Does Money Illusion Matter?

An Experimental Approach

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Abstract

Money illusion means that people behave differently when the same objective situation is represented in nominal or in real terms. To examine the behavioral impact of money illusion we studied the adjustment process of nominal prices after a fully anticipated negative nominal shock in an experimental setting with strategic complementarity. We show that seemingly innocuous differences in payoff presentation cause large behavioral differences. In particular, *if the payoff information is presented to subjects in nominal terms, price stickiness and real effects are much more pronounced than when payoff information is presented in real terms. The driving force of differences in real outcomes is subjects' expectation of higher nominal inertia in the nominal payoff condition.* Due to strategic complementarity, these expectations induce subjects to adjust rather slowly to the shock.

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1. Introduction

The notion of money illusion seems to be thorougly discredited in modern economics. Tobin (1972), for example, described the negative attitude of most economic theorists towards money illusion as follows: “An economic theorist can, of course, commit no greater crime than to assume money illusion.“ As a consequence, money illusion is anathema in the profession. For example, that the Index of the Handbook of Monetary Economics (Friedman and Hahn 1990) does not even mention the term “money illusion“. In principle, money illusion could provide an explanation for the inertia of nominal prices and wages and, thus, for the non-neutrality of money. The stickiness of *nominal* prices and wages seems to be an important phenomenon (see e.g. Akerlof, Dickens and Perry 1996, Bernanke and Carey 1996, Kahn 1996) and has puzzled economists for decades because it is quite difficult to explain in an equilibrium model with maximizing individuals. Instead of money illusion other factors like informational frictions (Lucas 1972), staggering of contracts (e.g., Fischer 1977, Taylor 1979), costs of price adjustment (Mankiw 1985) and near-rationality (Akerlof and Yellen 1985) have been invoked to explain nominal inertia.

In this paper we do not contest the potential relevance of these explanations. We do, however, argue that money illusion has been dismissed prematurely as a potential candidate for the explanation of sluggish nominal price adjustment. Our argument is based on theoretical considerations and on experimental evidence. At the theoretical level we will argue that in order to rule out the relevance of money illusion it is not sufficient that individuals are illusion-free but that the absence of money illusion is common knowledge. Yet, in our view, it seems highly unlikely that this common knowledge requirement is met in practice. At the empirical level we will show that, after a fully anticipated negative nominal shock, nominal inertia is the rule rather than the exception. Substantial nominal inertia arises even if informational frictions, costs of price adjustment and staggering are absent.

Money illusion means that behavior depends on whether the same objective situation is framed in nominal or in real terms. A particularly transparent example of money illusion is the case where people behave differently when they receive payoff information in real or in nominal terms. In fact, almost all business transactions involve nominal payoff information. To detect this kind of money illusion it would, thus, be necessary to find situations in which a real frame, e.g., only real payoff information, prevails. By comparing people’s behavior under the nominal and the real frame one could isolate the behavioral impact of money illusion. Unfortunately, business life does not seem to provide examples in which the same objective situation is sometimes represented in

nominal terms and sometimes in real terms. This is one important reason why we rely in our empirical examination on experimental methods.[3] In the present context a major advantage of experimental methods is that the "frame" is under the control of the experimenter. Accordingly, we implemented a treatment condition in which payoffs were represented in nominal terms and a control condition in which payoffs were represented in real terms. It turns out that this seemingly innocuous difference in payoff presentation causes large differences in behavior. In particular, nominal inertia is much more pronounced in the nominal payoff condition. This behavioral difference occurs although subjects in the nominal condition have been trained how to compute real payoffs from the nominally given payoffs. Under the nominal payoff representation it takes roughly twice as long for the economy to reach the "new"[4], post-shock-equilibrium. In addition, real income losses are approximately twice as large in the nominal condition. This indicates that the "veil of money", that is, the veil generated by the nominal representation of payoffs, has behavioral effects with large real consequences after an anticipated nominal shock.

The major cause for the slow price adjustment is the inertia of subjects' expectations about the prices set by others. In all experimental conditions subjects do not expect that the other subjects immediately jump to the 'new' equilibrium. Therefore, subjects do, in general, have no incentive to play the post-shock equilibrium immediately after the shock. Moreover, and perhaps more importantly, the stickiness of price expectations is much larger in the nominal payoff condition. Thus subjects expect much less adjustment by others in this condition after the shock. These differences in expectations suggest that money illusion matters because the absence of money illusion is not common knowledge and less so, because subjects themselves cannot pierce the veil of money.

The rest of the paper is organized in the following way. In section 2 we discuss the notion of money illusion and its potential aggregate implications in more detail. In section 3 we argue that experimental methods are an appropriate tool for the study of money illusion and outline our experimental design. In section 4 the experimental results are presented. In the final section we summarize and interpret our main results.

3. Section 3 provides further reasons for the application of experimental methods to the problem of money illusion.
4. In fact, it is the same equilibrium in real terms.

2. Money illusion, disequilibrium and strategic complementarity

2.1. Money illusion at the individual level

The term “money illusion“ has been used differently by various authors although the intuition on which the term is based seems to be rather similar.[5] Leontief (1936), for example, defined money illusion as a violation of the “homogeneity postulate“. This postulate stipulates that demand and supply functions are homogeneous of degree zero in all nominal prices, i. e., they depend only on *relative* and not on absolute prices. Patinkin (1949) used a slightly different definition that also takes into account the potential effect of people’s *real* wealth on their supply and demand behavior. According to Patinkin money illusion is absent if individuals’ net demand functions are homogeneous of degree zero in all money prices *and* real wealth.

Although the definition of Patinkin differs from Leontief’s by taking into account the “wealth constraint“, both definitions are, in our view, based on the same intuition. This intuition says that if the *real* incentive structure, i.e. the *objective* situation an individual faces, remains unchanged, the *real* decisions of an illusion-free individual do not change either. Two crucial assumptions underly this intuition: First, the objective function of the individual does not depend on nominal but only on real magnitudes. Second, people perceive that purely nominal changes do not affect their opportunity set. For example, people have to understand that an equiproportionate change in all nominal magnitudes leaves the real constraints unaffected. Whether people are indeed able to pierce the veil of money and to understand that purely nominal changes leave their objective circumstances unchanged is, in principle, an empirical question. Irving Fisher (1928: 4), for example, was convinced that ordinary people, in general, fail “to perceive that the dollar, or any other unit of money expands or shrinks in value“ after a monetary shock.

More recently Shafir, Tversky and Diamond (henceforth STD, 1997) provided evidence that indicates that frequently one or both preconditions for the absence of money illusion are violated. Their results suggest that people’s preferences as well as their perceptions of the constraints are affected by nominal values. Moreover, many people do not only seem to suffer from money illusion; they also expect that other people’s preferences and decisions are affected by money illusion. Problem 1 of STD’s questionnaire study neatly illustrates these claims. STD presented the following scenario to two groups of respondents:

5. For the different definitions of money illusion see Howitt (1989).

> *Consider two individuals, Ann and Barbara, who graduated from the same college a year apart. Upon graduation, both took similar jobs with publishing firms. Ann started with a yearly salary of $ 30,000. During her first year on the job there was no inflation, and in her second year Ann received a 2% ($ 600) raise in salary. Barbara also started with a yearly salary of $ 30,000. During her first year on the job there was a 4 % inflation, and in her second year Barbara received a 5% ($ 1500) raise in salary.*

Respondents of group 1 were then asked the happiness question: *"As Ann and Barbara entered their second year on the job, who do you think was happier?"* 36 percent thought that Ann was happier while 64 percent believed that Barbara was happier. This indicates that most subjects believed that preferences are affected by nominal variables because in real terms Ann does of course better.[6] Respondents of group 2 were asked the following question: *"As they entered their second year on the job, each received a job offer from another firm. Who do you think was more likely to leave the present position for another job?"* In line with the response to the happiness question 65 percent believed that Ann, which is doing better in economic terms, is more likely to leave the present job. Thus, a majority believed that other people's decisions are affected by money illusion.

Since the absence of money illusion means that an individual's preferences, perceptions and, hence, choices of real magnitudes are not affected by purely nominal changes it is natural to view money illusion as a framing or representation effect. From this viewpoint an individual exhibits money illusion if the preferences or the perceptions of constraints and the associated decisions depend on whether the same environment is represented in nominal or real terms. STD's analysis is based on a large body of research in cognitive psychology that shows that alternative representations of the same situation may well lead to systematically different responses (Tversky and Kahneman 1981, 1986).[7] Representation effects seem to arise because people tend to adopt the particular frame that is presented and evaluate the options within this frame. Because some options loom larger in one representation than in another, alternative framings of the same options can give rise to different choices.

STD argue that people tend to have multiple representations but that the nominal representation is often *simpler and more salient*. They suggest that people are generally aware of the difference between nominal and real values, but because money is a salient and *natural unit*, people often think of transactions in predominantly nominal terms. Thinking in terms of relative prices does not seem natural to many people. For example, most people tend to say "this beer has a price

6. There was a third group of respondents who was asked whether Ann or Barbara are doing better in economic terms. 71 percent answered that Ann is in fact doing better in economic terms.
7. For an early demonstration of a framing or representation effect in experimental economics see Selten and Berg (1970).

of 2$, and this steak a price of 7$“, but only few people would state “this steak has a price of 3.5 beers“.

2.2. Money illusion as a disequilibrating force

In the past economists frequently used the assumption of money illusion to account for the short-run non-neutrality of money. Irving Fisher’s explanation of business cycles is, for example, based on lenders’ money illusion during an upswing.[8] However, since the success of the rational expectations revolution an extreme reluctance to invoke money illusion as an explanation of the short-run non-neutrality of money has been established. While New Classical macroeconomists focus on informational frictions to account for short-run non-neutrality (Lucas 1972), New Keynesians mainly focus on costs of price adjustment or staggering (see e. g. Mankiw and Romer 1991).[9] In the absence of menu costs, staggering, and informational frictions, the models of New Keynesian and New Classical economists rule out that purely monetary changes have real effects. A common feature of these models is that they *exclusively* focus on the equilibrium states of their economies. In general, they remain silent on how economic agents move from one equilibrium to the other. In models that exclusively focus on equilibrium the assumption of the absence of money illusion is very intuitive because it is difficult to imagine that an illusion could persist in equilibrium.

However, as we will argue next, there is a strong a priori argument that money illusion is likely to affect the *adjustment process* of an economy after a fully anticipated monetary shock. Our argument is based on the simple fact that a Nash equilibrium involves the coordination of expectations. This can be illustrated in the context of a monopolistically competitive economy as analysed in, for example, Akerlof and Yellen (1985) or Blanchard and Kiyotaki (1987). To keep the argument simple we focus only on firms’ behavior. The reduced form real profit function for firms in these models can be written as

$$\pi_i = \pi_i(P_i/P, M/P) \tag{1}$$

where π_i is firm *i’s* real profit, P_i is the nominal price set by firm *i*, P is the aggregate price level and M denotes the supply of money.[10] In these models M/P is proportional to real aggre-

8. Fisher believed that lenders are willing to supply more in the face of a rise in nominal interest rates although real interest rates decline or remain unchanged due to inflation.
9. The near-rationality approach of Akerlof and Yellen (1985) can, in principle, be subsumed under the menu-cost approach by stipulating “cognitive“ menu costs of maximizing behavior.

gate demand. For simplicity, we assume identical firms and a unique symmetric equilibrium $P_i^* = P_j^*$, for all i, j. In this equilibrium each firm maximizes its real profits by setting $P_i^* = \bar{P}^*$. Since (1) is homogeneous of degree zero in P_i, P and M it is obvious that a change in M to λM, $(\lambda \neq 1)$, leads to post shock equilibrium values of λP_i^* and $\lambda \bar{P}^*$.

Suppose now that there are agents who believe that there are other agents who suffer from money illusion and do not fully adjust their nominal prices to λP_i. The first group of agents, therefore, anticipates a change in real aggregate demand M/P so that their members, in general, have an incentive to choose a price that differs from λP_i^*. For this conclusion to hold, it is not even necessary that there are indeed firms which believe that others suffer from money illusion. Suppose, for example, that there is one group of firms, which believes that a second group of firms believes, that there is a third group which suffers from money illusion and does, hence, not adjust fully. This means that the first group believes that the second group does not choose the equilibrium price λP_i^* and, hence, the first group also has an incentive to choose a price which differs from the equilibrium price. The basic message of this argument is, thus, that unless the *absence of money illusion is common knowledge*, there will, in general be no coordinated instantaneous adjustment to λP_i^*. As a consequence, the economy will go through a process of disequilibrium.

2.3. Strategic complementarity and the aggregate impact of money illusion

Considering the evidence of STD it seems rather likely that the absence of money illusion is not common knowledge. However, the disequilibrating force of money illusion does not per se provide a reason for aggregate nominal inertia. It also is not clear whether the disequilibrium is associated with economically significant welfare effects. At this point of the argument strategic complementarity becomes important. Haltiwanger and Waldman (1989) have shown that in the presence of strategic complementarity between agents' decisions the existence of a *small* group of nonrational subjects can have *large* effects on the process of adjustment to equilibrium.[11] In the above mentioned model of monopolistic competition strategic complementarity means that firm *i's* profit maximizing nominal price P_i' is *positively* related to the aggregate price level P. This

10. Equation (1) already incorporates (*i*) the maximizing behavior of all households, (*ii*) the cost minimizing behavior of all firms for given output and wages levels, (*iii*) the equilibrium real wage, and (*iv*) the equilibrium relation between real aggregate demand and real money balances. In Akerlof and Yellen (1985) the real wage is given by the Solow condition because firms are efficiency wage setters. In Blanchard and Kiyotaki (1987) housholds are wage setters so that firms take real wages as given when choosing nominal prices and output.
11. The papers by Haltiwanger and Waldman (1985) and Russell and Thaler (1985) examine a related point. They analyze the conditions under which nonrational subjects affect the equilibrium. In contrast, Haltiwanger and Waldman (1989) examine the conditions under which nonrational subjects affect the *adjustment* to equilibrium.

means that firms which believe that other agents' money illusion keeps P close to its pre-shock equilibrium level have a rational reason to choose a nominal price that is close to the pre-shock equilibrium level. Likewise, firms which believe that other firms believe that there is money illusion in the economy will also have a rational reason to stay close to the pre-shock price level, and so forth.

