

	Session ESL-1 June 9, 1998	Session ESL-2 June 18, 1998	Session ESL-3 July 14, 1998	Session ESL-4 October 8, 1998	Session ESF-1 June 10, 1998	Session ESF-2 June 19, 1998	Session ESF-3 July 16, 1998	Session ESF-4 October 6, 1998	Permutation test results
Proportion of wage= 10 in first part of session.	.5	.175	.275	.475	.66	.25	.166	1	P=.243
Proportion of wage= 10 in second part of session.	.925	.3	.5	.75	.312	.383	.383	1	P=.657
Proportion of wage= 10 in whole session.	.712	.237	.387	.612	.492	.317	.275	1	P=.443
Proportion of wage= 0 in first part of session.	0	.275	0	.1	0	.033	.133	0	P=.643
Proportion of wage= 0 in second part of session.	.05	.375	.075	.15	.183	.3	.35	0	P=.343
Proportion of wage= 0 in whole session.	.025	.325	.037	.125	.092	.166	.242	0	P=.500

Table 3

	Session ESL-1 June 9, 1998	Session ESL-2 June 18, 1998	Session ESL-3 July 14, 1998	Session ESL-4 October 8, 1998	Session ESF-1 June 10, 1998	Session ESF-2 June 19, 1998	Session ESF-3 July 16, 1998	Session ESF-4 October 6, 1998	Permutation test Results
Avg. Effort in first part of session	6.225	1.3	5.175	3.425	6.15	3.2	2.925	9.25	P=.771
Avg. Effort in second part of session	4.9	1.8	3.750	4.2	2.325	1.525	2.3	7.625	P=.486
Avg. Effort in whole session	5.5625	1.55	4.463	3.813	4.238	2.3625	2.613	8.4375	P=.614

Table 4

	Session ESL-1 June 9, 1998	Session ESL-2 June 18, 1998	Session ESL-3 July 14, 1998	Session ESL-4 October 8, 1998	Session ESF-1 June 10, 1998	Session ESF-2 June 19, 1998	Session ESF-3 July 16, 1998	Session ESF-4 October 6, 1998	Permutation test results
Spearman rank correlation coefficient	.282	.530	.316	.566	.501	.274	.406	-	P=.400
Tobit slope coefficient	.821	.499	1.117	1.442	.983	.913	1.088	-	P=.543
Average FI	28.974	12.709	25.655	21.003	22.485	13.121	15.188	39.940	P=.557

Table 5

	Session ESL-1 June 9, 1998	Session ESL-2 June 18, 1998	Session ESL-3 July 14, 1998	Session ESL-4 October 8, 1998	Session ESF-1 June 10, 1998	Session ESF-2 June 19, 1998	Session ESF-3 July 16, 1998	Session ESF-4 October 6, 1998	Permutation test results
Avg. Effort for Wages 0 to 4	1.143	.222	0	.118	.222	0	0	-	P=.286
Avg. Effort for Wages 5 to 8	5.615	2.591	3.167	1.455	2.636	1.774	1.15	-	P=.143
Avg. Effort for Wages 9 and 10	6.607	2.682	5.172	5.518	5.653	3.190	3.72	8.4375	P=.314

Table 6

Regression #	1	2	3	4	5	6
Data set	ESL 10 per.	ESF 10 per.	ESL 7 per.	ESF 7 per.	Both 10 per.	Both 7 per.
Dependent variable	Effort	Effort	Effort	Effort	Effort	Effort
Constant	-4.46 (-4.79)	-9.94 (-7.97)	-4.10 (-3.96)	-8.80 (-6.35)	-9.63 (-8.2)	-8.58 (-6.52)
ESL*constant					4.94 (3.22)	4.32 (2.53)
Wage	1.03 (10.34)	1.64 (12.54)	1.04 (9.35	1.61 (10.97)	1.61 (13.1)	1.58 (11.4)
ESL*wage					-0.56 (-3.47)	-0.52 (2.86)

