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

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December 2001

Abstract: We study whether people's behavior in unbalanced gift exchange markets with repeated interaction 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' behavior, as second movers (*workers*) might react differently to favorable actions by first movers (*firms*) in the two cases. While our data show strong deviations from the standard game-theoretic prediction, we find mainly secondary treatment effects. Wage offers are not higher when there is an excess supply of firms, and workers do not respond more favorably to a given wage when there is an excess supply of labor. The state of competition does not appear to have strong effects in our data. We also present data from single-period sessions that show substantial gift exchange even without repeated interactions.

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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 experimental analysis of these markets, first studied 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 are still quite unclear. In particular, there has been no previous study on whether and how gift exchange is affected by the competitive pressures that are present in a market environment. Our aim in this paper is to study whether a specific feature of market conditions, the state of competition, 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 experimental gift exchange markets there may be more workers than firms or the other way around, and this relation determines the degree of excess supply or demand for labor. We are interested in the psychological, not in the strategic, impact of changing from a situation with excess supply of labor to one of excess demand. We believe that the psychology of competition is an important economic issue. If the state of competition had a significant effect on behavior, due to some kind of interdependence of motivations, this would affect the very basis of how economists think about markets, since it would imply that a specific feature of the economic environment interacts with people's preferences in these markets.

One can describe the basic sequence of events in a typical 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 and are free to choose any of the feasible levels, including one with zero cost. Higher wages yield lower monetary payoffs for firms and higher ones for workers, while higher effort levels have the reverse effect on payoffs.

The standard game-theoretic prediction is that workers will invariably choose the lowest possible effort level, since this choice is dominant in a pecuniary sense; in anticipation of this, firms will only make the lowest possible wage offer. However, evidence from numerous studies shows substantial deviations from the equilibrium prediction.[2]

Experimental gift-exchange game results are very interesting and have been very influential. However, to date there is no clear agreement on the precise motivational forces underlying this behavior. A claim is often made that there is some form of *reciprocity* involved, although the usage of this term seems to vary. A worker may make a costly effort choice, because of his perception of the generosity of the wage offer. When there are different degrees of market imbalance, the wage offers may differ, and the way that a worker views a given wage may also differ. For example, an attractive wage offered when there are more workers than jobs may induce a more favorable response than the same wage offered when there are more jobs than workers. Alternatively, some papers have argued that costly effort provision is not a sign of positive reciprocity, but instead reflects a desire to "share the

[1] Fehr and Falk (1999) study the case where workers can make counter-offers.
[2] Other recent evidence of this includes Fehr, Gächter and Kirchsteiger (1997), Fehr, Kirchsteiger and Riedl (1998) and also Hannan, Kagel and Moser (forthcoming).

wealth". To the extent that costly effort stems from distributive concerns, we would not expect it to depend on the direction of market imbalance.

The focus of our study is very much related to the more general theme that preferences depend not only on the outcomes that follow from certain choices, but also on information concerning the *process* leading to these outcomes. Information pertaining to the process – and not to the outcome - may matter because it offers inferences about the intentions or disposition behind the actions of others. The state of competition is, in our view, a relevant aspect of the process by which market allocations are determined.

We believe that our paper contributes to the identification of the precise determinants of behavior in gift-exchange games. To make progress in this direction, it is necessary to rigorously study gift-exchange behavior from a variety of different angles. 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 would be unusual to find data with the desired variations in the non-outcome information. In contrast, experiments make it possible to generate this kind of evidence in a systematic manner.

Our results suggest, perhaps surprisingly, that gift-exchange behavior is not substantially affected by certain changes in the degree of 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. Behavior 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

Roth, Prasnikar, Okuno-Fujiwara & Zamir (1991) demonstrate convincingly that results are very different for an ultimatum game (one-to-one matching) and a "market game" in which a single agent on one side can agree to a proposal from any of nine agents on the other side. That kind of difference in behavior can easily be explained in terms of purely distributional preferences (see Bolton and Ockenfels, 2000). Our focus is on a more emotional link between market imbalance and behavior. In our markets, contracts can be considered to be incomplete, in the sense that workers make their choices after wages have been fixed. In this context, we conjecture that workers may perceive a higher wage to be inspired by "pure gift giving" (when there is an excess of workers) or "gift giving under competitive pressures" (when there is an excess of firms).

In previous gift-exchange experiments the number of workers was about 1/3 to 1/2 higher than the number of firms; no previous study has considered the case whether there are more jobs than workers. In times of economic boom or when a task is highly specialized, such conditions are not uncommon; Silicon Valley experienced a chronic shortage of high-tech workers in the late 1990's.

Information about the state of competition may be useful for inferring the intentions of others, as it pertains to the opportunities that others have in the market. In a 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]

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. Specific models of interdependent preferences differ with respect to whether motivation is affected by non-outcome information. 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. In contrast, Rabin (1993) presents models of reciprocal altruism, in which beliefs about intentions can affect behavior in two-person normal-form games. Charness and Rabin (forthcoming) offer experimental evidence on the effects of foregone alternatives on choice, as well as a model of "social-welfare" preferences (a combination of utilitarian and Rawlsian preferences), in which reciprocity considerations modify these preferences.