Under strategic complementarity rational firms have an incentive to partly *imitate* the behavior of the nonrational ones which gives the latter a *disproportionately large impact on the aggregate price level*. In contrast, in the presence of strategic substitutability, i. e., if P_i' is negatively related to P, rational firms have an incentive to partly *compensate* the behavior of the nonrational ones so that the latter have a *disproportionately small impact* on the aggregate. The results of Haltiwanger and Waldmann (1989) thus suggest that, given strategic complementarity, expectations about the existence of a small group of subjects that suffer from money illusion may generate substantial nominal inertia.

The essence of the above argument is that a rational player has to base his choice on his forecast of other players' actions. These actions in turn depend on the forecasts of the behavior of other players, and so on. Therefore, a rational player has to make forecasts about the forecasts of others, etc. If at one stage in this chain of reasoning there is the belief that money illusion gives rise to less than full adjustment, rational players have an incentive to adjust less than fully, too. The situation is similar to Keynes' famous "beauty contest"-analogy for professional investors. Keynes argued that to make profit in the stock market it is crucial to know the average opinion of the other investors which in turn depends on their estimates of the average opinion, and so on.[12] Keynes argument implies that in the absence of common knowledge of rationality rational investors may have a reason to trade at prices that deviate from fundamental asset values.[13]

Since strategic complementarity is important for our argument in favor of the behavioral relevance of (beliefs about) money illusion one would like to know to what extent it does prevail in naturally occurring economies. There are several papers that suggest that strategic complementa-

12. "... professional investment may be linkened to those newspaper competitions in which the competitors have to pick out the six prettiest faces from a hundred photographs, the prize being awarded to the competitor whose choice most nearly corresponds to the average preferences of the competitors as a whole: so that each competitor has to pick, not those faces which he himself finds prettiest, but those which he thinks likliest to catch the fancy of the other competitors, all of whom are looking at the same problem from the same point of view. It is not the case of choosing those which, to the best of one's judgment, are really the prettiest, nor even those which average opinion genuinely thinks the prettiest. We have reached the third degree where we devote our intelligences to anticipating what average opinion expects the average opinion to be. And there are some, I believe, who practise the fourth, fifth and higher degrees." (Keynes, 1936: 156).
13. A beautiful piece of evidence supporting this argument is provided by Smith, Suchanek and Williams (1988). The absence of common knowledge of rationality also is supported by the so-called beauty-contest games conducted by Nagel (1995) and Ho, Weigelt and Camerer (1996).

rity may well be an important feature of naturally occuring environments. It arises naturally in imperfectly competitive labor and product markets. It has been shown to prevail in the context of monopolistic competition (Ball and Romer 1987), it can arise from the nature of preferences and technologies (Bryant 1983) or in environments in which heterogeneous agents search for transaction partners (Diamond 1982). Oh and Waldman (1994) as well as Cooper and Haltiwanger (1993) provide evidence in favor of the relevance of strategic complementarity in naturally occuring economies.

3. An experimental approach to money illusion

One way to rigorously examine the validity of the above explanation of nominal inertia, and the associated real effects of money, is to look for a natural experiment in which an *exogenous* and *fully anticipated* monetary shock occurs. The shock has to be exogenous because if the central bank responds to real events in the economy there can be a comovement between money and output that has nothing to do with the real effects of money. The shock has to be fully anticipated because nobody doubts that nominal inertia and real effects occur in the presence of non-anticipated shocks. The effects of nonanticipated shocks should not be confounded with money illusion.

Of course, in order to unambigously identify whether the shock is fully anticipated the researcher needs to know *individual information sets* before the shock. Moreover, to judge whether the anticipated shock causes a disequilibrium and nominal inertia the researcher has to know the equilibrium values of nominal prices *before* and *after* the shock. By comparing the pre- and post shock equilibrium values of nominal prices with actual prices the researcher can identify (*i)* to what extent actual prices are anchored at the pre-shock equilibrium and (*ii*) how long it takes for actual prices to adjust to the new equilibrium. Finally, to examine whether money illusion is responsible for the existence of nominal inertia, if it occurs, the researcher should identify two similar natural experiments as described above. In one experiment the “world“ should be framed in nominal terms while in the other experiment it should be framed in real terms.

In our view, it seems extremely difficult, if not impossible, to meet the above requirements with field data. In fact, the exogeneity of monetary policy and the causality between money and output is a matter of considerable debate (e.g., Romer and Romer 1989, 1994; Hoover and Perez 1994; Coleman 1996). Judgements about whether monetary shocks are anticipated or not are usually controversial, too. Belongia (1996) for example, shows that the measurement of unantici-

pated money shocks may be quite sensitive to the choice of monetary aggregates.

Moreover, full knowledge of the pre- and post-shock equilibrium values of nominal prices is clearly beyond the information content of presently available data. Finally, as already mentioned in the introduction, almost all business transactions are shrouded in nominal money, i.e., it is very difficult to find real world examples of a real frame.

In an appropriate laboratory setting, however, the above mentioned data requirements can be met. The techniques of experimental economics allow the implementation of exogenous and fully anticipated nominal shocks and the experimenter can exert full control over pre- and post-shock equilibrium values of nominal prices. In addition, the experimenter controls the framing of the situation, e.g., whether subjects receive the payoff information in nominal or in real terms. These enhanced control opportunities suggest that laboratory experiments provide valuable information for the study of money illusion and nominal inertia which complements and helps to interpret the results of studies based on field data.

3.1. General description of the experimental design

To study the impact of money illusion we designed an *n*-player pricing game with strategic complementarity. The pricing game was divided into a pre-shock and a post-shock phase. At the beginning of the post-shock phase we implemented an exogenous and anticipated nominal shock. There were three treatment conditions that differed only with regard to the representation of payoffs (see Table 1 below). Therefore, in all three treatment conditions the real underlying structure of the game was *identical*. This means, for example, that the best reply functions were identical across treatments. As a consequence, in each treatment equilibrium prevailed at the same nominal and real prices.

Since our treatment conditions differ exclusively with respect to the representation of the same underlying game we can observe the aggregate effects of money illusion by comparing the behavior across treatments. There was a so-called Nominal Treatment (NT), a Semi-real Treatment (SRT) and a Real Treatment (RT). The Nominal treatment captures two important real world features: (*i*) Subjects received the payoff information in *nominal* terms. To compute their real payoffs they had to divide (‘deflate’) nominal payoffs by the prevailing aggregate price level. (*ii*) There was a *constant* smallest nominal accounting unit. Both features are obviously present in naturally occuring economies. We do not get paid, say, 10.000 steaks or so and so much units of the average commodity basket; we are, in general, paid in nominal money and most of our economic transac-

tions directly involve nominal money. Similarly, all modern societies have a legally enforced smallest *nominal* accounting unit that does not depend on the money supply or the general price level.

* Insert ***Table 1*** about here *

The SRT is a control treatment for the NT and differs from the latter only in one respect: Subjects' received the payoff information in *real* terms. The second feature of the NT, the constant smallest nominal accounting unit, was also present in the SRT. Note that if it is common knowledge that all subjects are capable of computing real payoffs from nominal payoffs one should observe no behavioral differences between the Nominal and the Semi-real treatment. Yet, if some agents in the NT confuse or believe that others confuse, etc., nominal with real payoffs, behavioral differences across treatments may arise.

The RT differs from the SRT also only in one respect: Instead of a constant smallest *nominal* accounting unit there was a constant smallest *real* accounting unit. This means that a change in the money supply was accompanied by a change in the smallest nominal accounting unit to keep the real acccounting unit constant. In both the SRT and the RT payoff information was given in real terms. By comparing the SRT with the RT we can examine the behavioral relevance of Fisher's (1928: 4) conjecture, that people fail "to perceive that the dollar or any other unit of money, expands or shrinks in value". If Fisher is right, adjustment in the RT should be significantly quikker because the experimenter has removed any source of money illusion. In the RT it is completely transparent that nothing has changed in real terms after the implementation of the nominal shock.[14]

Each treatment condition had 40 periods. During the first 20 periods of a session the money supply was given by M_0 . Then we implemented a fully anticipated monetary shock by reducing the money supply to M_1. This shock and the fact that the post-shock phase lasts again 20 periods was common knowledge. Our major interest concerns subjects' pricing behavior and the associated real effects in the post-shock phase. The pre-shock phase serves the purpose to make subjects acquainted with the computer terminal and the decision environment. In addition, and more importantly, the pre-shock phase allows us to see whether subjects will reach an equilibrium in each of the three treatment conditions. After all, one can only argue that money illusion is a disequilibrating force if equilibrium has in fact been reached before the shock.

14. For a more detailed discussion of payoff representation see section 3.2.

Each subject of an experimental session belonged to a group of n players. The group composition remained unchanged for all 40 periods. The real payoff of agent i was given by

$$\pi_i = \pi_i(P_i, P_{-i}, M) \qquad i = 1, ..., n \tag{2}$$

where P_i denotes *i's* nominal price, P_{-i} represents the average price of the *other n-1* group members while M denotes a nominal shock variable (money supply). The payoff functions (2) have the following properties:

(*i*) They are homogeneous of degree zero in P_i, P_{-i} and M.

(*ii*) There is a unique best reply for any P_{-i}.

(*iii*) The best reply is (weakly) increasing in P_{-i}.

In addition our functional specification[15] of (2) implies that the Nash equilibrium

(*iv*) is unique for every M,

(*v*) is the only Pareto efficient point in payoff space, and

(*vi*) can be found by iterated elimination of weakly dominated strategies.

Note that π_i does not depend on the average price P of all group members but on P_{-i}. This feature makes it particularly easy to play best reply for a given expectation about the other players' average price. If we made π_i dependent on P, so that P_i affects P, it would have been much more difficult for i to compute the best reply (see also section 3.2. below)

Property (*i*) was implemented because our analysis focuses on the impact of money illusion on the adjustment process of an economy with money-neutral (real) equilibria. To see that property (*i*) implies neutrality note that a change in M from M_0 to $\lambda M_0 = M_1$ leaves real payoffs unaffected if prices change to λP_i and $\lambda \bar{P}_{-i}$. Moreover, if P_i, $i = 1, ..., n$, is a best reply to P_{-i} at M_0, λP_i also is a best reply to $\lambda \bar{P}_{-i}$ at λM_0. Thus, λP_i^* for all i is the post-shock equilibrium.

Property (*ii*) was chosen because it is likely to speed up adjustment to the equilibrium. At the end of each period each player was informed about the realization of P_{-i}. Since i knew that all the other players had unique best replies the realization of P_{-i} was more informative.[16] Property (*iii*) captures strategic complementarity and was implemented for the reasons given in section 2.3. In principle, money illusion can have a permanent real effect on an economy with multiple equilibria, if – by affecting disequilibrium dynamics – it has an impact on equilibrium selection. Yet, since it

15. The functional form is presented in Appendix A.

16. If the other n -1 players have multiple best replies and choose more or less randomly among them, P_{-i} exhibits (ceteris paribus) more randomness compared to a situation with unique best replies.

is better to study the simple problems before the more difficult ones' we implemented a unique equilibrium (property (*iv*)). Property (*v*) was implemented to rule out that both money illusion and attempts to achieve out-of-equilibrium gains through cooperation cause deviations from equilibrium.[17]It is worthwhile to point out that – in the presence of attempts at achieving cooperation – property (*v*) is likely to speed up equilibrium adjustment. Since cooperation attempts may compensate the decrease in adjustment speed that is due to money illusion, property (*v*) renders it more difficult to detect the impact of money illusion. However, if we can observe that money illusion causes nominal inertia despite the potential countervailing force of cooperation we have an even stronger result.

Finally, property (*vi*) is likely to increase adjustment speed because it increases the chances that subjects find the equilibrium: The more methods are available for finding the equilibrium, the higher the chances that it will be found.

3.2. Experimental procedures and parameters

All major experimental parameters and design features are summarized in *Table 1*.[18] The experiment was conducted in a computerized laboratory with a group size of $n = 4$ subjects. In each group there were two types of subjects: Subjects of type *x* and subjects of type *y*. The payoff function differed among the types. This difference implied that *x*-types had to choose a relatively low price in equilibrium while *y*-types had to choose a relatively high price (see *Table 1* for details). In the pre-shock phase of each treatment the money supply was given by $M_0 = 42$ while in the post-shock phase it was given by $M_1 = M_0 / 3 = 14$. In the pre-shock equilibrium the average price over all *n* group members is given by $\bar{P}_0^* = 18$ while in the post-shock equilibrium it is $\bar{P}_1^* = 6$.