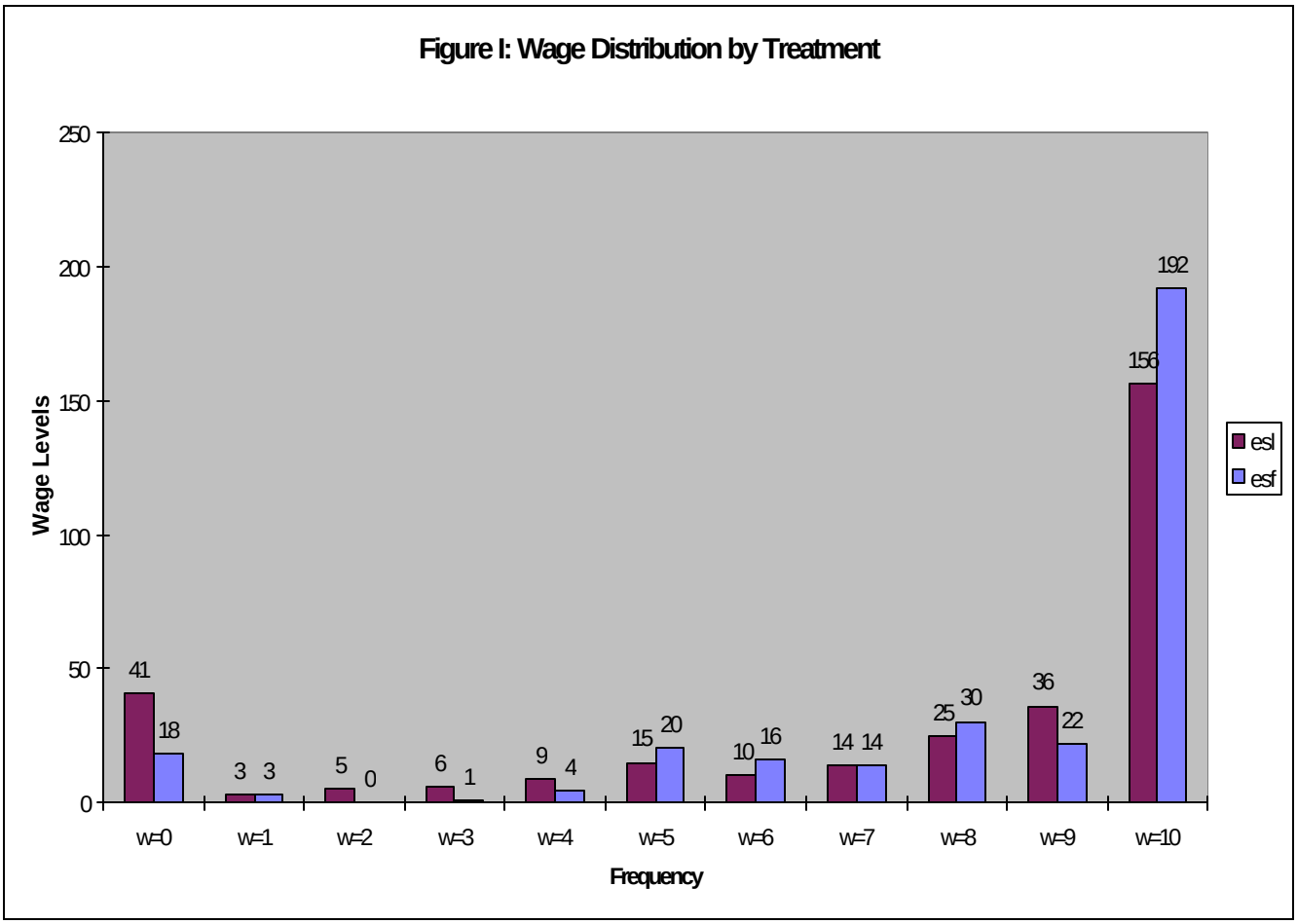


Figure I: Wage Distribution by Treatment
Wage Levels
250
200
150
100
50
0
41
18
3
3
5
0
6
1
9
4
15
20
10
16
14
14
25
30
36
22
156
192
w=0
w=1
w=2
w=3
w=4
w=5
w=6
w=7
w=8
w=9
w=10
Frequency
esl
esf