Previous experimental studies have looked at non-outcome information of different types. For example, several papers vary, in sequential two-person games, whether a self-interested party or some

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

external mechanism determines the choices available to the subsequent player. This relates to Sen's concept of chooser dependence. Charness (1996) is perhaps the most closely related to our work here, since it is based on experiments with gift-exchange games.[5] He finds that the attribution of volition has a significant effect on behavior when wages are relatively low. However, this kind of evidence is not present in the high-wage range: A high wage chosen by a self-interested employer leads to effort similar to that seen when this wage comes from a bingo-cage draw.

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 (2001) present some results from simple sequential dilemma games and find that foregone opportunities do not affect behavior significantly. On the other hand, Brandts and Solà (2001) and Falk, Fehr, and Fischbacher (forthcoming) 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.

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]

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[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.

[5] Blount (1995) and Offerman (1998) also find evidence that behavior in sequential games is affected by the process leading to the available alternatives.

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

This paper tests whether the behavior observed in gift-exchange markets is sensitive to the direction of a 50% supply/demand imbalance. Perhaps behavior simply reflects concerns about the distribution of outcome payoffs. It could, however, also be affected by whether people are on the long or the short side of the market. In a broad sense, one could think of this as another form of Sen's menu dependence: Workers would judge a given wage offer in relation to firms' set of options.

3. EXPERIMENTAL DESIGN, PROCEDURES & HYPOTHESES

The gift-exchange game is a type of two-player dilemma game that is played in a sequential fashion. Dilemma games are 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] For purely self-interested players these games have a unique subgame-perfect Nash equilibrium in which both players choose the minimum gift level.

Embedding a game of this type in a market environment with competition 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 this does not affect the pecuniary incentive structure of subjects given that they have been matched.

[7] In the context of a public good game these gifts can be seen as contribution levels. The investment (trust) game introduced by Berg, Dickhaut and McCabe (1995) is also a sequential dilemma game, and will be discussed later in the paper.

We conducted a total of twelve experimental sessions in Barcelona, involving a total of 226 participants. Eight of the sessions corresponded to our investigation of the effects of market imbalance; there were four (10-period) sessions with an *excess supply of labor* (hereafter, ESL) and four with an *excess supply of firms* (hereafter, ESF). 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. It is standard in gift-exchange games to employ this multi-period design. In our context, the repetition of the situation may have the effect of increasing the salience of the market imbalance, and so may increase the chance of a treatment effect.

However, repetition may also lead to reputation effects. In our experimental procedures interaction took place under anonymity, so that no identification of the other person in a match is possible. In this sense, each interaction should be considered to be a separate event. However, a worker knows that she might be anonymously re-matched with the same firm, so that dynamic considerations may be relevant. In addition, perhaps the number of participants is small enough to attempt to maintain a group reputation.

Our focus in this study is on the existence of a treatment effect from varying the state of competition. This effect should not be influenced by the possible presence of reputation, i.e. *a priori* there is no reason to believe that market imbalance will interact with reputation formation. However, to verify that gift exchange itself is not entirely an artifact of repeated interaction we also conducted four additional sessions with pure one-shot gift exchange (no multiple periods).

3.1 Design. In our multi-period sessions, 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, together with 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.[10]

The linearity of our payoff function simplifies the decision situation by making the marginal effect of effort independent of the wage. It will also facilitate the formulation of our null hypothesis below. Another important feature of our design is that the information available to participants was the same in

[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 subsequent experiments.

[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.

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 will offer a wage of zero or will not make any offer. As a consequence, all agents would obtain a payoff of 10, independently of the existence and type of market imbalance.

In our four sessions with one-shot encounters, we used the payoff functions:

$$FI = 800 - 20w + 100e \qquad (1')$$

$$WI = 800 - 20e + 100w, \qquad (2')$$

with 1 unit = 1 peseta. To generate suitable final earnings for participants in a one-shot experiment the parameter values are different than those in (1) and (2). Observe, however, that the transformation rate between wage and effort is equal to 5, just as in the previous payoff functions. The only difference is the size of the constant term; linearity and symmetry are preserved.

3.2 Procedures. The market imbalance sessions took about two hours. Subjects were recruited among students from a variety of fields of study using announcements in buildings at Universitat Pompeu Fabra. At the beginning of each experimental session, all participants were gathered in one room and

[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.

the instructions were read to them, while they read along.[12] During this time subjects could ask public questions about the procedures. The participants were then 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. 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 0 and had then been matched with a worker who chose a 0 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 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 market-imbalance session. At the end of a 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. Average total earnings were about 2000 pesetas.

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

For the four single-period sessions, participants were recruited using announcements in university buildings at the Autonomous University of Barcelona and were students from a variety of majors. Each session had an even number of participants (18, 18, 14 and 16), with half randomly assigned the role of first movers (*firms*) and the other half assigned the role of second movers (*workers*).[14] As in the multiple-period sessions, subjects were gathered in a room and the instructions were read to them while they read along. The single period developed as follows: First, each first mover decided separately on a wage offer. Each offer was then communicated to a randomly selected second mover, who then made an effort decision. After that the session was over; subjects were privately paid their earnings and left the room. The average payment in these half-hour sessions was approximately 1100 pesetas.

