Matching Friction, Coordination and Monetary Nonneutrality

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Abstract

This paper theoretically and experimentally investigates matching friction as a cause for monetary nonneutrality. We design a price-posting game where miss-matching (i.e., the player not being matched with a trading partner) is costly and can only be eliminated if players coordinate to play one equilibrium from a specific subset of multiple equilibria. In the lab, subjects coordinate by certain pricing and visiting rules and their actions appear to be consistent with such an equilibrium. The nominal shock disturbs the established coordinating patterns and, in particular, causes sellers to adjust the price targets in their pricing rule. The adjustment is persistent and differs at a disaggregate level in a way that the resulting nonneutrality gets along with the quantity theory (i.e., the aggregate price level is proportional to the aggregate nominal stock).

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1 Introduction

Neutrality of money, a well-established proposition in economics at least since Hume [16], asserts that a money injection, a purely nominal change, affects only nominal variables in the economy, provided that the injection is public information and changes each person's money holdings proportionally. As a pillar of monetary economics, the proposition is easily understood by laymen. But would laymen act as the proposition prescribes if they face such an injection in reality? We address this question by an experiment with two realistic features: (a) money serves as a transactional tool and (b) the trade process is subject to matching friction. Facing matching friction, people desire to coordinate in order to avoid miss-matching. On the theoretical ground, it is sensible to study coordination by a game that admits multiple equilibria. An experiment based on such a game is valuable as theory does not tell which equilibrium is selected, letting alone whether the nominal changes may affect the manner by which people coordinate.

Our experiment is built on a variant of the price-posting game of Burdett et al. [3]. There are equal numbers of buyers and sellers in a market. Each seller has a unit of an indivisible good to serve one buyer. Buyers hold an equal number of tokens valuable to sellers at the end of the game. Sellers move first to post prices in tokens. Observing the prices, buyers enter a matching stage in which each buyer chooses a seller to visit. Being visited, a seller is randomly matched with one of the visiting buyers and trades the good with that buyer at the posted price; players who miss matching in the current stage enter the next matching stage; and the game is over after all goods are sold. The market is *frictional* when waiting to trade in a matching stage is costly. By controlling the waiting cost, we control the degree of matching friction or the miss-matching cost. The market becomes *frictionless* when waiting to trade is costless, permitting treatment comparisons to identify causal effects of matching friction in the experiment. In the frictionless market, it is the dominant strategy for each seller to post the price equal to token holdings per buyer. The frictional market admits multiple equilibria.

In each main treatment, subjects play multiple rounds of the above game, i.e., the supergame; the supply of tokens increases at a mid round; subjects know the injection of tokens when it occurs; and the total real value of tokens at the end of each round and identities of sellers and buyers are fixed across different rounds. We choose the parameter values so that the supergame has an equilibrium (among many others) in which the posted price can be related to a focal point, and the buyer's strategy is psychologically sensible—just revisit the same seller as long as the buyer encounters no miss-matching in the last round and sellers post the same price. This equilibrium eliminates miss-matching (after a few rounds of play) and subjects may be much willing to avoid miss-matching. Of course, only the experiment can tell whether subjects can coordinate to play such an equilibrium. We have two basic hypotheses: (i) In the frictionless market, the injection of tokens does not have persistent effects; and (ii) In the frictional market, the injection has persistent effects by disturbing the established coordinating pattern.

Both hypotheses are confirmed in the lab. Let us focus on three related findings from the frictional market. First, subjects find ways to coordinate. A seller has a target for his real price (i.e., the price normalized by the number of tokens) which is closed to a focal point, and he moves gradually toward his target. A buyer is more likely to first visit the seller who is previously first visited by and matched with the buyer if the seller's current real price deviated less from the previous real price. Secondly, injections disturb coordination in the short run. A seller tends to post a real price lower than his target, and a buyer tends to rely less on the previous matching outcome to guide his current visiting. Lastly, injections have persistent real effects at a disaggregate level. We find three groups of sellers with largely equal sizes: an under-responding group for which the average prices do not fully absorb injected tokens at late rounds; an over-responding group for which the average prices overshot; and a normal group for which neutrality is regained. The under-responding and over-responding groups change their price targets after injections. Because the average prices following injections gradually absorb the nominal changes, this disaggregate-level nonneutrality actually goes along with the quantity theory (i.e., the aggregate price level is proportional to the aggregate nominal stock).

To summarize, we design an experiment with certain realistic features of the monetary transactions. Coupled with psychological reasoning that selects a specific equilibrium (from multiple equilibria) which eliminates miss-matching, economic theory seems to have predictive power in the lab. But subjects respond to nominal changes by a way that departs from any theoretical prediction and leads to the aforementioned disaggregate-level nonneutrality, which, in fact, is the takeaway point from our experiment.