Except for the pre-shock phase of the RT subjects had to choose an integer $P_i \in \{1, 2, \ldots, 30\}$ in each decision period.[19] In addition, they had to provide an expectation about P_{-i} which we denote by $\bar{P}_{-i}^e$. Finally, subjects indicated their confidence about their expectation $\bar{P}_{-i}^e$ by choosing an integer from 1 to 6 where 1 indicated that the subject is "not at all confident" while 6 indi-

17. In our pilot experiments we implemented a price-setting game with monopolistic competition. However, it turned out that subjects quickly realize that there are out-of-equilibrium cooperative gains to be made. It is well known from many public good experiments (see e. g. Ledyard 1995) that the adjustment process to Nash equilibria with free-riding is severly retarded, if not prevented, by subjects' attempts to achieve cooperative out-of-equilibrium gains. In our pilot experiments we had to learn this lesson once more. In the pre- as well as the post-shock phase equilibrium adjustment was strongly retarded by cooperation attempts.
18. The instructions are included in the Appendix B.

cated that he or she is "absolutely confident". This measure of confidence can be interpreted as an indicator of subjects' perception of the variability of P_{-i}. At the end of each period each subject was informed about the actual realization of P_{-i} and the actual real payoff π_i on a so-called "outcome screen" (see *Figure 2* in Appendix B). In addition, the outcome screen provided information about the subject's past choices of P_i, past realizations of P_{-i} and past real payoffs π_i.

Subjects received the payoff information in matrix form. In Appendix C we provide the payoff matrices of *y*-types for all treatment conditions. The payoff matrix shows the real or the nominal payoff, respectively, for each feasible integer combination of (P_i, P_{-i}). Since subjects' choice sets contained 30 elements the payoff matrix had a 30 x 30 dimension. To inform subjects about the payoffs of the other type, each subject also received the payoff matrix of the other type. This information condition was common knowledge. The presentation of payoffs in the form of a matrix made it particularly easy to find the best reply for any given P_{-i}: The subject just had to look for the highest real or nominal payoff in the column associated with P_{-i}.[20]

At the end of period 20 the nominal shock was implemented in the following way: Subjects were publicly informed that *x*- and *y*-types will receive new payoff tables. These tables were based on $M_1 = M_0 / 3$. Again each type received the payoff table for his own and the other type. Subjects were told that, except for payoff tables everything else remained unchanged. They were given enough time to study the new payoff tables and to choose P_i for period 21.[21] This procedure ensures that in period 21 subjects face an exogenous and fully anticipated negative nominal shock. At the beginning of period 21 it was common knowledge that the experiment will last for further 20 periods.

Before we present the experimental results it is worthwhile to emphasize that the only difference between the NT and the SRT concerns the payoff information. While in the NT the entries in the payoff table represent nominal payoffs $P_{-i}\pi_i$, the entries in the SRT represent π_i (compare Tables T1 and T2 with Tables T3 and T4 in Appendix C). Thus to compute the real payoff for a particular (P_i, P_{-i})-combination in the NT a subject just had to divide $P_{-i}\pi_i$ by P_{-i}. This was described at length in the instructions (see Appendix B). In addition, before the first decision

19. In the pre-shock phase of the RT subjects had to choose a price from the set $\{3, 6, \ldots, 90\}$. This means that the smallest *nominal* accounting unit was 3 in this phase. This is necessary to keep the smallest *real* accounting unit constant between the pre- and the post-shock phase of the RT: At a money supply of M_0 a change in P_i by $\Delta P_i = 3$ has the same effect on the real payoff as a change of $\Delta P_i = 1$ when the money supply is $M_1 = M_0/3$. Note that in the post-shock phase the set of nominal choice variables is given by $\{1, 2, \ldots, 30\}$ *in each treatment*.
20. The best replies are shaded in Appendix C, whereas the payoff table given to subjects did not contain this information.
21. In total subjects were given 10 minutes to study the new payoff tables and to make a decision in period 21. Yet, almost all subjects made their decision several minutes before time had elapsed.

period subjects had to solve several exercises that involved the computation of real payoffs from the nominal payoff table. The first decision period started only after everybody had solved the exercises correctly. In fact, every subject was able to solve the exercises.

We emphasize this procedure because it makes it highly implausible that subjects suffered from money illusion in the NT. However, as our discussion in section 2 shows, the absence of money illusion is not sufficient to rule out that money illusion matters. Instead, the absence of money illusion has to be common knowledge.

Note also that in the post-shock phase subjects face *exactly the same* payoff table in the SRT and the RT (compare Table T4 with Table T6 in Appendix C). This follows necessarily from the requirement that in both treatments payoffs were given in real terms so that the only difference was whether the nominal or the real accounting unit was held constant across *M*-levels. Since in the RT the *real* accounting unit was held constant between the pre- and the post-shock phase it becomes almost abvious that nothing *real* has changed after the shock (compare Table T5 with Table T6). This is less obvious in the SRT (compare Table T3 with T4). Since in the RT the experimenter has, so to speak, removed the veil of money, it is more likely that subjects understand that nothing real has changed *and* that this is common knowledge. In contrast, in the other two treatment conditions subjects themselves have to pierce the veil of money, and have to form beliefs about whether other subjects are capable of doing so, and so forth. Therefore, the NT and the SRT capture elements of the ‘monetary veil in which most business transactions are shrouded’.

4. Results

In total, 124 subjects participated in our experiment. 11 groups of four players participated in the Nominal treatment (NT), 10 groups in the Semi-real treatment (SRT) and another 10 groups in the Real treatment (RT). Subjects were undergraduate students from different disciplines. They were paid a show-up fee of $12 and their earnings from the experiment were on average $28. An experimental session lasted, on average, 90 minutes.

4.1 Is there short-run nominal inertia?

Many, if not most, economists believe that money is non-neutral in the short run (Taylor, 1997). In addition, it seems that most economists are rather reluctant to invoke money illusion as an explanation of the short-run non-neutrality. Instead, informational frictions, price adjustment costs or staggering are put forward as explanations. Our experimental design allows for a rigorous test of the short-run effects of an anticipated monetary shock. Since we control the pre- and post-shock levels of nominal equilibrium prices, and since we can unambiguously observe actual prices, the experiment provides precise information about nominal inertia. In addition, in case that nominal inertia prevails we can rule out staggering, costs of price adjustment and informational frictions as potential explanations of nominal inertia. Moreover, by comparing the adjustment of prices across treatment conditions it is possible to isolate the aggregate effect of nominal representations, i.e., of money illusion, on the adjustment process. Our main result with regard to nominal price adjustment is stated in Result 1:

R1: *In each treatment there is some nominal inertia in the early periods of the post-shock phase. However, nominal inertia is much more pronounced and lasts much longer in the Nominal treatment than in the other two treatment conditions.*

* Insert ***Figure 1*** about here *

Figure 1 provides an illustration of the adjustment process in all conditions. It depicts the evolution of the average price P of the median group in each treatment shortly before and after the shock.[22] In *Table 2* we also present the numerical values of the evolution of median and average

prices over time. Since average and median prices are, in general, close to each other we do not lose much information by concentrating on median prices. *Figure 1* shows that the median group was perfectly in equilibrium ($\bar{P}_0^* = 18$) in each treatment before the shock while post-shock prices are strikingly different across treatments. In the RT we observe that the nominal price is relatively close the equilibrium value of $\bar{P}_1^* = 6$ already in period 21 and that within 4 periods the equilibrium is reached. In the SRT it takes 6 periods until equilibrium is reached while in the NT 13 periods are needed for full adjustment. The adjustment difference is particularly large in period 21. The immediate adjustment of nominal prices in the NT is approximately only 50% of the adjustment in the other two conditions.

* Insert ***Table 2*** about here *

Table 3 provides additional information about the frequencies of equilibrium play after the shock. In the NT we observe that almost all observations (93%) are above the equilibrium and only very few (7%) are in equilibrium in the first five periods following the shock. In periods 26-30 the majority of the observations (58%) is still above the equilibrium while in the other two conditions already 70% or 82%, respectively, of the price observations are in equilibrium. *Table 3* illustrates that convergence is from above since the mass of observations was above equilibrium in the early post-shock periods and this mass is shifted to the equilibrium with differing speeds across treatments. Note how few observations below equilibrium are observed.

To test more formally for behavioral differences across treatments we conducted further non-parametric tests. A pairwise comparison of average prices by means of a Kruskal-Wallis-Test reveals that the average group prices in the NT are different from those in the Semi-real ($p < 0.01$ for t = 21-25, $p < 0.05$ for t = 26-30) and the Real treatment ($p < 0.01$ for t = 21-25, $p < 0.05$ for t = 26-30) in the first ten periods of the post-shock phase. In contrast, average group prices in the Semi-real and the Real treatment are not significantly different ($p = 0.18$ for t = 21-25, $p = 0.68$ for t = 26-30).

* Insert ***Table 3*** about here *

22. Note that the median group may change from period to period.

4.2 The short-run non-neutrality of money

So far we discussed only the evolution of nominal prices. Our results raise the question whether nominal inertia causes economically significant effects on real income. This leads directly to

R2: *Nominal inertia is associated with large real income losses. Income losses are considerably larger in the Nominal treatment than in the Semi-real treatment. They are lowest in the Real treatment.*

To check the support for R2 we calculate by how much actual real income of group *j*, π_j, falls short of real income in equilibrium π^*. For this purpose we have computed $\varepsilon_{jt} = (\pi_{jt} - \pi^*)/\pi^*$ for each group *j* in each period *t*. ε_{jt} is a measure of the income loss relative to the equilibrium payoff as a percentage of the equilibrium payoff. Since the equilibrium is efficient it is also a measure of the efficiency loss. *Figure 2* presents the evolution of ε_{jt} for the median group in each treatment. In addition, *Table 2* presents the evolution of the average value of ε_{jt} over all groups in each treatment.

* Insert ***Figure 2*** about here *

Figure 2 and *Table 2* indicate that shortly before the shock the efficiency losses are small in all treatments. However, the monetary shock leads to substantial efficiency losses in each treatment. Real income of the average group declines by 32% in the RT, 52% in the SRT and 65% in the NT. This ranking of efficiency losses across treatments remains stable over time. In line with the results on nominal inertia the efficiency losses of the median group are zero after period 25 in the RT and after period 26 in the SRT while considerable losses still occur between period 25 and 30 in the NT (see *Figure 2*). In period 30, for example, the median group in the NT still experiences an efficiency loss of more than 15%. If we aggregate the efficiency losses over *t* = 21-30, and compare them across treatments, we find that aggregate losses in the NT are almost twice as large as those in the SRT and almost four times as large as those in the RT.[23] Such large differences in efficiency losses across treatments indicate that money illusion matters, i.e. has a large economic impact.

23. To be precise: In total, groups in the NT lose 26% of the potential payoff in periods *t* = 21-30. Aggregate efficiency losses in the RT are 27% of the losses in the NT. In the SRT the losses are 55% of the losses in the NT.

4.3 Long-run adjustment to equilibrium?

One of the core propositions of modern macroeconomics is that in the long run money is neutral. In the absence of multiple equilibria and hysteresis effects, as described, for example in Blanchard and Summers (1986), there are good theoretical reasons for long-run neutrality. Yet, we are not aware of *direct* and unambiguous evidence in favour of long-run neutrality.[24] In the context of our simple laboratory economy we get, however, clear evidence that in the long run nominal prices adjust rather close to the new equilibrium.The result is summarized in

R3: *In the long run, i.e., towards the end of the post-shock phase actual nominal prices adjust close to the equilibrium in all three treatment conditions and the real effects of money vanish.*

R3 also means that in the long run money illusion has no impact on behavior because equilibrium is reached in *each* treatment. Support for R3 comes from *Figure 1*, and *Tables 2* and *3*. *Figure 1* shows that the median group is exactly in equilibrium in each treatment in periods 36-40. *Table 3* indicates that during these periods 69% of all group observations are *exactly* in equilibrium in the NT while in the other two conditions even 76% of all group observations are *exactly* in equilibrium. In addition, *Table 2* shows that the deviations from equilibrium are small because *average* prices are close to equilibrium in periods 36-40. *Table 2* also shows that the real effects of the money shock vanish over time because efficiency levels towards the end of the post-shock phase are rather similar to the efficiency levels shortly before the shock. This indicates that money is neutral in the long run.

24. In the survey paper on the real effects of money in the Handbook of Monetary Economics Blanchard (1990: 828) writes for example: The long-run neutrality of money "is very much a matter of faith, based on theoretical considerations rather than on empirical evidence". In the meantime, however, several empirical studies are available. Fisher and Seater (1993) reject the long-run neutrality of money for the U.S. Yet, Boschen and Otrok (1994) question these results.They argue that if one accounts for the exceptional period from 1930 to 1939 (when an extraordinary number of bank failures occurred) by a dummy variable, long-run neutrality prevails for U.S. data. Haug and Lucas (1997) provide independent evidence that the rejection of long-run neutrality by Fisher and Seater is based on the anomalous period of the 1930's. Lucas (1996) provides data suggesting that long-run neutrality is the rule rather than the exception. In contrast, Ball's (1998) empirical study indicates that money shocks have long-run effects by changing the equilibrium unemployment rate.

4.4 Causes of nominal inertia

In principle, there are at least two potential reasons for the substantial amount of nominal inertia observed in our experiment. First, it may be the case that subjects are simply confused after the monetary shock and do no longer play best replies to their expectations. The near-rationality approach, for example, assumes that a fraction of subjects fails to maximize after a monetary shock because the losses associated with non-adjustment are small. In our experiment some subjects may also be somehow anchored at the "historically" given pre-shock price level so that they do not play best reply. Second, it may be that nominal inertia is caused by sticky expectations. Subjects may believe that the prices of other agents remain relatively high and play best reply to this expectation. Due to strategic complementarity their own price will then also be relatively high.