Figure II: Average Wages over Time

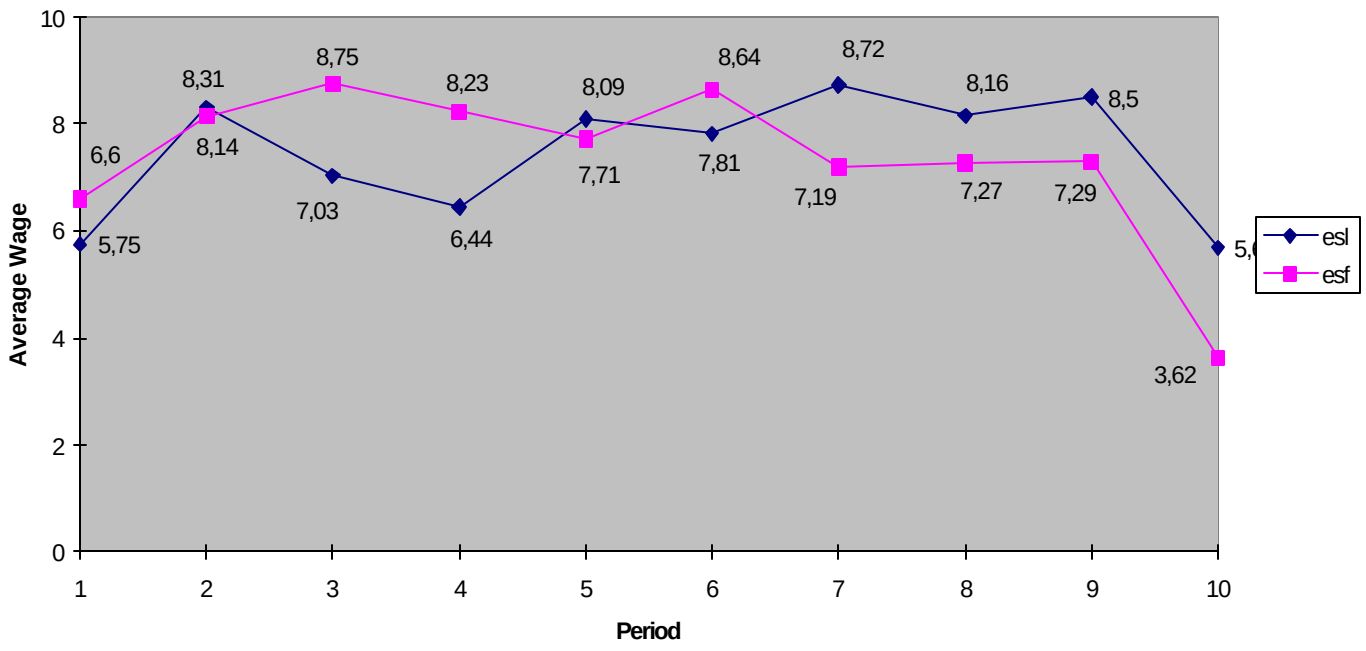


Figure III: Average Effort per Wage