The related literature

In the experimental literature, Fehr and Tyran [13, 14] provide two influential studies of people's responses to one-shot nominal changes. In their games, a firm is given a payoff function in which a parameter represents the nominal stock and the firm chooses an input representing its nominal price simultaneously with other firms. Nominal rigidity is attributed to subjects' concern that others might be subject to money illusion (i.e., mistreat nominal payoffs as real payoffs) in Fehr and Tyran [13] and to one's own price being complementary to others' in the payoff function in Fehr and Tyran [14].[1]

Different from the experiments of Fehr and Tyran [13, 14], our experiment emphasizes a transactional role of money. A transaction in our game, as many daily transactions, takes place after a seller is matched with a buyer (who does not have a counterpart in the games of Fehr and Tyran [13, 14]). Following Burdett et al. [3] and, more generally, the directed-search literature, we let the matching outcome be endogenized through the buyer's visiting and the seller's pricing decisions, rendering miss-matching a key factor that affects those decisions. The literature (experimental or not) rarely relates miss-matching to monetary nonneutrality; in fact, matching friction in general and miss-matching in specific do not require that transactions be monetary. Drawing from comparison of the frictional and frictionless markets, we find that with monetary transactions, costly miss-matching can cause persistent monetary nonneutrality in the lab by inducing persistent changes in the real-price targets.

Lian and Plott [24] and Duffy and Puzzello [11] also concern about one-shot nominal changes. They both consider money as a medium of exchange and find that one-shot nominal increases are neutral (Duffy and Puzzello [11] find that one-shot nominal decreases are nonneutral and this asymmetry between a nominal increase and decrease echos findings of Fehr and Tyran [13, 14]). The transactional role of money is an emphasis of a body of experimental works (see Duffy [10] for a comprehensive survey). In this body of works, Anbarci et al. [1], Duffy and Puzzello [12], and Jiang et al. [18] are most closely related to our work; these authors focus on welfare implication of *constant inflation* driven by *repeated* lump sum nominal changes, which, as is well

[1] Noussair et al. [23] study the money-illusion channel of Fehr and Tyran [13] in the asset market. Davis and Korenok [9] study the firm's price responses to one-shot nominal changes in a game related to Fehr and Tyran [13, 14] with the friction that some firms either cannot change prices or have imperfect information on the nominal change. Fehr and Tyran [13, 14] and we do not introduce such friction.

known, are nonneutral. Notably, miss-matching is not a factor that affects decision making in Lian and Plott [24] (trade is by double auction), Duffy and Puzzello [11, 12] (buyers and sellers are randomly matched in pairs), and in Jiang et al. [18] (trade is competitive). Anbarci et al. [1] embed the original game of Burdett et al. [3] into the Lagos and Wright [20] model; miss-matching is more costly in their game than in our game because in their game miss-matching drives one out of the market immediately. We intentionally limit the miss-matching cost because among others, it seems more interesting to see that a mild cost can cause nonneutrality.

There is a stream of experimental studies of coordination failure (see, e.g., Van Huyck et al. [26] and Cooper et al. [7]), which builds on the theoretical contribution of Cooper and John [8]. Our experiment adds to this literature that one-shot nominal changes may cause people to coordinate differently in a frictional environment.

2 A price-posting game

Here we first describe and analyze a price-posting game, denoted by G, which is adapted from Burdett et al. [3]. Next we describe how this game is used in our experiment to test neutrality of a purely nominal change.

2.1 The game G

The game G has $2I > 2$ players in a market. A player is either a buyer or a seller. Seller $j \in \{1, ..., I\}$ supplies one unit of an indivisible good. Buyer $k \in \{1, ..., I\}$ demands one unit of the good. A buyer's valuation of the good is u, and a seller's is 0. Each buyer holds M units of tokens. A player's valuation of a token is e.

There are $I + 1$ stages of actions; actions taken at each stage become public at the end of the stage. At stage 0, sellers simultaneously post prices of their goods in tokens. Stage $i \in \{1, ..., I\}$ is a matching stage. Let A_i^b (A_i^s, resp.) be the set of buyers (sellers, resp.) who have not bought (sold, resp.) the good at some stage $i' < i$; a player is *active* at stage i if he is in $A_i^b \cup A_i^s$ and *inactive* otherwise. Being active at stage $i > 1$, a player bears a *waiting cost* $c \in [0, eM/I)$ at that stage. We say that the market is *frictionless* if $c = 0$ and *frictional* if $c > 0$.

In a matching stage, all active buyers simultaneously choose active sellers to visit (inactive players have no actions and do not pay the waiting cost c). If visited, seller

$j \in A_i^s$ is randomly matched with one of the visiting buyers and trades the good with that buyer at the price p_j so that the seller's payoff is $ep_j - (i-1)c$ and the buyer's is $u - ep_j - (i-1)c$.[2]

We set $u = 2eM$ in the game G. As such, the buyer's payoff is equal to the seller's at the price M, which helps avoid the fairness issue in the experiment and, moreover, as discussed below, create a focal point for subjects. Also, because subjects are paid in the local currency and the currency is not perfectly indivisible, we let tokens be indivisible and let e be an integer. This completes the description of the game G.

The game G departs from the game of Burdett et al. [3] in one important aspect. In the latter game, $c = 0$ but there is only one matching stage. In both games, matching friction arises when *miss-matching* (i.e., one is not matched with a trading partner) is costly. In the latter game, miss-matching leads players who are not matched in the unique matching stage to leave the market without trade; in the game G, miss-matching leads active players who are not matched in the current matching stage to pay the waiting cost c in the next matching stage.[3]

The departure serves two purposes. First, although our interest is matching friction, we need a control treatment without any friction for our experiment. The game G allows us to have treatments with and without friction by controlling the waiting cost c. Second, if there is only one matching stage, then a player faces two possible outcomes: trading and no trading. Because a subject should be given substantial payoffs from trading, miss-matching can be rather costly; this may result in risk aversion being a significant factor that affects the subject's decision. The game G makes the miss-matching cost independent of the trade payoffs, helping control the effect of risk aversion on the subject's decision making.