3.3 Hypotheses. According to the most standard view of economic behavior, both wages and 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. In our set-up the "error" can only go in one direction, as it is possible for wage and 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 purely random mistakes in their decisions. Alternatively, Anderson, Goeree and Holt (1998) posit that relatively costly mistakes are less likely. Note that costs of deviations are the same under ESF and under ESL; given the linear structure of our payoff function in neither treatment do

[13] We used the telephone to communicate the offers to the other room.
[14] These sessions did not involve any market interaction, since they were specifically thought of as controls for the dynamic aspects of the other sessions.

these costs depend on the actions of others. Thus, incorporating the second conception of errors into the standard prediction leads to the following (weaker) null hypothesis, composed of three elements**:**

H_0:. *(i) Wage offers are the same under ESF than under ESL.*
(ii) Effort levels are independent of wage levels.
(iii) Holding wages constant, effort levels are the same under ESF and ESL.

We now move to our alternative hypotheses. The anticipation of firms' behavior is somewhat more complex than that of workers as it may involve both strategic and motivational elements. In our set-up, one might expect that in a tighter labor market (ESF) the additional competitive pressure could lead to higher wages than in one with slack. However, if firms anticipated that workers would return a gift more generously under ESL then this might lead to *higher* wages under ESL than under ESF. The interplay of these two (and other) forces is hard to predict, and hence we formulate a two-sided alternative to portion (i) of the null hypothesis:

H_{Ai}: *Wage offers are different under ESF than under ESL.*

Current models of interdependent preferences predict that higher wages will lead to higher effort levels, as is the case in all previous gift-exchange experiments.[15] In line with these models, we formulate the alternative to portion (ii) of our null hypothesis:

H_{Aii}: *Effort levels are increasing in wage levels.*

We conjectured earlier that there would be differences across the ESL and ESF treatments. Specifically, a high wage in ESL may seem more generous than the same wage in ESF, as high wage offers in the latter case may be viewed as an attempt to achieve a match, in light of the greater

[15] Bolton and Ockenfels (2000) and Fehr and Schmidt (1999) presume that people dislike payoff disparities. As these disparities increase with higher wages, effort is predicted to also increase. Charness and Rabin (forthcoming) presume that people like social efficiency and are also concerned about the lowest payoff. The first of these factors predicts positive effort levels, and the second factor contributes to a positive wage/effort relationship.

competitive pressure on the firms. This conjecture leads to our alternative hypothesis for portion (iii) of the null:

H_{Aiii}: Holding wages constant, effort levels are higher under ESL than under ESF.

4. RESULTS

The ESL and ESF results are displayed graphically in Figures I and II.

[Figures I and II about here]

The patterns are similar in both treatments; in both cases the modal outcome is the firm offering a wage of 10 and the worker choosing an effort level of 10. The height of the (0,0) column in ESF is much smaller, as the bulk of the 0 wage proposals were left unmatched.

Note the almost total lack of points to the southeast. In fact, there were only 4 occasions (3 in ESL and 1 in ESF) where effort exceeded the wage. While there is a slight "ridge of reciprocation" along the 45-degree line, people very rarely respond to a "gift" with a larger "gift", even though this would increase the total payoff. However, there are many observations with 0 effort, even when the wage is 10. This asymmetry leads to the workers earning, on average, substantially more than the firms do.[16]

4.1. Firm behavior. The average accepted wage offer in the ESF treatment was 8.36, 12% higher than the 7.45 average wage in the ESL treatment. However, when we consider all wages *offered* in

[16] This is a familiar phenomenon in dilemma games of this mature. This fact may be surprising at first sight, but it is actually very much in line with social-preference models such as Bolton and Ockenfels (2000) and Fehr and Schmidt (1999), and Charness and Rabin (forthcoming). According to these models, second movers will tend to make choices that give themselves more than half. In a certain sense, the responder holds more of the power, as the potentially large efficiency gains can only occur at the discretion of the responder.

ESF (recall that 1/3 of the offers in ESF are left unmatched), the average is 7.35. There is a high proportion of very high wages in both treatments; the median wage offer was 9 under both ESL and ESF, while the median accepted wage under ESF was 10.

A different perspective on firms' behavior is given by Figure III, which shows the average wages over time for both treatments.

[Figure III about here]

The differences between the three wage series appear to be quite small throughout the periods, with accepted wages in ESF being slightly higher. Comparing all wages across treatments, in this presentation one gets the impression that in the first part of the session average wage offers are higher under ESF than under ESL, while for the second part it is the other way around. Wages are generally high in all periods except the last one.

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. Our analysis primarily consists of nonparametric tests performed on these data points; we also report random-effects ordered-probit regressions, which take into account multiple observations. At some points we mention other types of tests, if we judge them to be informative.

Table 1a presents average wages for all eight ESL and ESF sessions, both for complete sessions and for the first and second part of the sessions. The results of the (two-tailed) permutation tests we performed show that the differences in average wage offers between the two treatments are not

significant at anything close to conventional levels for the three ways of organizing the data.[17] We have also examined the statistical significance of the disparity between the frequency of offers at the lowest and highest wage. Using the permutation test we have failed to find differences in the proportions of wages equal to zero as well as in 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. The smallest *p*-value for the six different permutation tests is equal to .243.

Even if we relax our strict requirement by considering each observation to be independent, non-parametric tests still fail to confirm any significant difference in wage offers across treatments. For example, a Kolmogorov-Smirnov test (see Siegel and Castellan 1988) on the cumulative proportions of wage offers made gives $\chi^2(2) = 1.69$, $p = .42$. In summary, the actions of the first-movers of our experimental markets do not appear to be affected by whether there is excess supply or excess demand.