Level for both Treatments

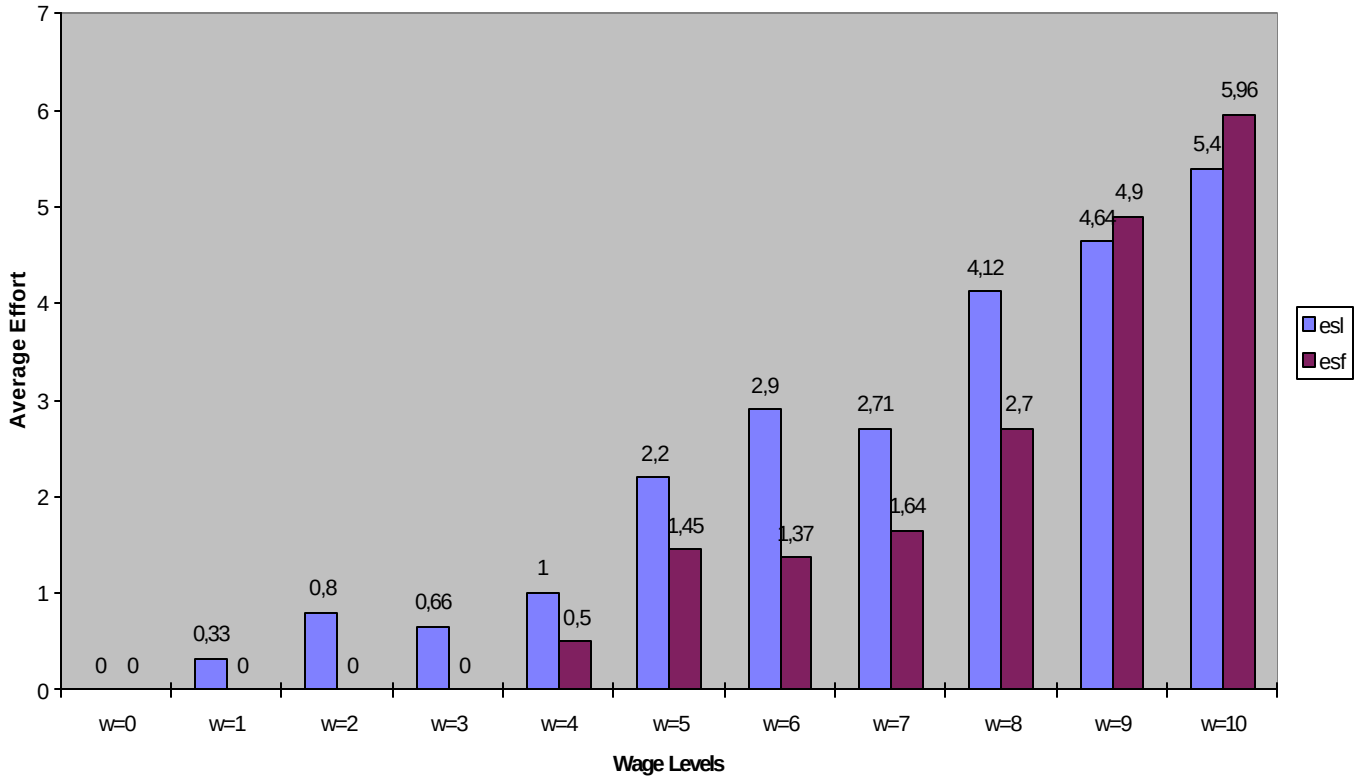
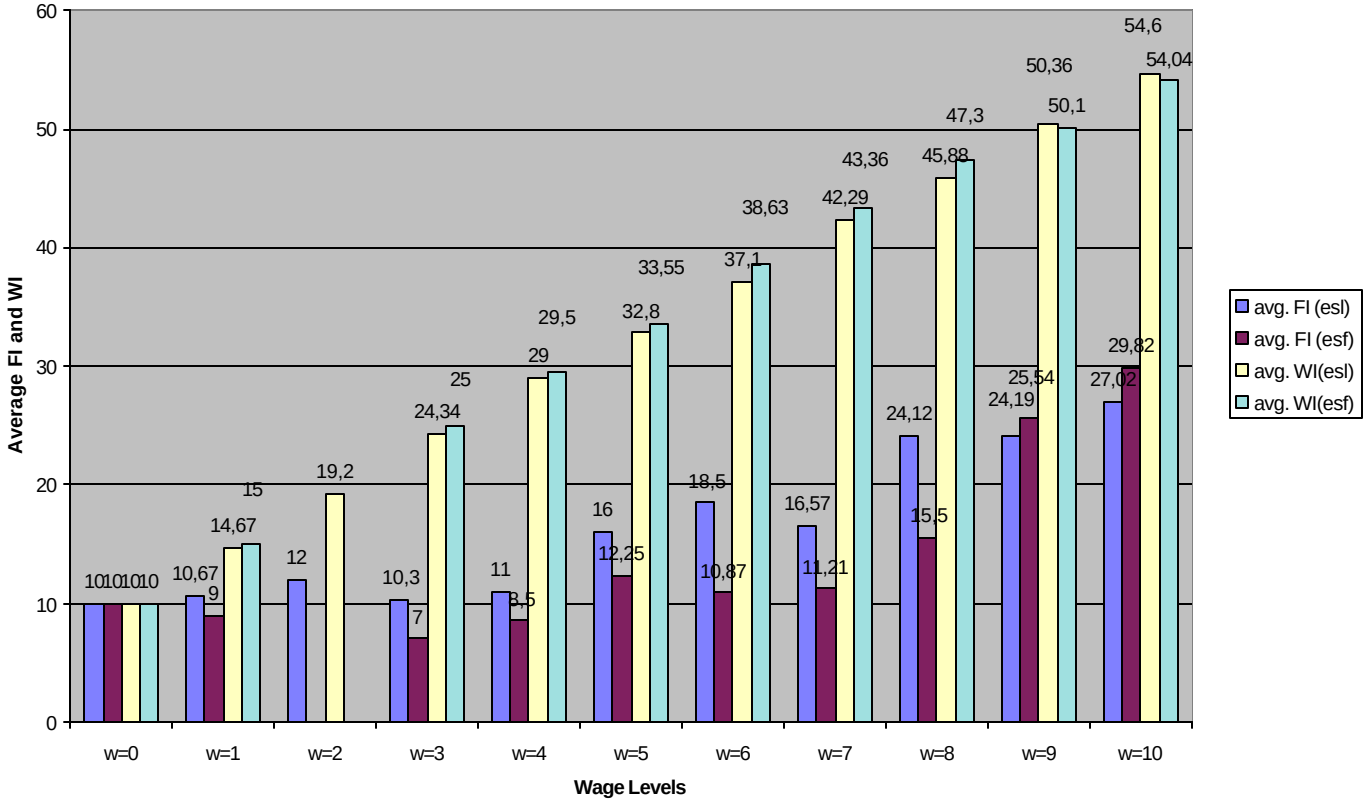