Analysis of G

A pure strategy of seller j is $p_j \in \{0, ..., M\}$. A behavioral strategy of seller j is a distribution μ_j over the set $\{0, ..., M\}$. Let $\mathbf{p} = (p_1, ..., p_I)$; let $h^i = (\mathbf{p}, A_1^b, A_1^s, ..., A_i^b, A_i^s)$ denote a history up to the start of stage $1 \le i \le I$; and let H^i be the set of all possible

[2] In this setting, sellers do not update price across stages. This is meant to capture the observation that in reality prices posted by sellers are not changed during a short period of time.

[3] The two games may reflect different real-life experiences. Think of two persons approaching a taxi simultaneously: the unserved person can approach another taxi later (by bearing a waiting cost) in the game G but cannot in the game of Burdett et al. [3]. For experimental tests of the original game of Burdett et al. [3], see Anbarci et al. [2] and Cason and Noussair [6].

h^i. A pure strategy of buyer k is a mapping $f_k = (f_{ki})_{i=1}^I$ such that f_{ki} assigns to each $h^i \in H^i$ up to the start of stage $i \geq 1$ a seller $f_{ki}(h^i) \in A_i^s$ to visit. A behavioral strategy of buyer k is a mapping $\sigma_k = (\sigma_{k1}, ..., \sigma_{kI})$ such that σ_{ki} assigns to each $h^i \in H^i$ a distribution $\sigma_{ki}(.; h^i)$ whose support is A_i^s. A strategy σ_k of buyer k is *identity-independent* if buyer k's visiting at each stage only depends on prices posted by sellers active at that stage.[4] Our solution concept is subgame perfect equilibrium.

Our first result uses the observation that in the frictionless market, posting $p_j = M$ is the dominant strategy of seller j (but there are many equilibria because buyers have many payoff-equivalent visiting choices).

Proposition 1 *When $c = 0$, there exist many equilibria and in any equilibrium, each seller posts the price equal to M at stage 0.*

Turning to the frictional market, we start with the subgame after some $\mathbf{p}$ has been posted, referred to as the game $G^{\mathbf{p}}$; a strategy $\sigma_k^{\mathbf{p}}$ of buyer k in $G^{\mathbf{p}}$ is identity-independent if it is a restriction of an identity-independent strategy σ_k in G to $G^{\mathbf{p}}$. By the standard fixed-point argument, we have the following.

Lemma 1 *When $c > 0$, there exists a subgame perfect equilibrium $(\sigma_1^{\mathbf{p}}, \sigma_2^{\mathbf{p}}, ..., \sigma_I^{\mathbf{p}})$ of $G^{\mathbf{p}}$ such that each buyer k's strategy $\sigma_k^{\mathbf{p}}$ is identity-independent and equal to $\sigma_1^{\mathbf{p}}$.*

The next result also follows from the standard fixed-point argument.

Proposition 2 *In the frictional market, there exists an equilibrium (μ, σ) such that each seller j's strategy μ_j is equal to μ_1, and each buyer k's strategy σ_k is identity-independent and equal to σ_1.*

Given parameter values, we can explicitly solve equilibria by linear programming. For the values used in our experiment (see section 3), we find multiple Proposition-2 equilibria, each of which has a nondegenerate support of σ_{k1} (i.e., buyers randomize their visiting choices in stage 1), implying that there is miss-matching because some sellers cannot sell their goods at stage 1 with a positive probability; some of these equilibria have a degenerate support of μ_1 (i.e., sellers post the same price) and, in

[4]Formally, σ_k is identity-independent if $\sigma_{ki}(j; h^i) = \sigma_{ki}(j'; h^{i\prime})$ holds provided that p_j in h^i is equal to $p_{j'}$ in $h^{i\prime}$ and that there is a one-to-one mapping ι from the set of active sellers A_i^s in h^i to the set of active sellers $A_i^{s\prime}$ in $h^{i\prime}$ satisfying $p_j = p_{\iota(j)}$ all $j \in A_i^s$.

particular, there are equilibria with supp $\mu_1 = \{p\}$ (i.e., the support of μ_1 is $\{p\}$) for $p = M$ and for $p = M - c/e$.

When a Proposition-2 equilibrium (μ, σ) has supp $\mu_1 = \{p\}$ for some p, there is an accompanying equilibrium which shares the same posted price but eliminates any miss-matching.