Table 1b shows the average wages for each of our single-period sessions and Table 2 shows the distribution of wage offers in those sessions. Note that the 33 wage observations are all independent from each other; the same is true for the 33 effort observations. As with the ESL and ESF treatments, 0 and 10 are the most frequent wage levels and there is some additional bunching for intermediate wage levels. The average wage is 4.84, lower than the average wage in the ESL and ESF treatments, but in line with the final-round wage offers in these treatments.

[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).

4.2 Worker behavior. The evidence has shown 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 IV presents average effort for the different feasible wage levels for the ESL and ESF treatments, aggregated over all four sessions of the respective treatments.[18]

[Figure IV about here]

Wages and effort levels appear to be positively related; we 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 4, 5 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]

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

[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 kind actions. Outcome-based models predict that a worker would make the same effort choice if a random process had chosen the same wage for the worker.
[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.

assumption of the independence of observations. Figure V shows the behavior over time of the average effort level over the 10 experimental periods for each of the treatments.

[Figure V about here]

With respect to treatment effects, we do not observe generally higher effort levels for ESL. Table 3a presents average effort levels for each ESL and ESF 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 Table 1a and reported above, the permutation test does not find any significant differences between treatments. Note that this is also true for early periods despite the apparent difference in Figure V.[22]

Table 4 presents the data for three additional session indicators, the Spearman rank correlation coefficient, the Tobit regression slope coefficient, and the average level of firm income. Note that these three indicators pertain not to the effort level but to the effort-wage relation. We feel that this last relation is a very natural one to use, since it directly captures the consequences of possible treatment effect for firms' payoffs. Table 4 shows that the treatment differences for these indicators are again not statistically significant.[23]

Table 5a 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 cannot reject part (iii) of our null hypothesis.

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[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.

[22] Average effort was actually somewhat higher under ESF than under ESL: 4.41 vs. 3.85. At first glance, this may even suggest that workers are actually more generous when the labor market is tighter! However, even though wage offers are similar across treatments, the average *effective* wage (offers accepted) is determinative, and we have seen that this is also higher with ESF. In fact, the proportion of average effort to average wage is quite similar.

[23] 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

While nonparametric tests are very clean, they may lack some of the power of the more conventional regression-analysis approach. We use a random-effects ordered probit model to accommodate our discrete data and multiple observations. Although this is based on a number of assumptions about the covariance structure, some of which may not be fully satisfied for our data, it provides a good tool to explore the apparent difference between treatments in the rate at which effort levels increase with wage levels.

Table 6 presents the results of these regressions, which all include period dummies (period 6 as the baseline period) and treatment dummies.[24] Consistent with the impressions one gets from the inspection of Figures II and IV, there are some significant period effects. The results for ESL*Wage and ESF*Wage reveal a significant positive relation between effort and wage levels for both treatments, in line with the results of our non-parametric tests. Our central concern is whether there is a treatment difference in the effort response to wages. Testing for the difference between the coefficients on ESL*Wage and ESF*Wage we find no effect, with $\chi^2(2) = 0.57$, $p = 0.75$. The p-value for the ESL coefficient implies that there is no treatment effect on the constant.

While we observe substantially positive effort levels in the ESL and ESL sessions, the question remains whether this is driven by repeated-interaction effects. Table 3b shows average effort levels in the four single-period sessions. Here we see that the average effort level is 2.43, substantially lower than the ESL and ESF levels (3.85 and 4.41, respectively), but still somewhat higher than both the final round ESL and ESF values. However, recall that the average wage offer is also lower in the single-

indicators we 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.

[24] All cut-points were highly significant.

period sessions. The average effort is 50.0% of the average wage, not much different than the 51.6% and 52.8% ratios in the ESL and ESF sessions, respectively.

Average gift exchange is still sizable; the sum of first and second movers' average contributions (6.27) is more than 30% of the maximum feasible level (20). On average, first movers earn more than 18% and second movers more than 54% more than in the absence of gift exchange. Effort levels increase with wage categories, as with the ESL and ESF treatments (see Table 5b).

Returning to the market interaction sessions, note that the information shown in Figure IV 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 actions without cooperative gains would not be easy to interpret, since firms would be earning less than at the zero wage level.

Figure VI shows average firm income (FI) and average worker income (WI) for both treatments, by wage level.

[Figure VI about here]

It can be seen directly that 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.[25]

The proportion of accepted 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 accepted 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.[26] One can, for instance, look at efficiency gains at a wage of 10: 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. None of the measures suggest the presence of a relevant treatment difference.

5. EVALUATION OF OUR HYPOTHESES AND DISCUSSION

Let us consider the data in the light of our hypotheses. Wage offers are similar across treatments, in contrast to H_{Ai}, although the effective wage paid is indeed a bit higher in the tighter labor market. We can easily reject part (ii) of the null hypothesis in favor of the alternative H_{Aii}; effort levels are increasing in wages. This result is largely in line with other sequential prisoner's dilemma games. The trust (investment) game of Berg, Dickhaut & McCabe (1995) is related to our game, the main differences being that in their game the first-mover's pass is tripled and the responder's pass is not augmented. They find that 92% of all senders sent a positive amount of money, and 80% of all

[25] 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.

[26] 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.

responders who were sent money returned a positive amount.[27] However, only a minority of first-movers who send positive amounts are made as least as well off as before by the response.