Figure IV: Average Firm and Worker Income per Wage Level; Both Treatments



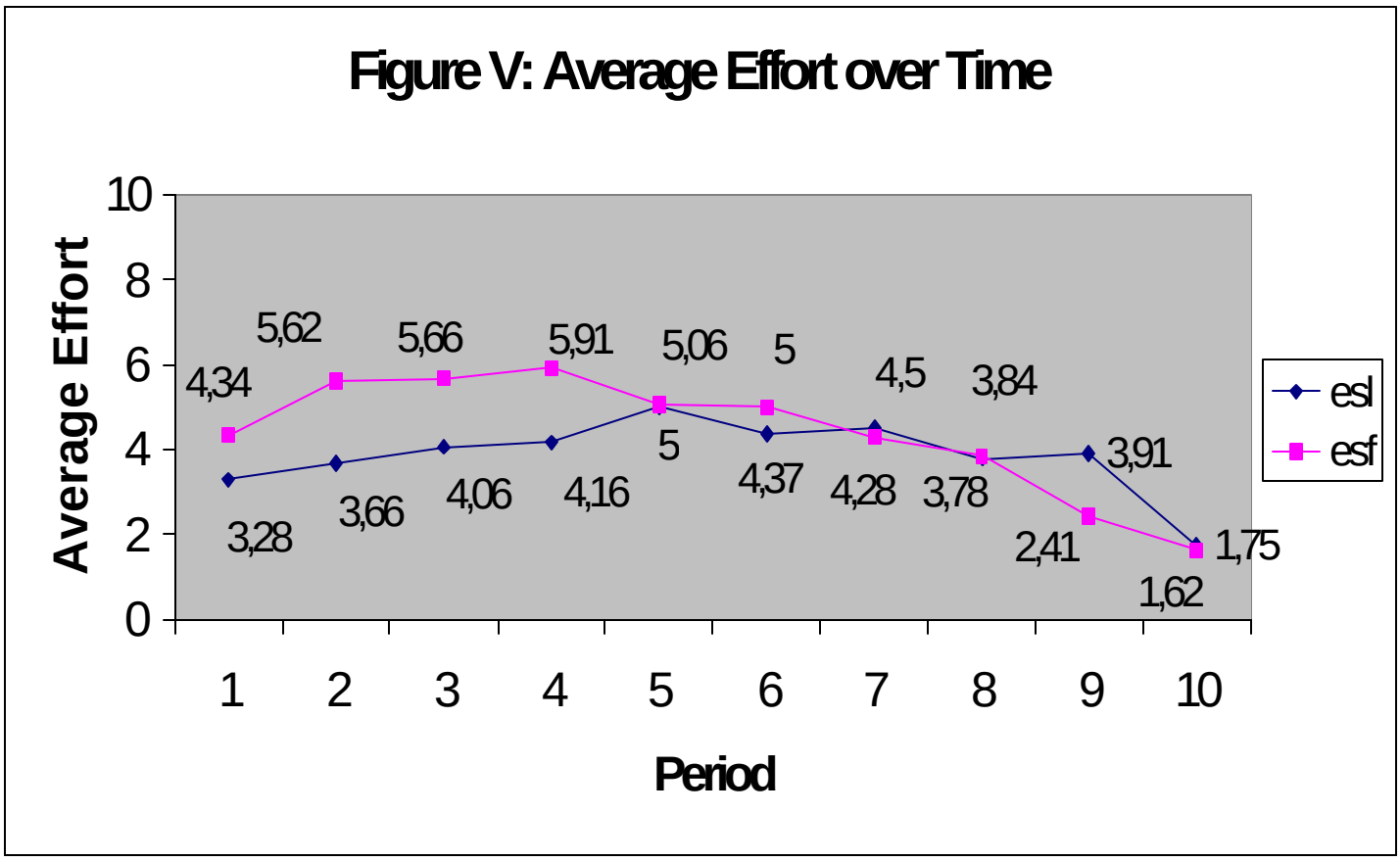


Figure V: Average Effort over Time
Average Effort
10
8
6
4
2
0
1
2
3
4
5
6
7
8
9
10
Period
esl
esf
4,34
5,62
5,66
5,91
5,06
5
4,5
3,84
3,91
1,75
3,28
3,66
4,06
4,16
5
4,37
4,28
3,78
2,41
1,62

APPENDIX

INSTRUCTIONS FOR AN ESF SESSION
(TRANSLATION FROM SPANISH)

(The first part of the instructions was read aloud while all the participants were in one room. The second and third part of the instructions was read separately to employers and employees in their corresponding rooms. In both rooms we went through the three exercices on the blackboard.)

1. GENERAL INFORMATION

You are about to participate in a study about the labor market. If you read these instructions carefully you may earn a considerable amount of money. During the experiment your earnings will be calculated in "PESOS". At the end of the experiment PESOS will be converted into pesetas at the rate of:

1 PESO = 5 PESETAS

In addition you will receive 500 pesetas for showing-up for the experiment. At the end of the experiment your earnings will be paid to you in cash.

In a moment, each of the 20 participants will be randomly assigned to one of two groups: 8 will be "employees" and 12 will be "employers".

In the experiment there will be several periods. In total there will be 10 periods. Your total earnings for your participation in the experiment will be the sum of your earnings in each of the 10 periods.

In each period you will partcipate in a labor market. Each labor market will have two stages:

Stage 1: In the first stage the employers will make decisions: they will be able to make "wage offers" to the employees. Employees will be able to accept these offers. After 5 minutes the first stage will be over. At that moment all those wage offers that have not been accepted will be randomly assigned to some of the employees who have not accepted any wage offer. Then stage 2 will begin.