With regard to best reply behavior the following result emerges from our analysis:

R4: *In all treatment conditions and all periods a large majority of subjects plays exactly a best reply to* $\overline{P}^{e}_{-i}$. *On average, deviations from the best reply are small in each treatment.*

* Insert ***Figure 3*** about here *

Support for R4 is provided by *Figure 3*. In this figure we show the evolution of the percentage of subjects who play *exactly* best reply to their expectation $\overline{P}^{e}_{-i}$. Before the shock this percentage is between 70 and 90 percent. Immediately after the shock there is a relatively small drop in the percentage of best replies. However, those subjects who deviate are, in general, playing relatively close to the best reply. Additional information on best reply behavior is provided by *Figure 4*. This figure compares, for given intervals of price expectations $\overline{P}^{e}_{-i}$, the average best reply with the average level of the actually chosen nominal prices in periods 21-25.[25] The numbers above the bars indicate the relative frequency of price observations in the respective intervals. In the NT, for example, 14 percent of all price expectations $\overline{P}^{e}_{-i}$ fall within the interval 16-18 (see top diagram of the figure). *Figure 4* indicates that for any interval the deviation of actual average prices from the average best reply is relatively small in the NT. The same holds true for the SRT and the RT .[26]

25. The best reply for *x*- and *y*-type subjects is weakly monotonic in P_{-i}. The impression of the non-monotonicity of best replies in *Figure 4* is created by the fact that we aggregated over *x* and *y* types and that the relative frequency of *x*- and *y*-types differs across expectation intervals. Based on tests for differences in best reply behavior across types we conclude that aggregation over types is unproblematic.

* Insert ***Figure 4*** about here *

Next we summarize our main result with regard to price expectations $\bar{P}^{e}_{-i}$:

R5: *In all treatment conditions there is some inertia in price expectations after the shock. However, price expectations exhibit much more inertia in the Nominal treatment compared to the other two treatment conditions.*

Support for R5 is provided by *Figure 5* and *Table 4*. *Figure 5* describes the evolution of the average expectation of the median group over time.[27] This picture is qualitatively strikingly similar to *Figure 1* which shows the evolution of average prices of the median group. In all three treatments price expectations exhibit some inertia but in the NT expectations are much more sticky. The jump in price expectations immediately after the shock is more than twice as big in the RT and the SRT as in the NT. Moreover, while it takes 5 and 7 periods, respectively, until expectations reach the equilibrium in the RT and the SRT, it takes 14 periods until equilibrium expectations prevail in the NT.

* Insert ***Figure 5*** about here *

* Insert ***Table 4*** about here *

Table 4 reveals that the evolution of average expectations (over all groups) follows roughly the same pattern as the evolution of average expectations of the median group. Expectations exhibit some stickiness in all three treatments immediately after the shock but the stickiness is much larger and adjustment takes much longer in the NT. Information about the inertia of expectations is also provided by *Figure 4*. The top diagram in *Figure 4* indicates, for example, that, in periods 21-25, 66% of price expectations in the NT are strictly above $P^{e}_{-i} = 9$. In contrast, only 22% of price expectation in the RT or the SRT are above 9. This can be considered as rather strong evidence that expectations do not jump to the new post shock equilibrium values and that the speed with which expectations adjust, is much lower in the NT.

26. In the Semi-real and the Real treatment we occasionally observe relatively "large" deviations from best reply behavior (e.g. in the interval 19-21 in the bottom diagram). However, these deviations are, in general, outlyers which is indicated by the small number of observations in the corresponding intervals.
27. The unit of observation is the average over all $\bar{P}^{e}_{-i}$-values in a group.

5. Interpretation and concluding remarks

At a superficial level it seems that money illusion does not fit well into the maximizing framework of modern economics. However, simple game-theoretic reasoning shows that in interactive situations with strategic complementarity the conditions for the absence of behavioral effects of money illusion are extremely demanding. Paraphrasing Abraham Lincoln[28], one can say that, to render money illusion behaviorally relevant, it is neither necessary to fool all the people some of the time nor some of the people all the time, not to speak of fooling all the people all the time. It is not even necessary that there are "fools", i. e., people with money illusion. All that is needed is that there are some people who believe that others believe, etc., that money illusion leads to less than full adjustment of nominal prices after a nominal shock. As long as such beliefs prevail rational individuals find it in their interest to not fully adjust their nominal prices, either.

The results of our experiments unambiguously show that people do not expect full price adjustment after a nominal shock. Moreover, as *Figure 5* shows, our results leave little doubt that in the Nominal Treatment the stickiness of price expectations is much larger than in the Real or the Semi-real Treatment. The veil of money that is incorporated in the Nominal Treatment is thus responsible for much of the inertia in price expectations. In the presence of strategic complementarity and best reply behavior this inertia in price *expectations* causes, in turn, inertia in price *choices*. Therefore, in period t+1 subjects have little reason to make big adjustments in price expectations after they observed the rather small adjustment of aggregate prices in period t. The small adjustment in expectations in t+1 again provides an incentive to change prices in period t+1 only a little. Thus, small adjustments in expectations cause small adjustments in actual prices which in turn render only small adjustments in expectations reasonable. The overall result of these incremental changes in expectations and prices is a rather slow convergence to the equilibrium which is associated with substantial income losses during the adjustment phase.

Most business transactions are shrouded in nominal money and the smallest nominal accounting unit is kept constant despite frequent changes in the money supply and nominal prices. Our Nominal Treatment captures these two important features of economic life. In other dimensions, however, our experimental design is simpler by orders of magnitude. The representation of the

28. In his speech on 8 Sept. 1858 A. Lincoln said: "You can fool all the people some of the time, and some of the people all the time, but you cannot fool all the people all the time."

strategic situation by payoff matrices makes it rather easy to find the best reply for a given price expectation. In contrast, in naturally occurring environments the nature of the decision problems and of strategic interaction may render it quite difficult to find best replies. As a consequence, nominal inertia due to near rationality is more likely to prevail. In the Nominal Treatment we trained subjects how to compute real payoffs from the nominal payoff table and the experiment did not start before everybody had correctly solved the training exercises. In naturally occurring environments no similar educational measures precede nominal shocks. It is, thus, more likely that beliefs about the prevalence of money illusion exist. In addition, in our experiment subjects received unambiguous feedback information about the aggregate price level at the end of each period. In reality, the relevant information signals are probably much more noisy and there is much more information to consider. Therefore, the formation of rational expectations is more complicated. Taken together, this suggests that we created an experimental environment which is relatively hostile to money illusion and nominal inertia. Yet, since money illusion has behavioral effects even in such a simple environment there is little reason to believe that it is irrelevant in more complicated environments.

In view of the simplicity of our experimental environment and because of the training procedure before the start of the experiment it seems unlikely that subjects themselves suffered from money illusion. In our view it is more likely that money illusion prevailed at a higher belief level, that is, that subjects expected others to suffer, or expected that others expected others to suffer, etc. from money illusion. The fact that subjects expect relatively small price adjustments after the shock in the Nominal Treatment also suggests that this interpretation is correct. In a more complicated environment, however, it seems more likely that at least some people suffer from money illusion themselves.

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Appendix A – Functional specification of payoffs

As explained in detail in section 3.1 our specification of subjects' payoff functions served several purposes. A particularly important purpose was to rule out that the adjustment to the equilibrium is confounded by subjects' attempts to achieve out-of-equilibrium cooperative gains. Note that this purpose rules out payoff functions that are derived from oligopolistic or monopolistic competition among firms. We achieved our aim by the payoff functions below because they imply that the equilibrium is the only efficient point in payoff space.

Note also that the equilibrium price for each individual *i* is a best reply not only to the equilibrium expectation for P_{-i} but also to out-of-equilibrium expectations that are *close* to the equilibrium expectation (see also payoff tables T1-T6 in Appendix C below). This feature of the payoff functions speeds up adjustment to equilibrium behavior because it ensures that the equilibrium price choice is also a best reply for expectations that are not exactly in equilibrium. The arctan-function in the denominator reflects this property of the payoff functions.

The real payoff for agent *i* of type $k = x, y$ is given by:

$$\pi_{ik} = \frac{V \cdot \left[\dfrac{1 + a\left(\dfrac{P_{-ik}}{M} - \dfrac{P_k}{M} \right)}{1 + b\left(\dfrac{\bar{P}_{-ik}}{M} - \dfrac{\bar{P}^*_k}{M} \right)^2} \right]}{1 + c\left\{ \left(\dfrac{P_{ik}}{M} - \dfrac{P^*_k}{M} \right) - d\left(\dfrac{\bar{P}_{-ik}}{M} - \dfrac{\bar{P}^*_k}{M} \right) + e \cdot \arctan\left[\left(\dfrac{\bar{P}_{-ik}}{M} - \dfrac{\bar{P}^*_k}{M} \right) \cdot f \right] \right\}^2}$$

In all periods and all experimental sessions the parameters *a*, *b*, *c*, d, e, *f* and *V* were the same. They were given by $a = 0.5$, $b = 0.6$, $c = 27$, $d = 1$, $e = 0.05$, $f = 20$ and $V = 40$.

P_{-ik} is the *actual* average price of the other *n*-1 players from the viewpoint of player *i* who is of type *k*. $\bar{P}^*_k$ is the *equilibrium* average price of the other *n*- 1 players from the viewpoint of a player of type *k*. P_{ik} is the *actual* price of *i* who is of type *k*. P^*_k is the *equilibrium* price of a player of type *k*.

Appendix B – Instructions

The original instructions were in German. This section reprints a translation of the instructions used in the Nominal treatment for agents of type *y*.

General instructions for participants

You are participating in a scientific experiment which is funded by the Swiss National Science Foundation. The purpose of this experiment is to analyze decision making in experimental markets. If you read instructions carefully and take appropriate decisions, you may earn a considerable amount of money. At the end of the experiment all the money you earned will be immediately paid out in cash.

Each participant is paid SFr.15.- for showing up. During the experiment your income will not be calculated in Swiss Francs but in points. The total amount of points you collected during the experiment will be converted into Swiss Francs, by applying the following exchange rate:

10 Points = 15 centimes.

Here is a brief description of the experiment. A more detailed description is given below. All participants are in the role of firms, selling some product. In this experiment, there are two types of firms: firms of type *x* and firms of type *y*. Each firm has to choose a selling price in every period. The income you earn depends on the price you choose and on the prices all other firms choose.

During the experiment you are **not allowed to communicate with any other participant. If you have any questions, the experimenters will be glad to answer them.** If you do not follow these instructions you will be excluded from the experiment and deprived of all payments.

The following pages describe the procedures of the experiment in detail.

Detailed information for firms of type *y*

This experiment lasts 20 periods plus one trial period. You are not paid for the trial period. You should nevertheless take the trial period seriously since you may gain experience in this period. This experience helps you to take decisions in the other periods which are paid out. You are in the role of a firm, just as all other participants in this experiment. All participants are in **groups of 4**, i.e. every participant is in a group with three other firms. There are two firms of type *x* and two firms of type *y* in every group.

You are a firm of type *y*

Consequently, there are two other firms of type *x* and one more firm of type *y* in your group. No participant knows which persons are in his or her group. Yet, everybody knows that the group composition remains constant throughout the experiment. The decisions taken by other groups are irrelevant for your group.

In every period all firms simultaneously decide which selling price they set for the current period. Every firm has to choose an integer price from the interval

$$1 \leq \text{selling price} \leq 30 \quad .$$

How much you earn depends on the price you choose and on the average price of all other firms in your group. Independent of the type, the average price for every firm is calculated by the following formula:

Average price = (Sum of selling prices of other 3 firms) / 3

Consequently, the average price will be in the interval

$$1 \leq \text{average price} \leq 30 \quad .$$

The average price is rounded to the next integer number.

How to read the income table for a firm of type *y*

The **green** income table shows your nominal income in points if you choose a specific price and a specific average price results in this period (see separate table). Your income at the end of the experiment is not based on nominal point income, but on real point income. The following relation between the two holds:

Real income = Nominal income / Average price of other firms

This formula holds for all firms. The real point income that will be paid out is rounded in every period to the next integer number.

Example:
Suppose, you choose a price of 2 and the actual average price is 4. In this case your nominal point income is 29 points. Your (rounded) real income is 7 points (= 23 / 4).

When you decide which price to choose, you do not yet know which average price will actually result in this period. The green income table can consequently help you to calculate your real point income given your **expectation** on the average price of other firms.

Example:
Given an expectation on the average price you can read off the green table the payoff you get when choosing different selling prices. For example, if you expect an average price of 30 and choose a price of 17, your expected nominal income is 141 points, your expected real income is 5 points (= 141 / 30). If you choose a price of 10 at this expected price, your expected nominal income is 86 points, your expected real income 3 points (= 86 / 30).

Please note that you are in a group with one firm of type *y* and two firms of type *x*. To determine the income of the other firm of type *y*, you have to use the green table. To determine the income of the other two firms of type *x*, you have to use the blue income table. This table also shows nominal income in points. The same formula above is used to calculate real payoffs for firms of type *x*.

What the screens show

On both screens described below the current period is indicated in the upper left corner, and the upper right corner displays remaining time in seconds to decide or to view the screen.

The upper half of the **input screen** (see figure on next page) has three cells, where you can enter data into the computer.