Another related sequential prisoner's dilemma study is Clark and Sefton (2001). Here they use a series of 2x2 games, so the choice set is far more restricted. Like us, they find that responders choose to cooperate (essentially, choosing an effort level of 10) much more frequently after first-mover cooperation (essentially a wage of 10) than after first-mover non-cooperation (essentially a wage of 0). The responder cooperation rates averaged 34% after first-mover cooperation vs. 4% after non-cooperation.[28] In our data, we find that 38% of wage offers of 10 receive an effort response of 10, while a wage offer of 0 never receives an effort response of 10.[29] Clark and Sefton (2001) also observe that cooperation rates were sensitive to the cost (effectiveness of cooperation), suggesting that different levels of gift exchange would occur with different multiplying factors.

Our result that we wish to specially highlight is that we cannot reject part (iii) of the null. 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. However, we find little support for differences in behavior, as is seen in the test results with quite a number of different indicators as well as in the regression results; none of our indicators come close to showing a difference. After evaluating treatment effects in a variety

[27] These figures combine the "no history" and "social history" treatments.
[28] These figures derived from their Table 6, p. 58.
[29] If we restrict effort and wage choices to {0,10}, our game in their 2x2 format would be:

	e = 0	e = 10
w = 0	10, 10	60, 0
w = 10	0, 60	50, 50

of ways, the data allow us to conclude that our imbalanced-market conditions, as an isolated factor, did not affect effort responses significantly.

As there is no clear treatment effect, our data suggest that effort responses reflect distributional preferences more than gratitude for perceived generosity. In fact, formal models of social preferences that do not include positive reciprocity, such as Fehr and Schmidt (1999), Bolton and Ockenfels (2000), and Charness and Rabin (forthcoming), can explain gift exchange as an equilibrium phenomenon and, in general terms, fit together well with our data. However, perhaps there is something in our data that suggests the need to go beyond models of the type just mentioned.

In this respect, we do observe one modest treatment difference that bears some mention. 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 obscure 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. The average effort for intermediate wages was 1.94 in the ESF treatment, compared to 3.33 in the ESL treatment. The main difference is that 46 of 80 (58%) responses to intermediate wages were 0 under ESF, while only 22 of 64 (34%) responses were 0 under ESL. However, Figure I shows that intermediate wage offers are only a modest fraction of all wage offers.

On further reflection it appears that conceiving of behavior in gift exchange markets fundamentally in terms of the rewarding of perceived generosity may lead to somewhat of a paradox. Positive reciprocity implies that workers expend effort to reward wage levels that they perceive to be generous. But to appear truly generous a wage should surely be above the one that yields the highest

profits for firms. In our data this is actually not possible, since the *ex post* profit-maximizing wage is 10 in both the ESF and the ESL treatments.[30] More generally, however, if certain wage levels that are perceived as generous are sufficiently reciprocated, then they may themselves become the profit-maximizing ones. This reflection is consistent with the fact that our data square with models that do not incorporate this kind of positive reciprocity.

Our treatment design is intended to generate a difference in a worker's attribution for a received wage. Another approach to comparing enforced and spontaneous generosity might be to study the effect of imposing a nonzero minimum wage. While this is reasonable in principle, recall that the median wage accepted was either 9 or 10. For a minimum wage to appear generous, it presumably should not be much less than the median, or it will have no different effect than the 0 minimum wage already imposed. Requiring a wage of 9 or 10 would essentially reduce the interaction to a form of dictator game.

The results from our single-period sessions are similar in spirit to those in the ESL and ESF treatments, demonstrating that repeated interaction is not required for gift exchange. However, the reduced wage and effort levels, as well as the decay seen in the ESL and ESF sessions, suggest that behavior may have been influenced by some type of strategic consideration, perhaps such as group reputation-building. Nevertheless, the average firm income in period 10 is still substantially larger than the equilibrium prediction, in both treatments.

[30] Of course, this does not address the question of *why* a wage of 10 is expected (*ex post)* to maximize firm income in each treatment.

6. CONCLUSION

Our principal finding is that in our data the differences between our ESL and ESF treatments treatment effects are merely 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. One reaction to our findings might be the view 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 must be evaluated in the light of some other features of our results as well as of other relevant experimental evidence. As discussed in detail in section 4, 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. Thus, the absence of treatment effects is not the result of laboratory behavior conforming to the standard game-theoretic prediction.

Our evidence can be put into perspective 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). Second, perhaps due to a form of self-serving bias, people may react more strongly to perceived negative intentions (which may play no role in our context) than to perceived positive intentions.[31]

[31] See Offerman (forthcoming).

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 (2000). With competitive 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. Our results suggest that models of interdependent preferences may not need to take into account the effects of market imbalance on motivation.

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Table 1a

Average Wages in Multi-period Sessions

	Session ESL-1	Session ESL-2	Session ESL-3	Session ESL-4	Session ESF-1	Session ESF-2	Session ESF-3	Session ESF-4	Permutation test results
Average wage in first part of session	8.275	4.125	8.675	7.025	8.817 [9.15]	6.85 [7.975]	6.267 [7.575]	9.617 [10]	*P*=.514 [*P*=.228]
Average wage in second part of session	9.376	5.325	8.25	8.15	6.317 [6.8]	5.633 [7.85]	5.567 [7.5]	9.7 [10]	*P*=.458 [*P*=.886]
Average wage in whole session	8.825	4.925	8.462	7.588	7.567 [7.975]	6.242 [7.912]	6.917 [7.538]	9.658 [10]	*P*=.914 [*P*=.514]

In this Table, the first row in each of the ESF cells includes all wages offered. The second line, in brackets, includes only those (8 of 12) wage offers that were accepted. The permutation tests were carried out using all wage offers made.