Stage 1: In the second stage, each of the employees who have accepted a wage offer will make a decision: he/she will choose a "quantity of labor".

Before the experiment starts we will give you a decision sheet on which you will register your decisions in each period. You will also register the decision of the person in the other group with whom you have entered into a relation in the period. After that you will calculate your earnings.

2. HOW DOES THE MARKET WORK?

At the beginning of each period the labor market will open. In the first stage of the market the employers will be able to make wage offers to the employees.

We will write the wage offers on the blackboards of both the employer and the employer room as they are made. In total employers and employees will have **5 minutes** to trade. Each employer will be able to make more than one offer, but each new offer will have to be larger than the highest offer that has not yet been accepted.

If an employee accepts a wage offer he/she establishes a "labor contract" with the employer who has made the offer. Any employee can establish a wage contract with any employer and any employer can "hire" any employee. However, if an employer and an employee have closed a labor contract these participants will not be able to establish any other contract in the period.

When an employess accepts a wage offer of an employer, both should immediately register this wage on their decision sheets.

No employer will know with which employer he/she has closed a contract, and no employer will know the employee.

After 5 minutes the second stage will begin. At that moment each employee who has accepted a wage will have to decide which quantity he/she wants to work. Then we will communicate the quantity of work to the employer with which he/she has entered into a contract for the period. No other employee and no other employer will be informed about the chosen quantity of work.