Proposition 3 *Let $c > 0$. Let (μ, σ) be a Proposition-2 equilibrium with supp $\mu_1 = \{p\}$ for some p. Then there exists a strategy profile $\sigma' \neq \sigma$ of buyers such that (μ, σ') is an equilibrium and has all goods sold at stage 1.*

Proof. When $p_j \neq p$ some j in $\mathbf{p}$, let $\sigma_k'^{\mathbf{p}}$ be identical to $\sigma_k^{\mathbf{p}}$. When $p_j = p$ all j in $\mathbf{p}$, let supp $\sigma'_{k1} = \{k\}$ (i.e., buyer 1 visits seller 1, buyer 2 visits seller 2, and so on) and let σ'_{ki} be identical to $\sigma^{\mathbf{p}}_{ki}$ for $i \geq 2$. Provided that no seller deviates from posting p in the equilibrium (σ, μ), no seller is to deviate when buyers follow σ'. Also, provided that all sellers post p at stage 0, it is not beneficial for buyer k to deviate from σ'_{k1}. ■

In Proposition 3, the strategy profile σ' specifies buyers to respond to the prices and identities of sellers at stage 1. In particular, when sellers post the "right" price (i.e., p), σ' specifies buyers to make their visiting choices by a one-to-one mapping that connects the identity of each buyer to the identity of a distinct seller. But when sellers post "wrong" prices, σ' specifies buyers in the subgame following the wrong prices to act according to the corresponding identity-independent strategies in σ.

Coordination, multiple equilibria, and focal point

As noted above, the game G with the parameter values used in our experiment has multiple equilibria. A narrative among theorists is that people may coordinate by a focal point when facing multiple equilibria. Because people care fairness a lot (as documented in the experiment literature), sellers in the lab may easily pay attention to the price M (which equalizes the buyer's and seller's payoffs). Starting from thinking M, sellers may end up with posting $M - c/e$. The rationale is that if I post $M - c/e$ and you post M, I can be matched earlier than you and, hence, my payoff is at least no worse than yours. Regardless of which price may be viewed as a focal point by sellers, a seller may use the focal point as the reference and chooses a price around it.

When sellers post prices around the focal point in the lab, buyers ought to be incentivized to coordinate their visiting choices by connecting each buyer's identity to a distinct seller's. But such coordination is difficult in the one-shot game—there are many equally possible ways to make the connection, subjects must choose one way individually, and there is no obvious choice for an individual subject. We discuss how repeated play may help establish connection below.

2.2 Supergame for experiment

We let subjects play $T > 1$ rounds of G in our experiment. We increase the number of tokens per buyer from M to $M' \geq M$ in a mid round, referred to as round τ; without saying otherwise, the injection of tokens is announced at the start of round τ. The underlying game for the experiment the supergame, i.e., the T-round repetition of G, with a sudden injection of tokens at round τ. Because we intend to test neutrality of *the number of tokens*, we increase the exchange rate e' following the token injection to eM/M' and let the new exchange rate be announced at the same time when tokens are injected, implying that the total real value of tokens is constant in the supergame and the token injection is purely nominal.

Two remarks on the exogenous value of all tokens are in order. First, because this exogenous value is constant, one may relate tokens to coins made out of gold when the stock of gold is constant. Our experiment then resembles the scenario that coins are suddenly debased. The debasement is purely nominal—M/M' post-debasement coins are identical to one pre-debasement coin; our experiment tests whether the purely nominal change affects the real allocation. A *real allocation* refers not only to who holds goods at the end of a round of play (each buyer buys one unit of goods in any equilibrium) but also the real value of the number of tokens paid by each buyer and the waiting costs borne by each subject.

Second, playing G by multiple rounds opens a door to endogenize the value of tokens. In a general equilibrium setup which endogenizes the value of tokens, if a purely nominal change affects the current real allocation, then it may affect the future value of tokens, which may in turn have a feedback effect on the current allocation. Our experiment does not pursue this interesting direction in order to focus on the direct responses to one-shot nominal changes.[5]

[5] For experiments which endogenize the value of fiat money, see e.g., Camera et al. [4], Camera and Casari, [5], and Jiang et al. [17].

Analysis of the supergame

It is well known that the repetition of an equilibrium in G is an equilibrium in the supergame, and the supergame can have other equilibria. Among those other equilibria, our interest is a sort that buyers in the frictional market condition their current visiting choices on the previous matching outcomes. Psychologically, a buyer in the lab may rely on those outcomes to convince himself that among many equally possible visiting choices, a specific one is the best. Although those outcomes are not unique, there may be a natural and simple one to serve this purpose—if the buyer is successfully matched with a seller at stage 1 in the last round, then he may say to himself that this choice works in the last round to avoid miss-matching, why shall I try something else now?

To be formal, denote a strategy profile of the supergame by (μ^T, σ^T), where $\mu^T = (\mu(1), ..., \mu(T))$, $\mu(t) = (\mu_1(t), ..., \mu_I(t))$, $\sigma^T = (\sigma(1), ..., \sigma(T))$, $\sigma(t) = (\sigma_1(t), ..., \sigma_I(t))$, $\sigma_k(t) = (\sigma_{k1}(t), ..., \sigma_{kI}(t))$, $\mu_j(t)$ specifies a distribution of prices for seller $j \in \{1, ..., I\}$ to post at stage 0 in round t, and $\sigma_{ki}(t)$ specifies a distribution of sellers for buyer $k \in \{1, ..., I\}$ to visit at stage i in round t. Somewhat abusing notation, given a strategy μ_j in G, we use $\mu_j(t) = \mu_j$ to mean that $\mu_j(t)$ always specifies the same distribution in round t of the supergame as μ_j in G. Likewise, given a strategy σ_k in G, we use $\sigma_{ki}(t) = \sigma_{ki}$ to mean that $\sigma_{ki}(t)$ specifies the same distribution at stage i in round t of the supergame as σ_{ki} in G when the within round-t history of the supergame up to the start of stage i is the same as the history in G up to the start of stage i. Moreover, let $A_b(0) = A_s(0) = \{1, ..., I\}$; let $A_b(t)$ ($A_s(t)$, resp.) be the set of buyers (sellers, resp.) who are not successfully matched at stage 1 in round $t > 0$; and let $v_k(t)$ be the seller who is visited by buyer $k \notin A_b(t)$ in round $t > 0$.