Table 1b

Average Wages in Single-period Sessions

	Session S-1 (9)	Session S-2 (9)	Session S-3 (7)	Session S-4 (8)	Overall (33)
Average wage in session	6.444	5.111	5	3.25	4.848

The numbers of observations are in parentheses.

Table 2
Single-period Session Results

Wage levels	**0**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
Frequency of wage levels	**8**	**2**	**1**	**1**	**3**	**7**	**0**	**1**	**0**	**1**	**9**
Average effort levels	**1.5**	**0**	**2**	**4**	**1.3**	**3.1**	**-**	**0**	**-**	**6**	**3.75**

Table 3a

Average Effort in Multi-period Sessions

	Session ESL-1	Session ESL-2	Session ESL-3	Session ESL-4	Session ESF-1	Session ESF-2	Session ESF-3	Session ESF-4	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*=.742
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	5.562	1.55	4.463	3.813	4.238	2.362	2.613	8.438	*P*=.614

Table 3b

Average Effort in Single-period Sessions

	Session S-1 (9)	Session S-2 (9)	Session S-3 (7)	Session S-4 (8)	Overall (33)
Average effort in session	4.333	2.667	1.286	1	2.424

The numbers of observations are in parentheses.

Table 4

Permutation Tests on Other Measures, Multi-period Sessions

	Session ESL-1	Session ESL-2	Session ESL-3	Session ESL-4	Session ESF-1	Session ESF-2	Session ESF-3	Session ESF-4	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 5a

Average Effort by Wage Classification, Multi-period Sessions

	Session ESL-1	Session ESL-2	Session ESL-3	Session ESL-4	Session ESF-1	Session ESF-2	Session ESF-3	Session ESF-4	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 5b

Average Effort by Wage Classification, Overall

	ESL Sessions	ESF Sessions	Single-period Sessions
Avg. Effort for Wages 0 to 4	0.281 (64)	0.077 (26)	1.467 (15)
Avg. Effort for Wages 5 to 8	3.172 (64)	1.938 (80)	2.75 (8)
Avg. Effort for Wages 9 and 10	5.26 (192)	5.855 (214)	3.6 (10)

The numbers of observations are in parentheses.

Table 6

Random-efforts Ordered Probit Regression on Effort (All Periods)

Independent variable	*Coeff.*	*Std. Err.*	*Z*	*P > |Z|*
Period 1	0.422	0.245	1.72	0.08
Period 2	0.054	0.226	0.24	0.81
Period 3	0.441	0.226	1.96	**0.05**
Period 4	0.370	0.230	1.61	0.11
Period 5	0.134	0.228	0.59	0.56
Period 7	0.073	0.226	0.32	0.75
Period 8	-0.384	0.231	-1.66	0.10
Period 9	-0.983	0.234	-4.19	**0.00**
Period 10	-1.490	0.284	-5.24	**0.00**
ESL	-0.404	0.550	-0.73	0.46
ESL*Wage	0.389	0.039	10.06	**0.00**
ESF*Wage	0.352	0.051	6.90	**0.00**