Price decision: Enter an integer number between 1 and 30 into the first cell. You can activate this cell (as well as the other cells) by clicking into the cell with your mouse. If you want to revise your decision, you can erase the number by hitting the backspace key.

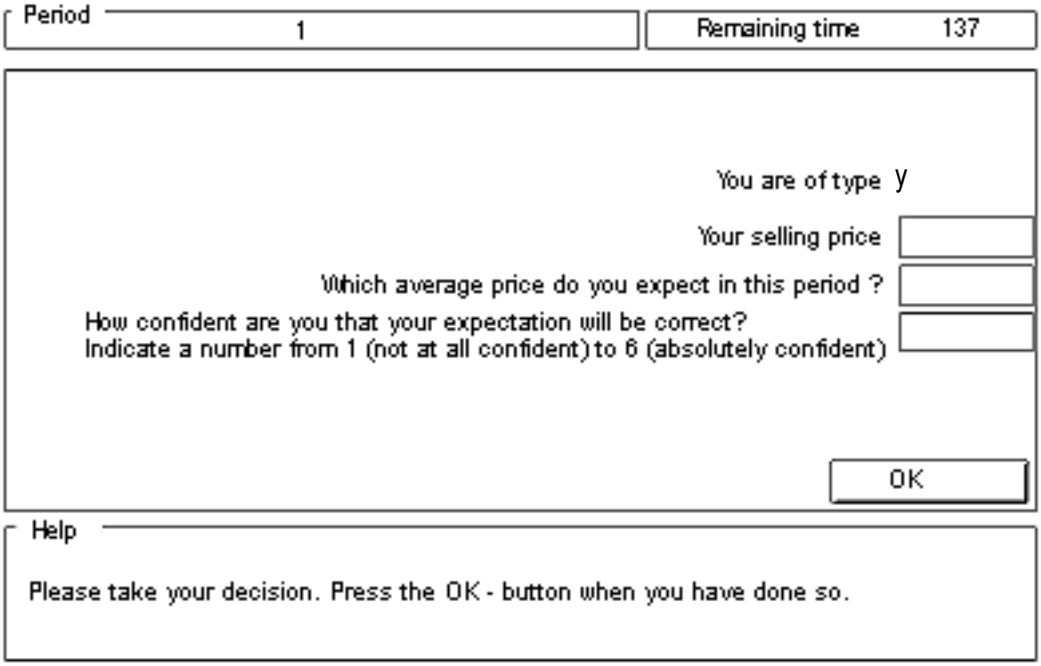
Expected average price: Enter an integer number between 1 and 30 into the second cell. This input does not affect your income and will not be known to other firms. Your payoff will be determined by the actual average price of this period. Please try to indicate an expectation that is as exact as possible since this is going to help you to take your own price decision.

Confidence: Enter an integer number from 1 to 6 to indicate how confident you are that the average price you expect (= number in the second cell) will actually result.

The numbers stand for:
1 = I am not at all confident that my expectation will be correct
2 = I am not very confident that my expectation will be correct
3 = I am not quite confident that my expectation will be correct
4 = I am quite confident that my expectation will be correct
5 = I am very confident that my expectation will be correct
6 = I am absolutely confident that my expectation will be correct

When you finished entering the numbers into the respective cells, press the **OK-button**. Once you have pressed the button, you cannot revise your decision any more for this period.

Figure B1: Input screen

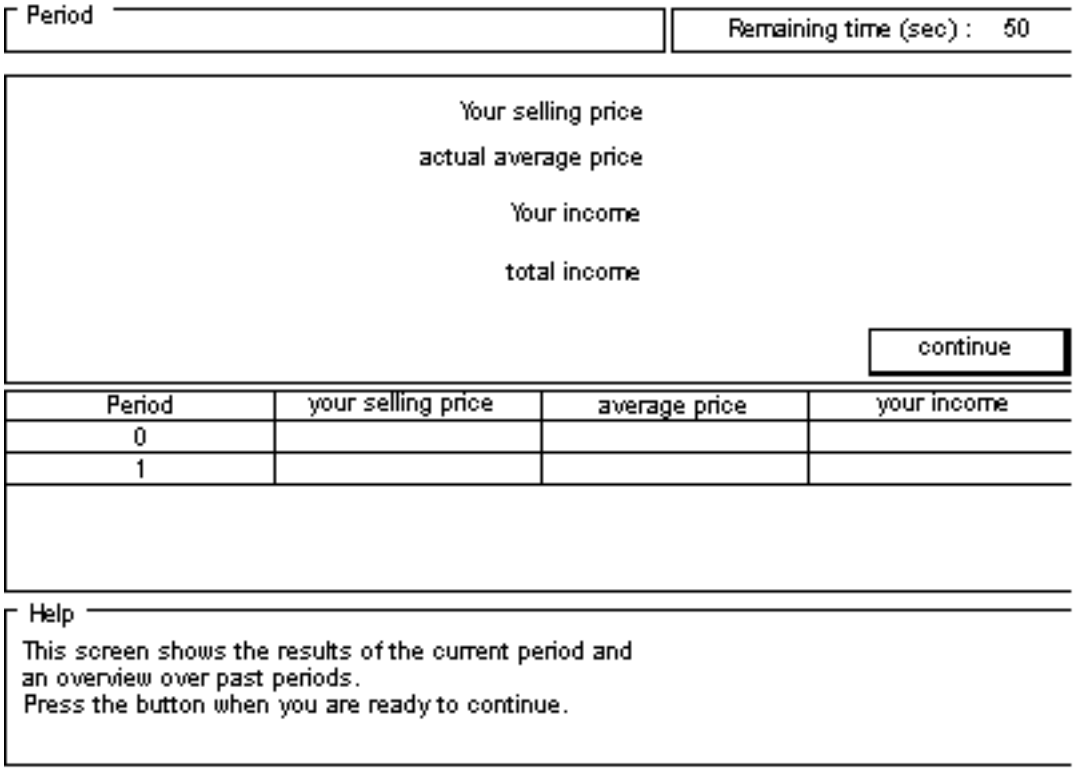


As soon as all firms have decided on their prices, the outcomes of this period will be shown in the outcome-screen.

The upper part of this screen shows the outcomes of the current period. This screen shows your decision of the current period, the average price, your real income of this period, and your total real payoff.

The lower part of this screen displays the outcomes of past periods.

Figure B2: Outcome screen



Overview: What you have to do in every period.

In every period every firm has to choose a price. Every integer price from 1 to 30 can be chosen $1 \leq$ selling price ≤ 30.

- Enter your price decision into the first cell of the input screen.
- Enter into the second cell the average you expect for this period $1 \leq$ expected average price ≤ 30 .
- Enter your confidence in your price expectation into the third cell (numbers **1 to 6**).

When you have completed the three cells, press the OK-Button. The remaining time to take your decisions is shown in the upper right corner of the screen.

When all participants have taken their decisions, or when the time has elapsed, all participants are shown the outcome screen. This screen shows your decisions, actual average prices and your real payoff in points for the current and the past periods.

To take your decisions the following aids are at your disposal:

Green

income table: Helps you to estimate your expected nominal point income (You are a firm of type *y*). Your payoff is determined by your real income in points.
You can calculate your real income from the nominal income (= numbers shown in the income table) by applying the following formula:

Real income = Nominal income / Average price of other firms

Blue
income table: Helps to estimate the nominal point income of the firms of type *x* in your group. The payoff of these firms are also determined by their real point income. To calculate the real income of firms of type *x*, you also apply the formula above.

Outcome screen: Displays your selling price, the actual average price and your real income for the present and the past periods.

Do you have any questions?

Control questions

You have to answer all of the following questions. If you do not answer a question, you will be excluded from the experiment and all payments. Wrong answers do not have any consequences. If you have any questions, please ask us.

1. Please indicate an expectation for the average price of other firms from 1 to 30.
   Expected average price .....

2. Please indicate a selling price from 1 to 30.
   Selling price .....

3) What is your expected nominal income in points at the prices you indicated in 1) and 2)?
   Your nominal income .....

4. What is your expected real income in points at the prices you indicated in 1) and 2)?
   Your real income .....

5. Suppose you choose a price of 1. The other firm of type *y* chooses a price of 30. The first firm of type *x* chooses a price of 7 and the second firm of type *x* chooses a price of 23.

a) What is your average price at the (fictitious) prices? .....
   What is your nominal income? .....
   What is your real income? .....

b) What is the average price of the other firm of type *y*?.....
   What is the nominal income of this firm? .....
   What is the real income of this firm? .....

c) What is the average price of the first firm of type *x*? .....
   What is the nominal income of this firm? .....
   What is the real income of this firm? .....

d) What is the average price of the second firm of type *x*?.....
   What is the nominal income of this firm? .....
   What is the real income of this firm? .....

Table 1: Experimental design

		Nominal treatment	**Semi-real treatment**	**Real treatment**
All periods	Representation of payoffs	nominal ($P_{-i}\pi_i$)	real (π_i)	real (π_i)
	Smallest nominal accounting unit	constant (variation in real terms)	constant (variation in real terms)	adjusted to keep real accounting unit constant
	Group size	$n = 4$	$n = 4$	$n = 4$
	Information feedback in period t	P_{-i}, π_i	P_{-i}, π_i	P_{-i}, π_i
	Real equilibrium payoff (pre- and post-shock, for both types)	40	40	40
Pre-shock (t = 1 - 20)	Choice variable	$P_i \in \{1, 2, \ldots, 30\}$	$P_i \in \{1, 2, \ldots, 30\}$	$P_i \in \{3, 6, \ldots, 90\}$
	Money supply M_0	42	42	42
	Average equilibrium price $\bar{P}^*$ and average equilibrium expectation for the *whole* group	18	18	18
	Equilibrium price for type x	9	9	9
	Equilibrium expectation $\bar{P}^e_{-i}$ for type x	21	21	21
	Equilibrium price for type y	27	27	27
	Equilibrium expectation $\bar{P}^e_i$ for type y	15	15	15
Post-shock (t = 21 - 40)	Choice variable	$P_i \in \{1, 2, \ldots, 30\}$	$P_i \in \{1, 2, \ldots, 30\}$	$P_i \in \{1, 2, \ldots, 30\}$
	Money supply M_1	14	14	14
	Average equilibrium price $\bar{P}^*$ and average equilibrium expectation for the *whole* group	6	6	6
	Equilibrium price for type x	3	3	3
	Equilibrium expectation $\bar{P}^e_{-i}$ for type x	7	7	7
	Equilibrium price for type y	9	9	9
	Equilibrium expectation $\bar{P}^e_{-i}$ for type y	5	5	5

Table 2: Evolution of prices and efficiency losses over time (all entries rounded to integers)

	Average price of the median group			Average price			Average efficiency loss (percent)		
period	Nominal treatment	Semi-real treatment	Real treatment	Nominal treatment	Semi-real treatment	Real treatment	Nominal treatment	Semi-real treatment	Real treatment
18	18	18	18	19	18	19	3	4	3
19	18	18	18	18	18	21	1	2	12
20	18	18	18	18	18	21	1	1	9
21	13	9	8	13	9	8	65	52	32
22	13	7	7	13	8	7	47	20	11
23	11	7	6	11	7	7	35	15	5
24	10	7	6	10	7	7	27	9	6
25	9	6	6	10	7	7	17	15	5
26	10	6	6	10	7	6	16	8	4
27	10	6	6	10	6	6	16	4	1
28	9	6	6	9	6	6	11	5	1
29	8	6	6	9	6	6	9	4	1
30	7	6	6	9	7	6	14	11	2
31	6	6	6	8	6	7	8	5	10
32	6	6	6	8	6	6	6	3	6
33	6	6	6	7	6	6	6	2	3
34	6	6	6	7	6	6	5	3	0
35	6	6	6	7	6	6	3	3	0
36	6	6	6	7	6	7	10	2	10
37	6	6	6	7	6	7	5	1	3
38	6	6	6	7	6	7	14	2	4
39	6	6	6	7	6	7	12	1	12
40	6	6	6	7	6	7	2	3	7

Table 3: Percentage of price observations above, in and below the equilibrium (group averages as units of observation)

	Nominal (n =11)			Semi-Real (n =10)			Real (n =10)		
Period	above	in	below	above	in	below	above	in	below
21 - 25	93	**7**	0	82	**16**	2	52	**34**	14
26 - 30	58	**42**	0	28	**70**	2	14	**82**	4
31 - 35	42	**56**	2	22	**74**	4	22	**78**	0
36 - 40	31	**69**	0	22	**76**	2	24	**76**	0

Table 4: Evolution of expectations

	Average expectation of the median group			Average expectation		
period	Nominal treatment	Semi-real treatment	Real treatment	Nominal treatment	Semi-real treatment	Real treatment
18	18	18	18	19	18	20
19	18	18	18	19	18	20
20	18	18	18	18	18	22
21	14	9	9	14	10	9
22	13	9	8	13	8	8
23	12	8	6	13	8	7
24	12	7	7	12	8	7
25	11	7	6	11	7	7
26	11	7	6	10	7	6
27	10	6	6	10	7	6
28	9	6	6	10	7	6
29	8	6	6	9	7	6
30	8	6	6	9	6	6
31	7	6	6	9	6	6
32	7	6	6	8	6	6
33	7	6	6	8	6	6
34	6	6	6	7	6	6
35	6	6	6	7	6	6
36	6	6	6	7	6	6
37	6	6	6	7	6	7
38	6	6	6	7	6	7
39	6	6	6	7	6	7
40	6	6	6	7	6	7