Proposition 4 *Let $c > 0$ and $M' = M$. Let (μ, σ) be a Proposition-2 equilibrium of G with supp $\mu_1 = \{p\}$ for some p. Let μ^T be such that $\mu_j(t) = \mu_j$ all (t, j). Let σ^T be such that for each k, either when $t = 1$ or when some seller does not post p in round $t > 1$, $\sigma_{k1}(t) = \sigma_{k1}$; when all seller post p in round $t > 1$, supp $\sigma_{k1}(t)$ is $\{v_k(t-1)\}$ if $k \notin A_b(t-1)$ and $\sigma_{k1}(t)$ assigns the equal probability to members in $A_s(t-1)$ otherwise; and $\sigma_{ki}(t) = \sigma_{ki}$ all $t \geq 1$ and $i > 1$. Then (μ^T, σ^T) is an equilibrium of the supergame.*

Proof. It suffices to check that at stage 1 in round $t > 1$, buyer $k \in A_b(t-1)$ does not deviate to visit some seller $j \notin A_s(t-1)$. Let $N = \#A_b(t-1)$. If the buyer does not deviate, the probability for him to get matched at stage 1 is $\frac{1}{N^{N-1}}\sum_{L=0}^{N-1}C_L^{N-1}(N-1)^{N-1-L}\frac{1}{L+1}$, which is no less than 0.5; if he deviates, the probability is 0.5. On the other hand, conditional on that he is not matched at stage 1, the probability that there are total n sellers not matched at stage 1 when he does not deviate is equal to the probability that there are total n sellers not matched at stage 1 when he deviates. This completes the proof. ■

In Proposition 4, the strategy σ_k^T specifies that if buyer k is succesfully matched with seller j at stage 1 in the last round, then he revisits seller j at stage 1 in this round, and that if buyer k is not succesfully matched at stage 1 in the last round, then he randomly picks a seller who is not succesfully matched at stage 1 in the last round to visit. For this strategy, it may help think of the scenario that each buyer randomly selects a seller to visit; now if one buyer can commit to visiting a distinct seller, then it is best for everyone's interest to let the specific buyer go to that distinct seller at stage 1 and let other buyers randomly visit other sellers. Notice that the Proposition-4 equilibrium eliminates any miss-matching by at most T rounds of play.

Hypotheses

Suppose subjects play some equilibrium $(\mu(t),\sigma(t))_{t=\tau}^T$ of the supergame from round 1 to round $\tau-1$. Neutrality holds if following the token injection, subjects play an equilibrium $(\mu'(t),\sigma'(t))_{t=\tau}^T$ equivalent to $(\mu(t),\sigma(t))_{t=\tau}^T$ in the sense that pM'/M in $\mu'_j(t)$ and $\sigma'_k(t)$ plays the same role as p in $\mu_j(t)$ and $\sigma_k(t)$ for any nominal price p. Neutrality does not hold if subjects switch to an equilibrium not equivalent to $(\mu(t),\sigma(t))_{t=\tau}^T$. As usual, theory is silent about whether there may be a switching. We have the following hypotheses for our experiment.

In the frictionless market, prior to the token injection, a seller is capable of figuring out that it is optimal to post M in each round and she is going to do so accordingly. Given this, the injection of tokens has no persistent real effect.

In the frictional market, prior to the token injection, subjects may not be capable of figuring out an exact equilibrium (because off-the-equilibrium-path plays incur complicate probabilities). But, as noted above, sellers may post prices around a reference price, which comes from a focal point, and buyers may make their current visiting choices conditional on their last round matching outcomes, which serve a psychological purpose. Therefore, subjects may establish certain coordinating patterns so that they may appear to coordinate on a Proposition-4 equilibrium (μ^T, σ^T) with p equal to the reference price such as M or $M - c/e$. The token injection does not change why sellers view some price as a focal point or why buyers prefer some previous matching outcomes to guide their current visiting choices. Nonetheless, in the absence of common knowledge regarding how each other responds to the injection, the injection can disturb the established coordinating patterns and, hence, have persistent real effects.

3 Details of experiment

In the experiment, we set $I = 6$ in a group and let six buyers and six sellers in the group play the supergame. Subjects are randomly assigned into groups; they are informed about their role (i.e., buyer or seller) and the corresponding identity number; they are informed that their identities remain unchanged throughout the experiment; and subjects observe who buys from whom and at what price in the end of each round.