Number of observations = 632

LR $X^2(11) = 281.67$, $p = 0.000$

Log-likelihood = -894.749

Figure I

ESL Wages and Effort

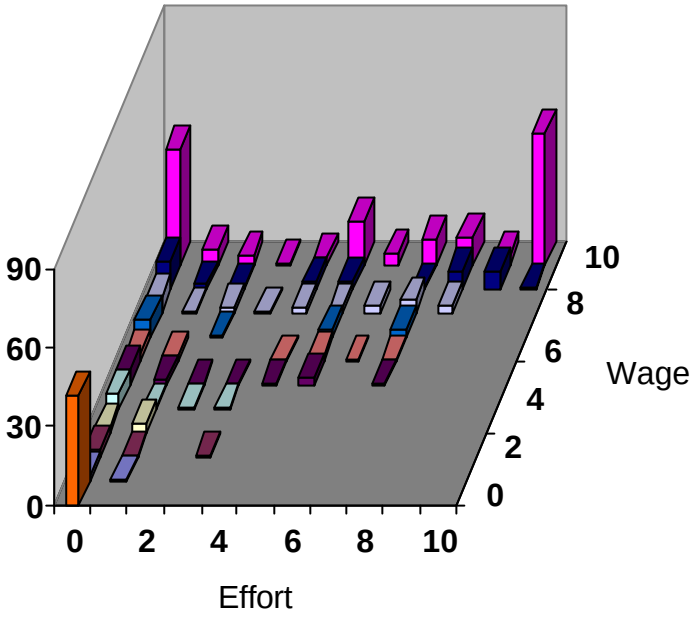


Figure II

ESF Wages and Effort

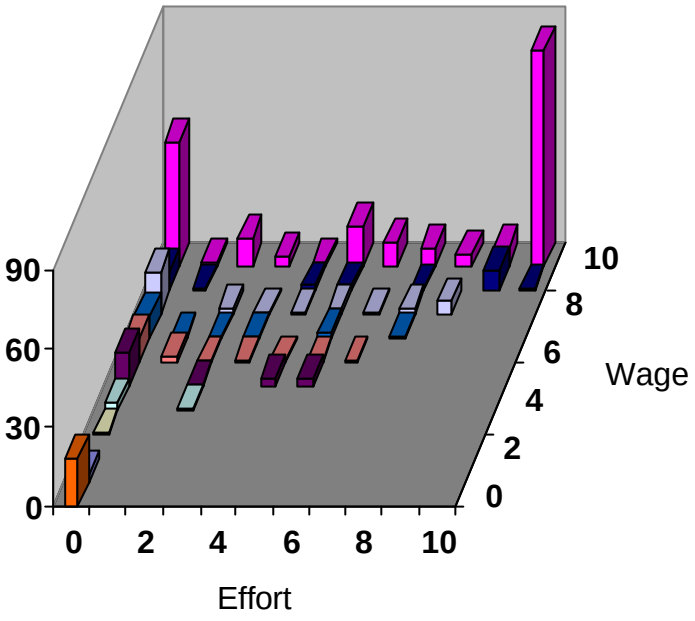


Figure III: Average Wages over Time

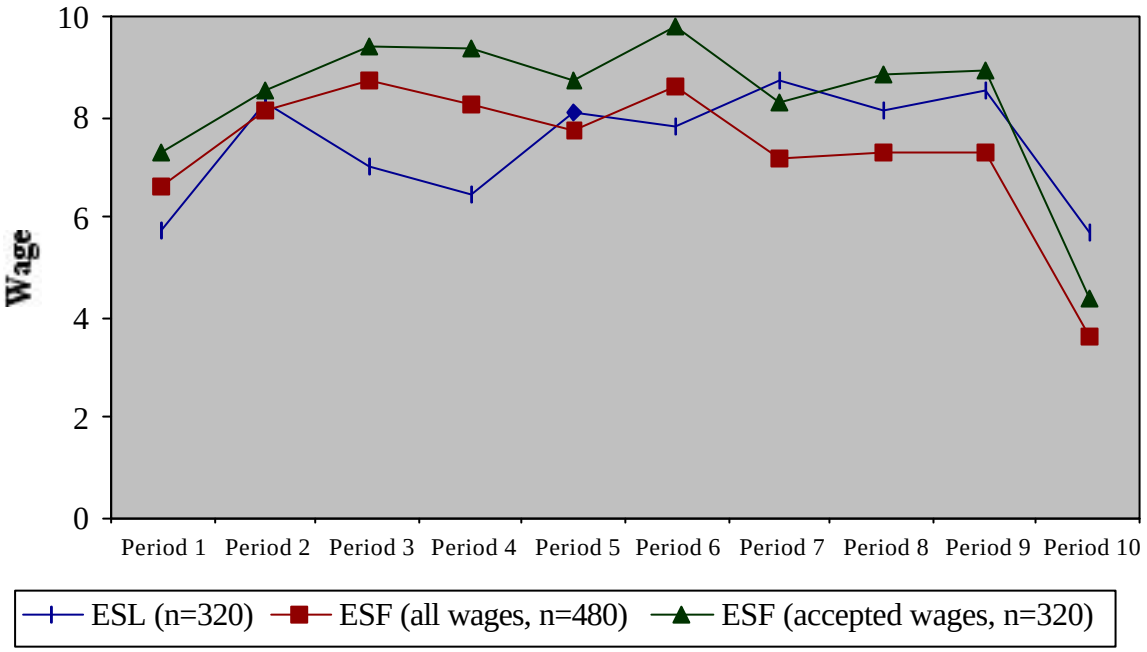


Figure IV: Average Effort per Wage Level

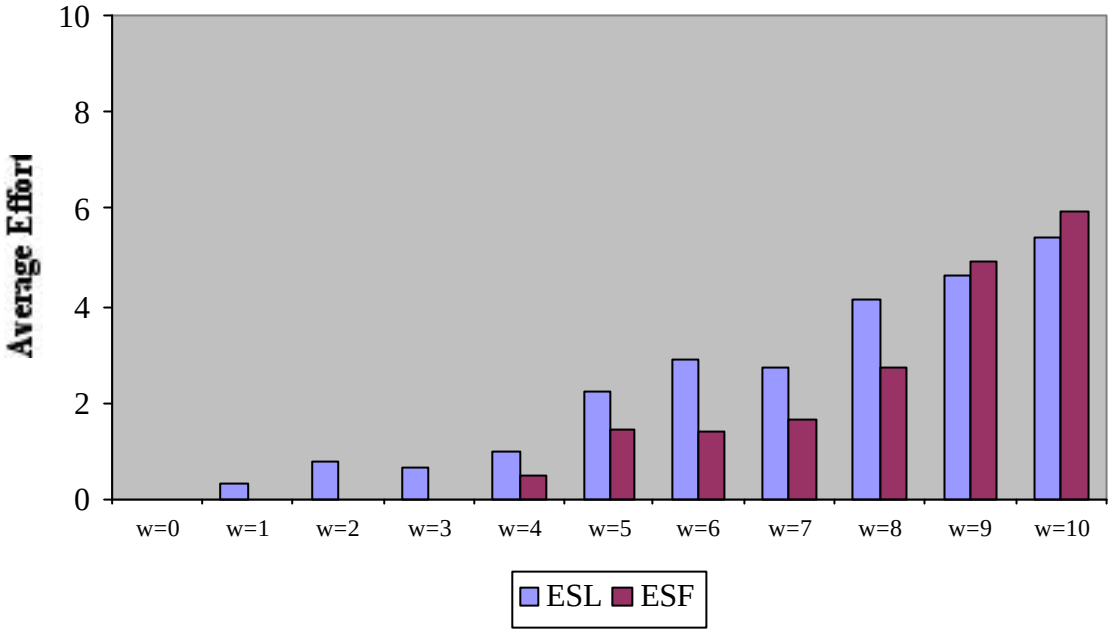