Figure 1: Nominal average prices of median group by treatment

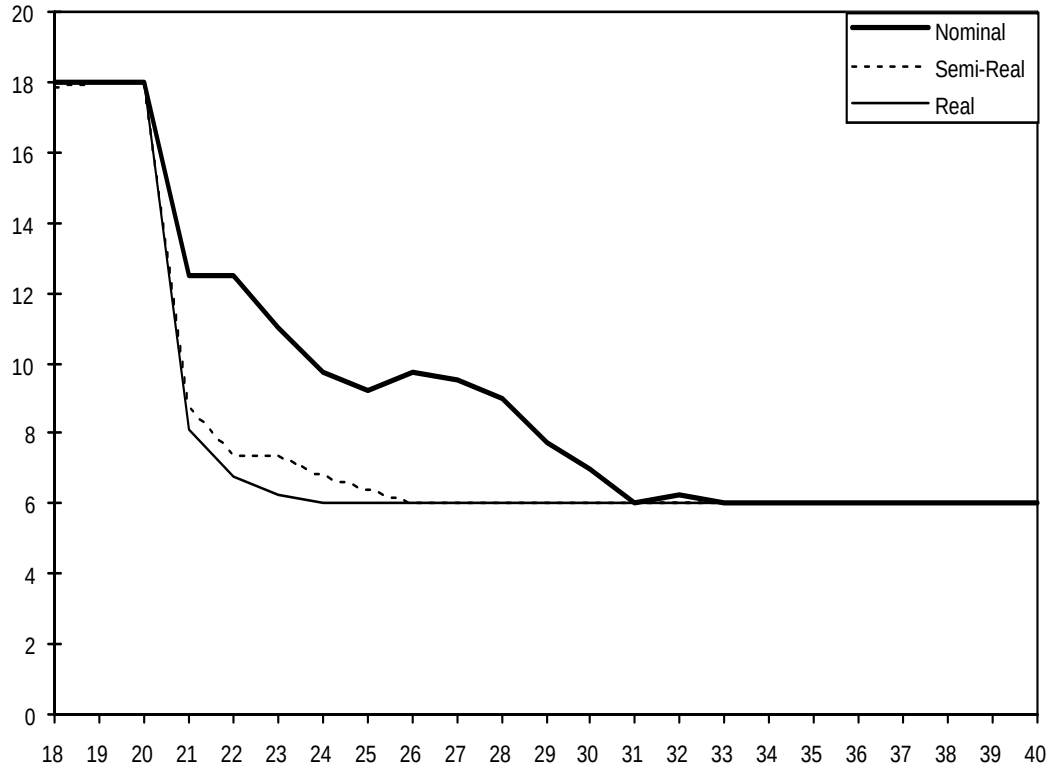


Figure 2: Efficiency losses of the median group

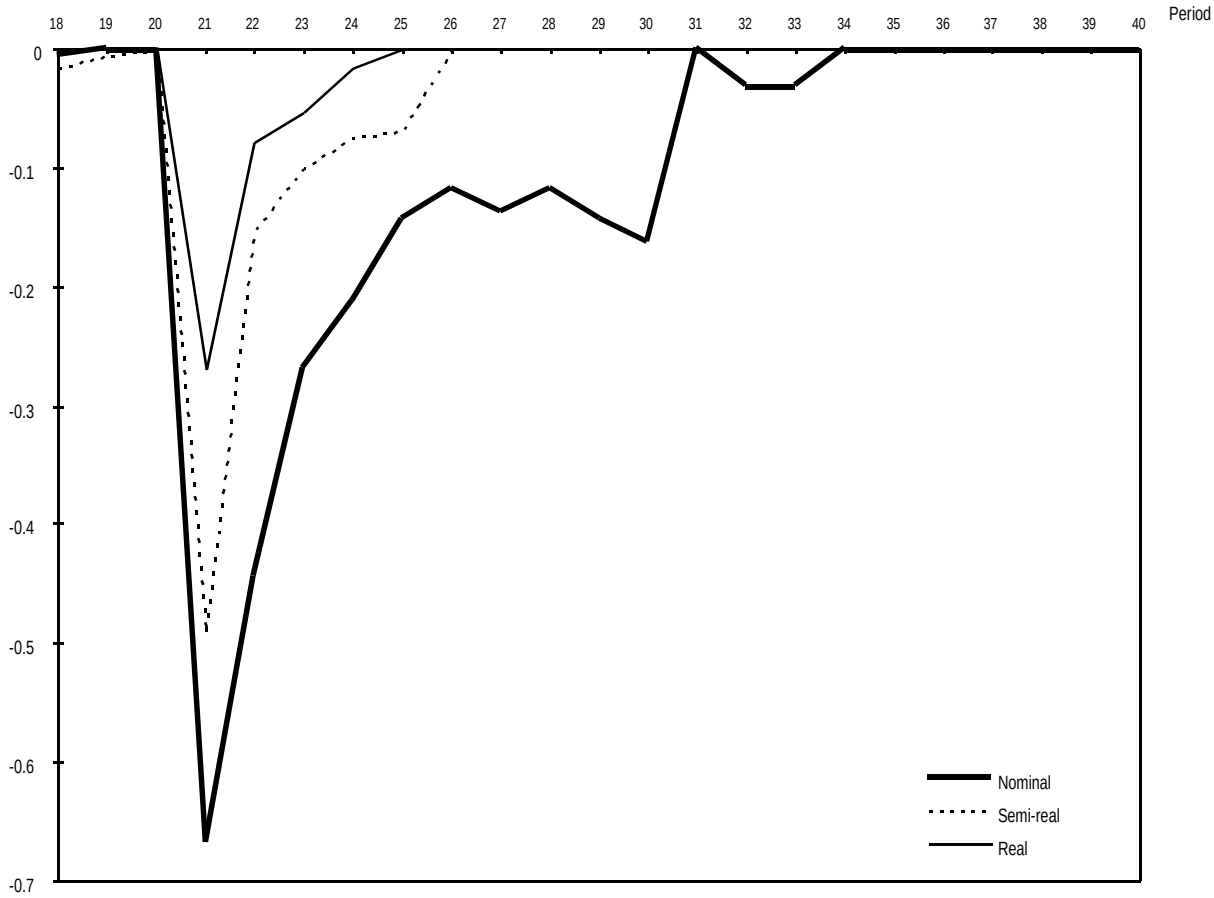


Figure 3: Percentage of exact best replies

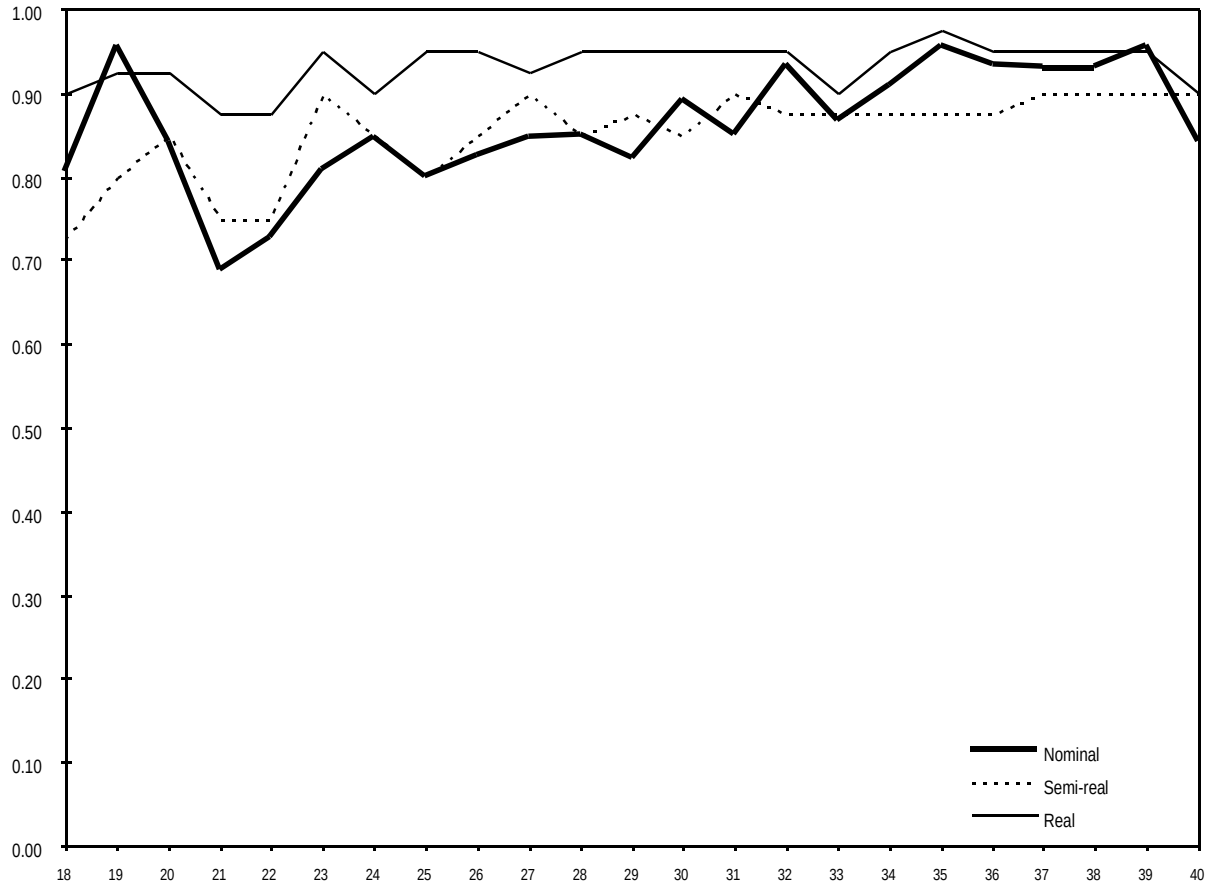


Figure 4: Actual average prices and average best reply for given expectations by treatment (cumulative for periods 21 - 25)