For the experiment, we set parameter values as follows. The buyer's valuation u of the good is 16, and the valuation eM of tokens held by a buyer is 8. These costs and values are measured in the local currency unit; the local currency for our experiment is RMB and its unit is yuan.[6] Because we intend to examine whether mild matching friction can affect people's behaviors significantly, we set the waiting cost $c = 0.5$ yuans in the frictional market. We choose this value mainly because 0.5 yuans is the smallest denomination most people observed in circulation; the rationale is that psychologically, subjects may deem such a cost "small" but also may not overlook it. At the start of the experiment, subjects are informed that they are going to receive payoffs of T' rounds randomly selected from the total T rounds of play; a subject's payoff of one round of play is his or her payoff at the end of the game G implied by the play in that round.

In main treatments, we set $T = 20$ and $\tau = 11$. Denoting token holdings per buyer M in round t by M_t, then $M_t = 100$ for $1 \leq t \leq 10$ and $M_t = 100 + \delta$ with $\delta \in \{2, 6, 10\}$ for $t \geq 11$. The magnitudes of the token change rate in round 11 are chosen to be close to inflation experienced by subjects in real life; because our choices

[6] When we ran the experiment in Shanghai, 2015, 1 yuan $\approx$ 0.15 USD and the local minimum monthly wage was 2,000 yuans.

are much smaller than those used in the literature, our design also complements the literature by exploring how subjects may respond to small nominal shocks. The main treatments cover the frictional market (c= 0.5) and the frictionless market ($c = 0$). We also design several variant treatments, details of which are given in Appendix A.

For each treatment, prior to the T rounds of formal play, there are five practice rounds that are not counted toward payoffs; M in each practice round is equal to M_1. Prior to the practice rounds, subjects are given an exercise and asked to calculate the payoff to assess whether they understood the rules of the experiment. After the formal play is complete, subjects are given a questionnaire about how they make their choices in the experiment.

The experiment was conducted in a laboratory in Shanghai using Ztree (Fischbacher [15]). The subjects were undergraduate students recruited from a major university in Shanghai. The experimental instructions were in Chinese; see Online Appendix B for the English version. Subjects received a participation fee of 40 yuan, plus payoffs from seven randomly drawn rounds (i.e., $T' = 7$). In the main treatments, there were 636 subjects; there were 3 sessions (96 subjects) for $(c, \delta) = (0, 2)$, 3 sessions (108 subjects) for $(0.5, 2)$, 4 sessions (108 subjects) for $(0, 6)$, 4 sessions (108 subjects) for $(0.5, 6)$, 3 sessions (96 subjects) for $(0, 10)$, and 4 sessions (120 subjects) for $(0.5, 10)$. A between-subjects design was implemented that a subject only participated in the experiment once (i.e., one of the treatments).

4 Findings

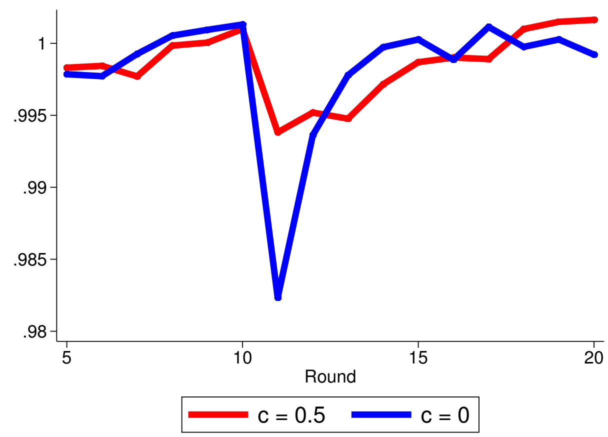
This section reports findings from the main treatments.

4.1 Prices and payoffs

Here we examine prices and payoffs and how they are affected by injections of tokens in the main treatments. We find it convenient to present an individual seller's price in the real form, i.e., normalized by the stock of tokens. Formally, the *real price* ϕ_{jt} of seller j in round t is the nominal price normalized by the ratio of M_t to M_{10}, i.e.,

$$\phi_{jt} = \frac{p_{jt}}{M_t} \times M_{10}, \tag{1}$$

Figure 1: Paths of change rates $\pi_t(S_c)$ of average real prices



where p_{jt} is the price posted by seller j in round t. Although each individual seller's real price and nominal price differ only if $t \geq 11$ (note $\phi_{jt} = p_{jt}$ if $t \leq 10$), for expositional convenience we define the *change rate of the real price of seller j from round* 10 *to round t* for all $t \geq 1$ as

$$\pi_{jt} = \frac{\phi_{jt}}{\phi_{j10}}; \tag{2}$$

note that $p_{jt} = p_{j10}M_t/M_{10}$ if $\pi_{jt} = 1$. Given a set S of sellers, let