Figure V: Average Effort over Time

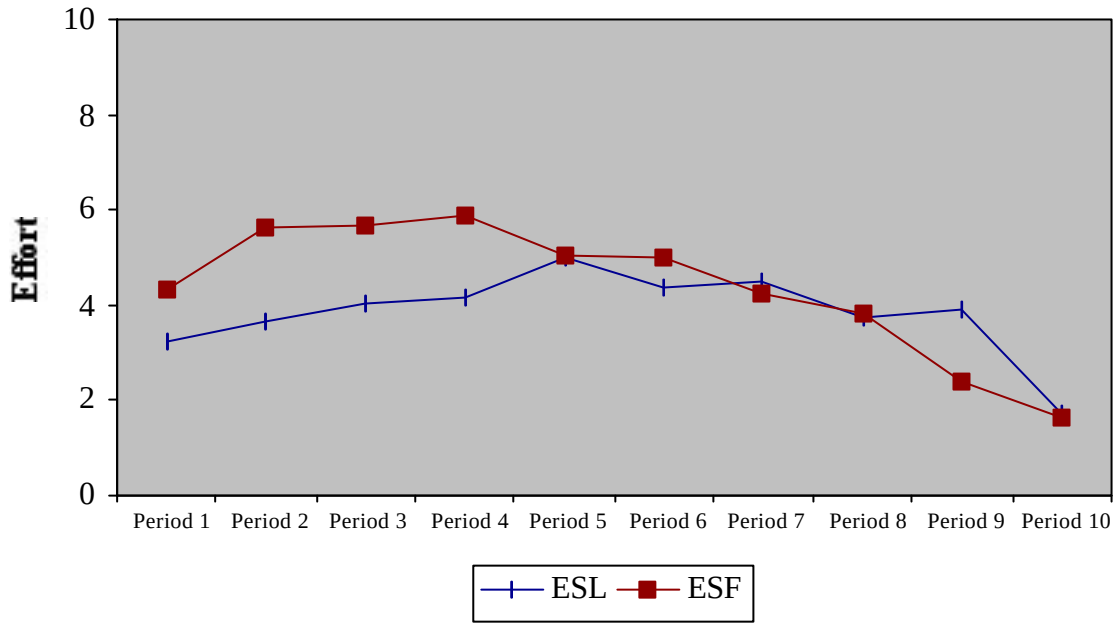
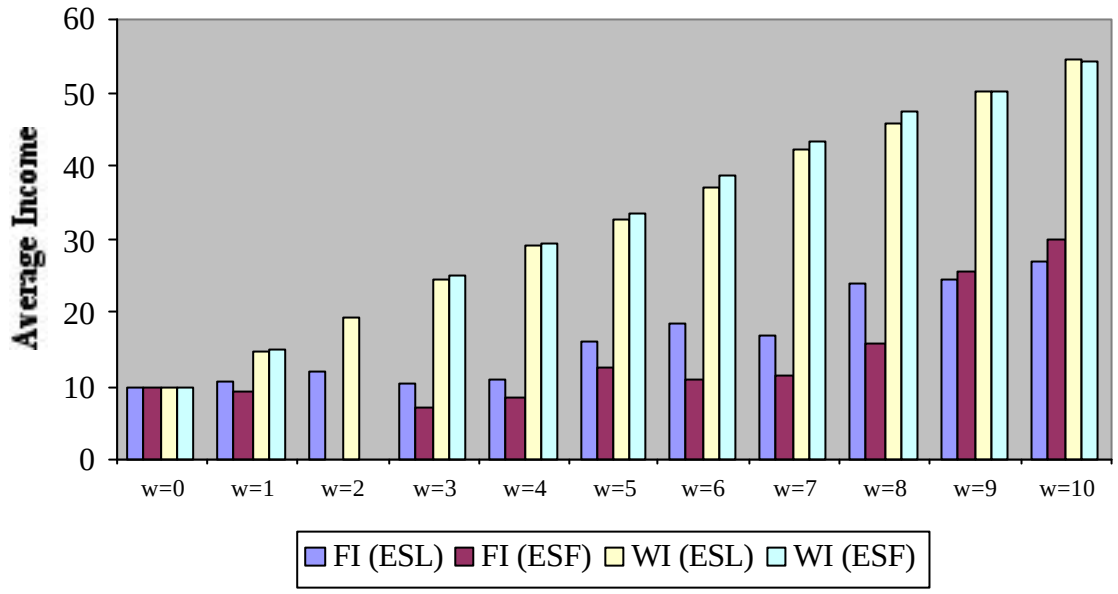


Figure VI: Average Firm and Worker Income per Wage Level



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

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 accepted 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 accepted 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.