3. HOW TO CALCULATE YOUR EARNINGS FOR THE PERIOD?

A wage and a quantity of work are transformed into earnings for the employer and the employee who have closed a contract in the period. For the employer a wages becomes a cost and a quantity of work becomes a gain. For the employee the wage becomes a gain and the quantity of work becomes a cost.

The employer will choose a wage between 0 and 10 and the employee will choose a quantity of work between 0 and 10.

The earnings (in pesos) for a period of an employee and of an employer who are matched will be determined in the following way:

Earnings of the employer = 10 – wage + 5 x quantity of work.

The higher the quantity of work the higher will be the earnings of the employer and the higher the wage the lower will be the earnings of the employer.

Earnings of the employee = 10 – quantity of work + 5 x wage.

The higher the quantity of work the lower will be the earnings of the employee and the higher the wage the higher will be the earnings of the employee.

An employer that has not made an offer in a period will obtain an earnings of 10 pesos. An employer that has made an offer but has not entered into a relation with an employee will obtain earnings of 10 pesos. An employee that has not accepted any offer may be randomly assigned to

one of the wage offers that have not been accepted. If there is no wage offer to which you can be assigned , the employee will earn 10 pesos.

Are there any questions?

During the experiment it will not be allowed to talk or communicate with the other participants. If you have a question, please, raise your hand and one of us will come to your desk to answer it.

Now please take one of these pieces of paper. If on the paper you see a "1", please follow our indications for moving to another room. If on the paper you see a "2", please stay in this room and follow our indications.

INSTRUCTIONS AND EXERCISES FOR THE EMPLOYERS.

An employer who wishes to make a wage offer should raise his/her hand. Once one of us has given an indication that he/she can talk, he/she will say his/her employer number and the wage offer. Right after that he/she should register the wage on the decision sheet.

Now we are going to do some exercises. Please, use the expressions to calculate earnings that we gave to you earlier.

1. Let's suppose that you, being able to choose wages between 0 and 10, have made a "wage offer" of 8 pesos which has been accepted by an employee and that in the second stage of the period the employee chooses a "quantity of work" of 5.

What will be your earnings and the earnings of the employee with which you have closed a contract for the period?

My earnings = ……..pesos.

Earnings of the employee = ……pesos.

2. Let's suppose that you, being able to choose wages between 0 and 10, have made a wage offer of 3 pesos which has been acepted by an employee and that in the second stage of the period the employee chooses a quantity of work of 6.

What will be your earnings and the earnings of the employee with which you have closed a contract for the period?

My earnings = ……..pesos.

Earnings of the employee = ……pesos.

3. Let's suppose again that you, being able to choose wages between 0 and 10, have made a wage offer of 3. However, let's now suppose that in the second stage of the period the employee chooses a quantity of work of 0.

What will be your earnings and the earnings of the employee with which you have closed a contract for the period?

My earnings = ……..pesos.

Earnings of the employee = ……pesos.

INSTRUCTIONS AND EXERCISES FOR THE EMPLOYEES.

An employee who wishes to accept a wage offer that has been made should raise his/her hand. Once one of has given an indication that he/she can talk, he/she will say his/her employee number and state which wage offer he /she accepts. Right after that he/she should register the accepted wage on the decision sheet.

Now we are going to do some exercises. Please, use the expressions to calculate earnings that we gave to you earlier.

1. Let's suppose that an employer, being able to choose wages between 0 and 10, has made a "wage offer" of 8 pesos which you have accepted and that in the second stage of the period you choose a "quantity of work" of 5.

What will be your earnings and the earnings of the employer with which you have closed a contract for the period?

My earnings = ……..pesos.

Earnings of the employer = ……pesos.

2. Let's suppose that an employer, being able to choose wages between 0 and 10, has made a wage offer of 3 pesos which you have acepted and that in the second stage of the period you choose a quantity of work of 6.

What will be your earnings and the earnings of the employer with which you have closed a contract for the period?

My earnings = ……..pesos.

Earnings of the employer = ……pesos.

3. Let's suppose again that an employer, being able to choose wages between 0 and 10, has made a wage offer of 3 which you have accepted. However, let's now suppose that in the second stage of the period you choose a quantity of work of 0.

What will be your earnings and the earnings of the employee with which you have closed a contract for the period?

My earnings = ……..pesos.

Earnings of the employer = ……pesos.