$$\pi_t(S) = \frac{\sum_{j\in S} \phi_{jt}}{\sum_{j\in S} \phi_{j10}} \tag{3}$$

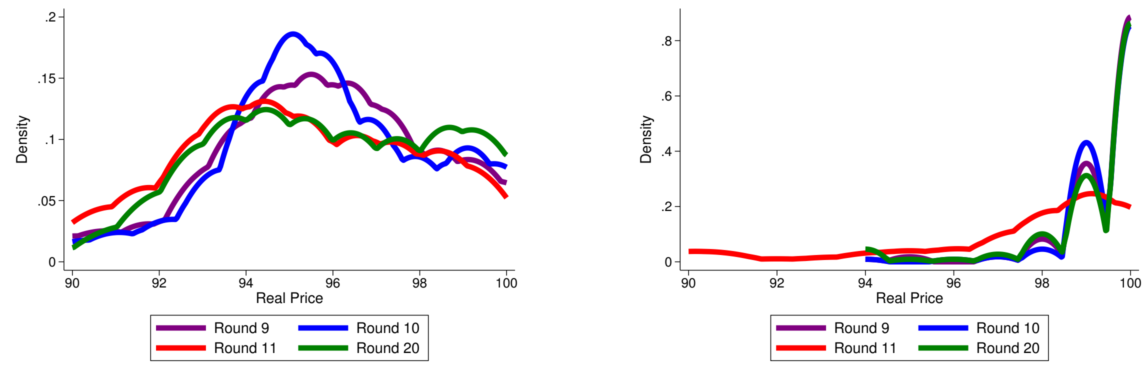
denote the *change rate of the average real prices of sellers in S from round* 10 *to round t*. If $\pi_t(S)$=1 then the average real price posted by sellers in S in round $t \geq 11$ is M_t/M_{10} times of the average prices posted in round 10, meaning that overall, sellers in S have adjusted their prices at t to absorb the increase in the stock of tokens.

Aggregate-level data

Figure 1 displays two paths of the change rate $\pi_t(S_c)$ of the average real prices from round 10 to t (see (3)), where S_c denotes the set of sellers who participate in the main treatments with $c \in \{0, 0.5\}$. Pooling sellers from treatments with the same c is meant to reflect the overall response of sellers to token injections by controlling the nature of the market; we discuss effects of injection sizes below. There are four

Figure 2: Distributions of real prices

(a) $c = 0.5$ (b) $c = 0$



notable patterns of the two paths in Figure 1. First, $\pi_t(S_c)$ moves toward unity as t moves to 10 from the left for each c; that is, the average of prices posted by sellers in S_c appears to settle down (i.e., do not vary much from round to round) before tokens are injected. Second, $\pi_t(S_c)$ obviously drops at $t = 11$ for each c; that is, the average of nominal prices respond sluggishly right after tokens are injected. Third, $\pi_t(S_c)$ moves toward unity again as t moves to 20 for each c; that is, the average of nominal prices gradually moves up to absorb the injected tokens. Fourth, the average of real prices move down more deeply at $t = 11$ when $c = 0$ than when $c = 0.5$,[7] and move back to the pre-injection level more quickly when $c = 0$ than when $c = 0.5$. Remarkably, in the variant treatments with no nominal change (see Appendix A), $\pi_t(S_c)$ is almost a horizontal line for each value of c.

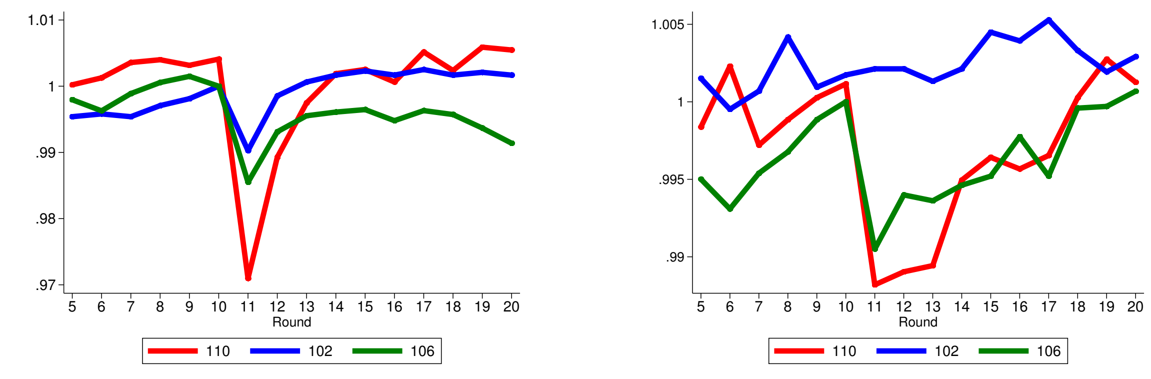
Formally, we examine whether the average of real prices of sellers in S_c in round $t \geq 6$ ($t \neq 10$) is significantly different from the average in round 10 by the two-sample t-test; throughout, statistical significance refers to a p-value no greater than 0.05. When $c = 0.5$, the average of real prices at $t \in \{11, 12, 13\}$ is significantly lower than the average in round 10; there is no significant difference for other t. When $c = 0$, the average of real prices at $t \in \{11, 12\}$ is significantly lower than the average in round 10; there is no significant difference for other t. Statistics of tests for main treatments not present in this section are reported in Appendix B.

The average real price may not tell the whole story because the distribution of

[7]This, however, may not be robust as it is not observed in the the relevant variant treatments reported in Appendix A.