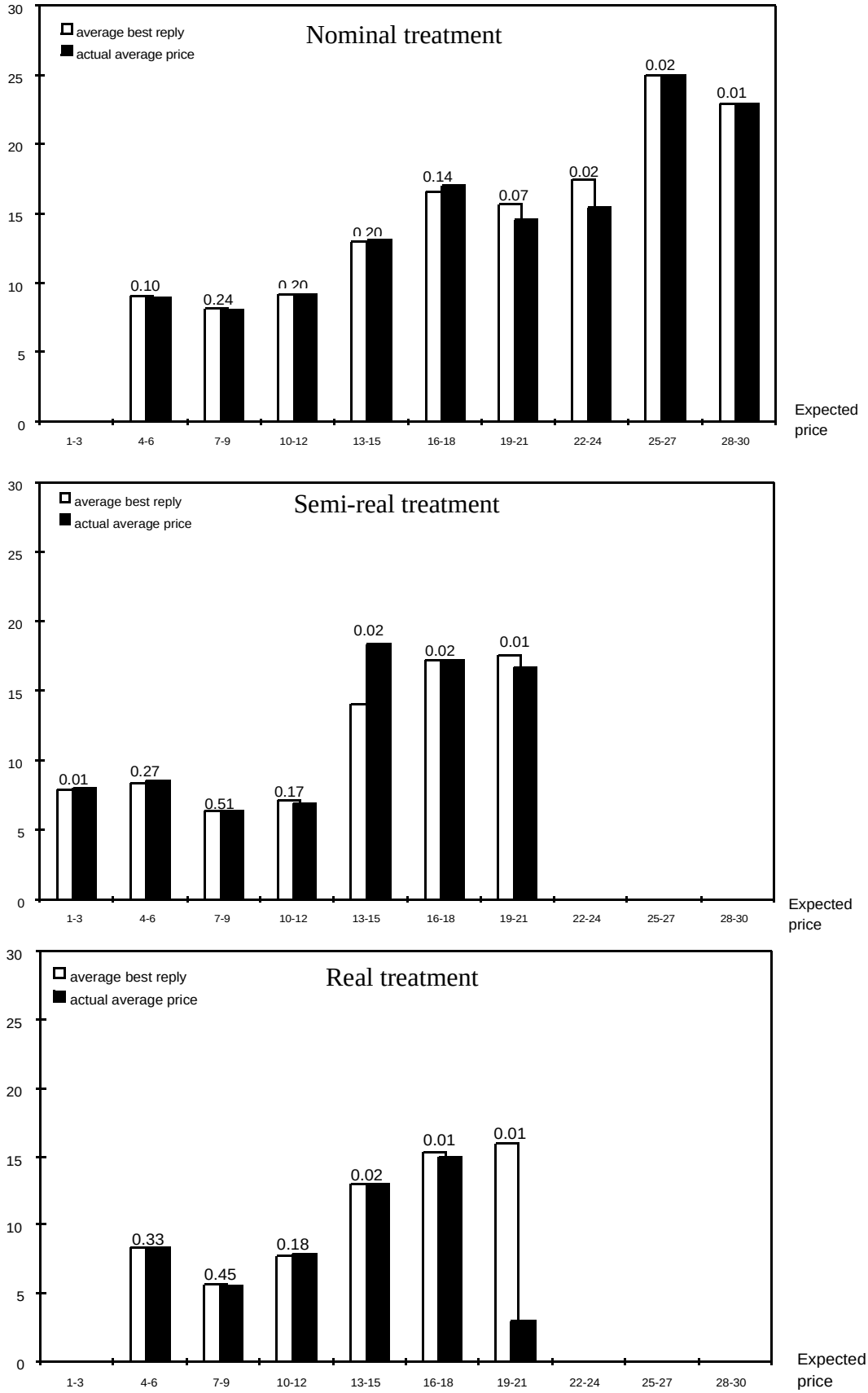
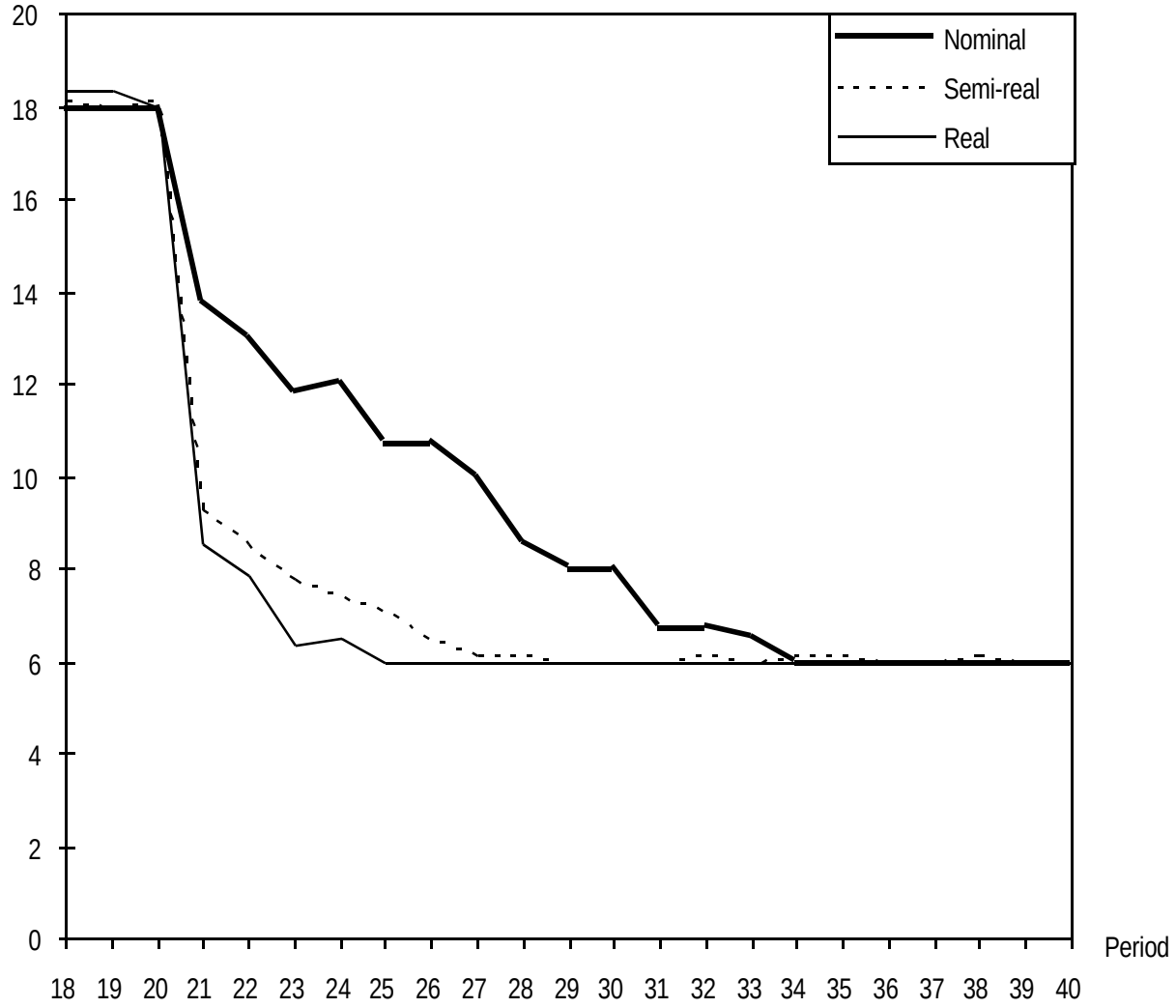


Figure 5: Expectation of the median group by treatment



Payoff Table T1: Nominal, pre-shock, Type *y*

Average price of other firms

elling price	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**	**15**	**16**	**17**	**18**	**19**	**20**	**21**	**22**	**23**	**24**	**25**	**26**	**27**	**28**	**29**	**30**
1	9	16	22	27	31	34	37	39	41	42	44	45	47	50	53	56	58	59	60	59	59	58	58	57	56	55	54	53	52	51
2	10	18	24	29	34	37	40	42	44	46	47	49	51	53	57	60	63	63	64	63	63	62	61	60	59	58	57	56	55	54
3	11	20	27	33	37	41	44	46	48	50	51	53	55	57	61	65	67	68	68	68	67	66	65	64	63	62	60	59	58	57
4	12	22	30	36	41	44	48	50	52	54	55	57	59	62	66	70	72	73	73	73	72	71	69	68	67	65	64	63	61	60
5	14	24	33	40	45	49	53	55	57	59	60	62	64	67	71	76	78	79	79	78	77	76	74	73	71	70	68	66	65	63
6	16	28	37	45	50	55	58	61	63	64	66	67	69	73	77	82	85	86	85	84	83	81	79	78	76	74	72	70	69	67
7	18	31	42	50	56	61	64	67	69	70	72	73	76	79	84	89	92	93	92	91	89	87	85	83	81	79	77	75	73	71
8	20	36	47	56	63	68	72	74	76	78	79	80	83	86	92	97	101	101	100	99	97	94	92	89	87	84	82	80	78	76
9	23	40	54	64	71	76	80	83	84	86	87	88	91	95	101	107	110	110	109	107	105	102	99	96	93	90	88	85	83	80
10	26	46	61	72	80	86	90	92	94	95	96	97	100	104	111	117	121	121	119	117	114	110	107	104	100	97	94	91	88	86
11	29	51	69	81	90	97	101	103	105	106	107	108	110	115	122	129	133	133	131	128	124	120	116	112	108	104	101	97	94	91
12	32	58	77	92	102	109	114	116	118	118	119	120	122	127	135	143	147	147	144	140	136	131	126	121	117	113	109	105	101	98
13	35	64	87	104	116	123	128	131	132	133	133	134	136	141	150	159	163	162	159	154	149	143	138	132	127	122	117	113	109	104
14	37	70	96	116	130	140	145	148	149	149	149	149	152	157	167	177	181	180	176	170	164	157	150	144	138	132	127	122	117	112
15	39	74	105	129	146	157	164	168	169	168	168	168	170	176	187	198	203	201	196	189	181	173	165	158	151	144	138	132	126	121
16	40	78	113	141	162	176	185	189	191	191	190	189	191	198	210	223	227	225	218	209	201	191	182	173	165	157	150	143	136	130
17	39	79	117	151	177	195	207	213	216	216	214	214	216	223	237	251	256	253	245	235	223	212	201	191	181	172	163	155	148	141
18	37	78	119	157	189	213	229	239	243	244	243	242	244	252	268	283	289	285	275	263	250	236	223	211	199	189	179	170	161	153
19	35	74	117	159	197	227	250	264	272	274	274	274	276	285	303	321	327	322	311	296	280	264	249	234	221	208	196	186	176	167
20	32	69	112	156	199	236	266	287	300	306	308	309	312	323	343	363	370	364	351	334	315	296	278	261	245	230	216	204	192	182
21	29	63	104	149	195	239	277	306	326	337	343	346	351	364	387	409	418	412	397	378	356	334	312	292	273	256	240	225	211	199
22	25	57	95	139	186	234	279	317	346	365	377	385	393	408	434	459	470	465	449	427	403	377	352	328	306	285	266	249	233	219
23	23	50	85	126	173	223	273	319	357	387	407	421	434	453	482	511	524	521	506	483	456	426	397	369	343	319	297	276	258	241
24	20	45	76	113	158	206	259	311	358	398	429	452	471	495	527	559	578	579	566	544	515	483	450	417	387	358	332	308	287	267
25	18	40	67	101	142	188	240	295	349	398	439	472	500	530	565	601	625	633	627	608	579	545	509	472	437	403	373	345	320	297
26	16	35	60	89	126	169	219	273	330	386	436	480	517	552	591	629	660	679	682	671	646	613	574	534	494	456	420	387	358	331
27	14	31	53	79	112	151	196	248	305	364	421	473	519	560	600	640	678	709	727	727	712	683	645	603	559	516	475	437	402	370
28	12	28	47	70	99	134	175	223	277	335	395	452	505	550	591	631	676	720	753	771	770	751	719	677	631	583	537	493	453	416
29	11	24	41	62	87	118	155	198	248	304	363	422	478	526	565	605	654	708	758	795	814	811	790	754	708	658	607	558	512	469
30	10	22	37	55	77	104	137	175	221	272	328	386	442	490	527	565	616	677	741	796	836	855	851	827	788	739	686	631	579	530

Payoff Table T2: Nominal, post-shock, Type *y*

Average price of other firms

elling price	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
1	9	14	16	17	20	23	22	21	20	19	18	17	16	15	14	13	13	12	12	11	11	10	10	9	9	9	8	8	8	8
2	12	18	21	22	26	29	28	26	24	22	21	19	18	17	16	15	14	13	13	12	12	11	11	10	10	9	9	9	8	8
3	18	25	28	29	34	37	35	32	29	27	25	23	21	19	18	17	16	15	14	13	13	12	11	11	10	10	10	9	9	9
4	26	36	39	40	45	49	45	40	36	33	29	27	24	22	21	19	18	17	16	15	14	13	13	11	11	11	10	10	10	9
5	35	52	56	56	62	67	60	53	46	40	36	32	29	26	24	22	20	19	18	16	15	15	14	13	12	12	11	11	10	10
6	40	71	81	81	89	95	83	70	60	51	44	39	34	31	28	25	23	21	20	18	17	16	15	14	14	12	12	12	11	11
7	35	80	109	115	129	137	119	97	80	66	56	48	42	37	33	30	27	24	22	21	19	18	17	16	15	14	13	13	12	11
8	25	68	119	151	176	193	172	139	111	89	73	61	52	45	39	35	31	28	26	24	22	20	19	17	16	15	14	13	13	12
9	18	50	102	157	200	236	237	201	158	123	98	79	66	56	48	42	37	33	30	27	25	23	21	19	18	17	16	15	14	13
10	12	34	74	129	176	226	279	275	229	177	136	107	86	71	59	51	44	39	35	31	28	26	23	22	20	19	17	16	15	14
11	9	24	51	92	129	173	253	318	312	255	195	148	115	92	75	63	54	46	41	36	32	29	27	24	22	21	19	18	17	16
12	7	18	36	63	89	121	189	284	357	348	281	212	160	124	98	80	67	57	49	43	38	34	30	28	25	23	21	20	19	17
13	5	13	26	45	62	84	131	210	316	395	382	305	229	172	132	104	84	70	59	51	45	39	35	32	29	26	24	22	21	19
14	4	10	19	33	45	60	91	145	232	347	433	416	331	246	183	140	110	89	74	62	53	46	41	36	33	30	27	25	23	21
15	3	8	15	25	34	44	66	102	161	254	379	470	449	355	262	194	148	116	93	77	65	56	48	43	38	34	31	28	26	24
16	3	6	12	19	26	34	49	73	112	176	277	410	506	481	378	278	206	156	122	98	81	68	58	50	44	39	35	32	29	26
17	2	5	10	15	20	26	37	54	81	123	192	300	440	541	513	402	294	217	164	127	102	84	70	60	52	46	41	36	33	30
18	2	4	8	12	16	21	29	42	60	89	134	207	322	471	576	544	425	310	227	171	133	107	87	73	62	54	47	41	37	34
19	2	4	7	10	14	17	24	33	46	66	97	145	223	344	500	611	575	448	326	238	179	139	111	91	76	64	56	49	43	39
20	1	3	6	9	11	14	19	27	37	51	72	105	156	238	365	530	645	606	470	341	249	186	144	115	94	79	67	58	50	44
21	1	2	5	7	10	12	16	22	30	40	56	78	112	166	253	386	559	679	636	492	357	259	194	150	119	97	81	69	59	52
22	1	2	4	6	8	10	14	18	24	32	44	60	84	120	177	267	407	588	712	666	514	372	270	202	156	124	101	84	71	61
23	1	2	4	5	7	8	12	15	20	27	35	48	65	90	128	187	282	428	616	745	695	536	387	280	209	161	128	104	87	73
24	1	2	3	5	6	8	10	13	17	22	29	38	51	69	95	135	197	296	449	645	778	725	558	402	291	217	167	132	108	89
25	1	2	3	4	6	7	9	11	15	19	24	32	41	55	73	101	143	207	311	469	673	811	754	580	417	301	224	172	136	111
26	1	2	3	4	5	6	8	10	13	16	21	26	34	44	58	78	107	150	218	325	490	701	843	783	601	432	312	232	178	141
27	1	1	2	3	4	5	7	9	11	14	18	22	28	36	47	61	82	112	157	227	339	510	728	875	813	623	447	322	239	183
28	1	1	2	3	4	5	6	8	10	12	15	19	24	30	39	50	65	86	117	164	237	353	530	755	908	842	644	461	332	247
29	1	1	2	3	4	4	6	7	9	11	13	17	21	26	32	41	52	68	90	123	171	247	367	550	784	940	870	666	477	343
30	0	1	2	3	3	4	5	6	8	10	12	15	18	22	27	34	43	55	72	95	128	179	257	381	570	811	972	899	687	491

Payoff Table T3: Semi-real, pre-shock, Type *y*

Average price of other firms

elling price	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
1	9	8	7	7	6	6	5	5	5	4	4	4	4	4	4	4	3	3	3	3	3	3	3	2	2	2	2	2	2	2
2	10	9	8	7	7	6	6	5	5	5	4	4	4	4	4	4	4	4	3	3	3	3	3	3	2	2	2	2	2	2
3	11	10	9	8	7	7	6	6	5	5	5	4	4	4	4	4	4	4	4	3	3	3	3	3	3	2	2	2	2	2
4	12	11	10	9	8	7	7	6	6	5	5	5	5	4	4	4	4	4	4	4	3	3	3	3	3	3	2	2	2	2
5	14	12	11	10	9	8	8	7	6	6	5	5	5	5	5	5	5	4	4	4	4	3	3	3	3	3	3	2	2	2
6	16	14	12	11	10	9	8	8	7	6	6	6	5	5	5	5	5	5	4	4	4	4	3	3	3	3	3	3	2	2
7	18	16	14	13	11	10	9	8	8	7	7	6	6	6	6	6	5	5	5	5	4	4	4	3	3	3	3	3	3	2
8	20	18	16	14	13	11	10	9	8	8	7	7	6	6	6	6	6	6	5	5	5	4	4	4	3	3	3	3	3	3
9	23	20	18	16	14	13	11	10	9	9	8	7	7	7	7	7	6	6	6	5	5	5	4	4	4	3	3	3	3	3
10	26	23	20	18	16	14	13	12	10	10	9	8	8	7	7	7	7	7	6	6	5	5	5	4	4	4	3	3	3	3
11	29	26	23	20	18	16	14	13	12	11	10	9	8	8	8	8	8	7	7	6	6	5	5	5	4	4	4	3	3	3
12	32	29	26	23	20	18	16	15	13	12	11	10	9	9	9	9	9	8	8	7	6	6	5	5	5	4	4	4	3	3
13	35	32	29	26	23	21	18	16	15	13	12	11	10	10	10	10	10	9	8	8	7	7	6	6	5	5	4	4	4	3
14	37	35	32	29	26	23	21	19	17	15	14	12	12	11	11	11	11	10	9	9	8	7	7	6	6	5	5	4	4	4
15	39	37	35	32	29	26	23	21	19	17	15	14	13	13	12	12	12	11	10	9	9	8	7	7	6	6	5	5	4	4
16	40	39	38	35	32	29	26	24	21	19	17	16	15	14	14	14	13	13	11	10	10	9	8	7	7	6	6	5	5	4
17	39	40	39	38	35	33	30	27	24	22	19	18	17	16	16	16	15	14	13	12	11	10	9	8	7	7	6	6	5	5
18	37	39	40	39	38	36	33	30	27	24	22	20	19	18	18	18	17	16	14	13	12	11	10	9	8	7	7	6	6	5
19	35	37	39	40	39	38	36	33	30	27	25	23	21	20	20	20	19	18	16	15	13	12	11	10	9	8	7	7	6	6
20	32	35	37	39	40	39	38	36	33	31	28	26	24	23	23	23	22	20	18	17	15	13	12	11	10	9	8	7	7	6
21	29	32	35	37	39	40	40	38	36	34	31	29	27	26	26	26	25	23	21	19	17	15	14	12	11	10	9	8	7	7
22	25	29	32	35	37	39	40	40	38	37	34	32	30	29	29	29	28	26	24	21	19	17	15	14	12	11	10	9	8	7
23	23	25	28	32	35	37	39	40	40	39	37	35	33	32	32	32	31	29	27	24	22	19	17	15	14	12	11	10	9	8
24	20	23	25	28	32	34	37	39	40	40	39	38	36	35	35	35	34	32	30	27	25	22	20	17	15	14	12	11	10	9
25	18	20	22	25	28	31	34	37	39	40	40	39	38	38	38	38	37	35	33	30	28	25	22	20	17	16	14	12	11	10
26	16	18	20	22	25	28	31	34	37	39	40	40	40	39	39	39	39	38	36	34	31	28	25	22	20	18	16	14	12	11
27	14	16	18	20	22	25	28	31	34	36	38	39	40	40	40	40	40	39	38	36	34	31	28	25	22	20	18	16	14	12
28	12	14	16	18	20	22	25	28	31	34	36	38	39	39	39	39	40	40	40	39	37	34	31	28	25	22	20	18	16	14
29	11	12	14	16	17	20	22	25	28	30	33	35	37	38	38	38	38	39	40	40	39	37	34	31	28	25	22	20	18	16
30	10	11	12	14	15	17	20	22	25	27	30	32	34	35	35	35	36	38	39	40	40	39	37	34	32	28	25	23	20	18

Payoff Table T4: Semi-real, post-shock, Type *y*

Average price of other firms

elling price	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
1	9	7	5	4	4	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0	0
2	12	9	7	6	5	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0
3	18	13	9	7	7	6	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0
4	26	18	13	10	9	8	6	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0
5	35	26	19	14	12	11	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0
6	40	36	27	20	18	16	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0	0	0	0
7	35	40	36	29	26	23	17	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0	0	0
8	25	34	40	38	35	32	25	17	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0	0
9	18	25	34	39	40	39	34	25	18	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0
10	12	17	25	32	35	38	40	34	25	18	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0
11	9	12	17	23	26	29	36	40	35	26	18	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1
12	7	9	12	16	18	20	27	36	40	35	26	18	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1
13	5	7	9	11	12	14	19	26	35	40	35	25	18	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1
14	4	5	6	8	9	10	13	18	26	35	39	35	25	18	12	9	6	5	4	3	3	2	2	2	1	1	1	1	1	1
15	3	4	5	6	7	7	9	13	18	25	34	39	35	25	17	12	9	6	5	4	3	3	2	2	2	1	1	1	1	1
16	3	3	4	5	5	6	7	9	12	18	25	34	39	34	25	17	12	9	6	5	4	3	3	2	2	2	1	1	1	1
17	2	3	3	4	4	4	5	7	9	12	17	25	34	39	34	25	17	12	9	6	5	4	3	3	2	2	2	1	1	1
18	2	2	3	3	3	4	4	5	7	9	12	17	25	34	38	34	25	17	12	9	6	5	4	3	2	2	2	1	1	1
19	2	2	2	3	3	3	3	4	5	7	9	12	17	25	33	38	34	25	17	12	9	6	5	4	3	2	2	2	1	1
20	1	2	2	2	2	2	3	3	4	5	7	9	12	17	24	33	38	34	25	17	12	8	6	5	4	3	2	2	2	1
21	1	1	2	2	2	2	2	3	3	4	5	7	9	12	17	24	33	38	33	25	17	12	8	6	5	4	3	2	2	2
22	1	1	1	2	2	2	2	2	3	3	4	5	6	9	12	17	24	33	37	33	24	17	12	8	6	5	4	3	2	2
23	1	1	1	1	1	1	2	2	2	3	3	4	5	6	9	12	17	24	32	37	33	24	17	12	8	6	5	4	3	2
24	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	12	16	24	32	37	33	24	17	12	8	6	5	4	3
25	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	12	16	23	32	37	33	24	17	12	8	6	5	4
26	1	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	11	16	23	32	37	33	24	17	12	8	6	5
27	1	1	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	11	16	23	32	36	33	24	17	12	8	6
28	1	1	1	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	11	16	23	31	36	32	24	16	11	8
29	1	1	1	1	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	11	16	23	31	36	32	24	16	11
30	0	1	1	1	1	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	11	16	23	31	36	32	24	16

Payoff Table T5: Real, pre-shock, Type *y*

Average price of other firms

elling price	**3**	**6**	**9**	**12**	**15**	**18**	**21**	**24**	**27**	**30**	**33**	**36**	**39**	**42**	**45**	**48**	**51**	**54**	**57**	**60**	**63**	**66**	**69**	**72**	**75**	**78**	**81**	**84**	**87**	**90**
3	9	7	5	4	4	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0	0
6	12	9	7	6	5	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0
9	18	13	9	7	7	6	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0
12	26	18	13	10	9	8	6	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0
15	35	26	19	14	12	11	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0
18	40	36	27	20	18	16	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0	0	0	0
21	35	40	36	29	26	23	17	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0	0	0
24	25	34	40	38	35	32	25	17	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0	0
27	18	25	34	39	40	39	34	25	18	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0
30	12	17	25	32	35	38	40	34	25	18	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0
33	9	12	17	23	26	29	36	40	35	26	18	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1
36	7	9	12	16	18	20	27	36	40	35	26	18	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1
39	5	7	9	11	12	14	19	26	35	40	35	25	18	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1
42	4	5	6	8	9	10	13	18	26	35	39	35	25	18	12	9	6	5	4	3	3	2	2	2	1	1	1	1	1	1
45	3	4	5	6	7	7	9	13	18	25	34	39	35	25	17	12	9	6	5	4	3	3	2	2	2	1	1	1	1	1
48	3	3	4	5	5	6	7	9	12	18	25	34	39	34	25	17	12	9	6	5	4	3	3	2	2	2	1	1	1	1
51	2	3	3	4	4	4	5	7	9	12	17	25	34	39	34	25	17	12	9	6	5	4	3	3	2	2	2	1	1	1
54	2	2	3	3	3	4	4	5	7	9	12	17	25	34	38	34	25	17	12	9	6	5	4	3	2	2	2	1	1	1
57	2	2	2	3	3	3	3	4	5	7	9	12	17	25	33	38	34	25	17	12	9	6	5	4	3	2	2	2	1	1
60	1	2	2	2	2	2	3	3	4	5	7	9	12	17	24	33	38	34	25	17	12	8	6	5	4	3	2	2	2	1
63	1	1	2	2	2	2	2	3	3	4	5	7	9	12	17	24	33	38	33	25	17	12	8	6	5	4	3	2	2	2
66	1	1	1	2	2	2	2	2	3	3	4	5	6	9	12	17	24	33	37	33	24	17	12	8	6	5	4	3	2	2
69	1	1	1	1	1	1	2	2	2	3	3	4	5	6	9	12	17	24	32	37	33	24	17	12	8	6	5	4	3	2
72	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	12	16	24	32	37	33	24	17	12	8	6	5	4	3
75	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	12	16	23	32	37	33	24	17	12	8	6	5	4
78	1	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	11	16	23	32	37	33	24	17	12	8	6	5
81	1	1	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	11	16	23	32	36	33	24	17	12	8	6
84	1	1	1	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	11	16	23	31	36	32	24	16	11	8
87	1	1	1	1	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	11	16	23	31	36	32	24	16	11
90	0	1	1	1	1	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	11	16	23	31	36	32	24	16

Payoff Table T6: Real, post-shock, Type *y*

Average price of other firms

elling price	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30
1	9	7	5	4	4	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0	0
2	12	9	7	6	5	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0
3	18	13	9	7	7	6	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0	0
4	26	18	13	10	9	8	6	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0	0
5	35	26	19	14	12	11	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0	0	0	0	0
6	40	36	27	20	18	16	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0	0	0	0
7	35	40	36	29	26	23	17	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0	0	0
8	25	34	40	38	35	32	25	17	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0	0
9	18	25	34	39	40	39	34	25	18	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0	0
10	12	17	25	32	35	38	40	34	25	18	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1	0
11	9	12	17	23	26	29	36	40	35	26	18	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1	1
12	7	9	12	16	18	20	27	36	40	35	26	18	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1	1
13	5	7	9	11	12	14	19	26	35	40	35	25	18	12	9	7	5	4	3	3	2	2	2	1	1	1	1	1	1	1
14	4	5	6	8	9	10	13	18	26	35	39	35	25	18	12	9	6	5	4	3	3	2	2	2	1	1	1	1	1	1
15	3	4	5	6	7	7	9	13	18	25	34	39	35	25	17	12	9	6	5	4	3	3	2	2	2	1	1	1	1	1
16	3	3	4	5	5	6	7	9	12	18	25	34	39	34	25	17	12	9	6	5	4	3	3	2	2	2	1	1	1	1
17	2	3	3	4	4	4	5	7	9	12	17	25	34	39	34	25	17	12	9	6	5	4	3	3	2	2	2	1	1	1
18	2	2	3	3	3	4	4	5	7	9	12	17	25	34	38	34	25	17	12	9	6	5	4	3	2	2	2	1	1	1
19	2	2	2	3	3	3	3	4	5	7	9	12	17	25	33	38	34	25	17	12	9	6	5	4	3	2	2	2	1	1
20	1	2	2	2	2	2	3	3	4	5	7	9	12	17	24	33	38	34	25	17	12	8	6	5	4	3	2	2	2	1
21	1	1	2	2	2	2	2	3	3	4	5	7	9	12	17	24	33	38	33	25	17	12	8	6	5	4	3	2	2	2
22	1	1	1	2	2	2	2	2	3	3	4	5	6	9	12	17	24	33	37	33	24	17	12	8	6	5	4	3	2	2
23	1	1	1	1	1	1	2	2	2	3	3	4	5	6	9	12	17	24	32	37	33	24	17	12	8	6	5	4	3	2
24	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	12	16	24	32	37	33	24	17	12	8	6	5	4	3
25	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	12	16	23	32	37	33	24	17	12	8	6	5	4
26	1	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	11	16	23	32	37	33	24	17	12	8	6	5
27	1	1	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	11	16	23	32	36	33	24	17	12	8	6
28	1	1	1	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	11	16	23	31	36	32	24	16	11	8
29	1	1	1	1	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	11	16	23	31	36	32	24	16	11
30	0	1	1	1	1	1	1	1	1	1	1	1	1	2	2	2	3	3	4	5	6	8	11	16	23	31	36	32	24	16