Figure 3: Paths of change rates $\pi_t(S_{c,\delta})$ of average real prices

(a) $c = 0$ (b) $c = 0.5$



real prices is not degenerate. Denote by $\Phi_t(S)$ the distribution of real prices of sellers in S in round t. Figure 2 displays $\Phi_t(S_c)$ for $t \in \{9, 10, 11, 20\}$ and $c \in \{0, 0.5\}$. For each c, $\Phi_9(S_c)$ and $\Phi_{10}(S_c)$ are similar, while $\Phi_{11}(S_c)$ obviously shifts away from $\Phi_{10}(S_c)$. And, $\Phi_{20}(S_c)$ appears to be closer to $\Phi_{10}(S_c)$ when $c = 0$ than when $c = 0.5$. We use the two-sample Kolmogorov-Smirnov test to compare $\Phi_t(S_c)$ with $\Phi_{10}(S_c)$ for $t \geq 6$ ($t \neq 10$) and $c \in \{0, 0.5\}$. When $c = 0.5$, there is significant difference only at $t \in \{11, 12, 13, 14\}$; when $c = 0$, there is significant difference only at $t \in \{11, 12\}$. In general, test results based on distributions are consistent with test results based on averages.

Figure 3 displays the paths of $\pi_t(S_{c,\delta})$, where $S_{c,\delta}$ is the subset of sellers in the set S_c who participate in the treatment with the injection size $\delta \in \{2, 6, 10\}$ (i.e., with $M_{11} = 100 + \delta$). Observe that the statistic $\pi_t(S_{0.5,2})$ is persistently above unity at $t \geq 11$, which may be attributed to some participant-specific effects as $\pi_t(S_{0.5,2})$ tends to be above unity for $t \leq 10$. In the formal test, the average of real prices at round 11 is significantly lower than the average of real prices at round 10 for all (c, δ) except $(0, 2)$ and $(0.5, 2)$; the average of real prices goes back to the round-10 level 3 rounds after injections for $(0.5, 10)$, 2 rounds for $(0.5, 6)$, 1 round for $(0, 10)$, and 7 rounds for $(0, 6)$, while no nominal rigidity is observed for $(0.5, 2)$ and $(0, 2)$. At round 20, there is no cross-size difference in the average of real prices for each c. The test for the distributions of real prices has largely consistent outcomes. Overall, nominal rigidity is more likely to be observed for a larger size injection; the size effect declines across time and is not significant towards the end.

Also in the formal test, injections affect the average and distribution of sellers' payoffs at round $t \in \{11, 12\}$ and the effect in general disappears at $t > 12$ in each market; the test results for buyers are largely consistent with the results for sellers.

Result 1 In both frictional and frictionless markets, overall prices respond to token injections sluggishly in the *short run* (i.e., the early rounds following injections); prices fully absorb injected tokens in the *long run* (i.e., the late rounds following injections). The absorbing process is quicker on the frictionless market. Nominal rigidity is more likely to be observed for a larger size injection while the size effect is not significant in the long run. Finally, injections affect payoffs for buyers and sellers in the short run but do not in the long run.

Disaggregate-level data

In the frictionless market, the distribution $\Phi_t(S_0)$ of real prices is much concentrated for most t. Indeed, more than 60% of sellers post prices equal to M_t and around 25% post prices equal to $M_t - 1$ at $6 \leq t \leq 10$ and $t \geq 14$. Moreover, more than 60% of sellers have π_{jt} (see (2)) fall in the interval $(0.995, 1.005)$ and around 85% have it fall in the interval $(0.99, 1.01)$ in round $t \geq 16$ (i.e., the real price posted by seller j in round $t \geq 16$ is very close to the real price posted by j in round 10).

The distribution $\Phi_t(S_{0.5})$ of real prices in the frictional market is more diverse. Even so, prices posted before injections of tokens are centered around $M - c/e$ (which is equal to 93.75) and more than 58% of prices fall between 92 and 96; this seems consistent with our hypothesis that $M - c/e$ may serve as a focal point for sellers.[8] How does a seller respond to an injection of tokens in the long run? One may conjecture from Figure 1 that a seller has a tendency to set the real price in round 11 lower than the real price in round 10, and this tendency gradually disappears as t moves toward 20—the seller sets the real price in late rounds comparable to that in round 10. To examine this conjecture, let Π_t denote the distribution of π_{jt} for all $j \in S_{0.5}$. Let

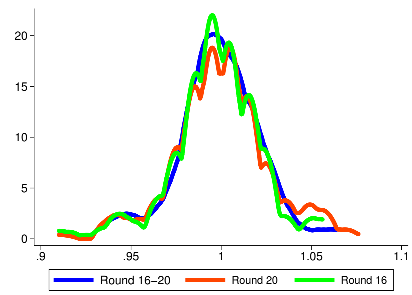
$$\bar{\pi}_j = \sum_{t=16}^{20} \pi_{jt}/5 \tag{4}$$

and let Π denote the distribution of $\bar{\pi}_j$ for all $j \in S_{0.5}$. If the conjecture is correct, then

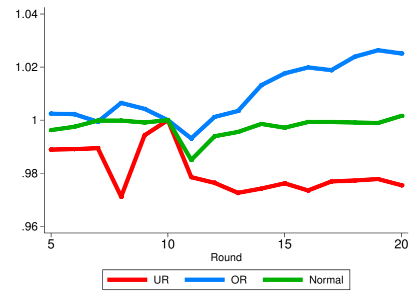
[8] Although $\Phi_t(S_{0.5})$ is more disperse than $\Phi_t(S_0)$, it is far distant from the distribution in any Proposition-2 equilibrium when sellers randomize prices.

Figure 4: Classification of seller response types in $S_{0.5}$: Base and consequence

(a) Distribution of Real Price Change Rate



(b) Paths of Real Price Change Rate from Different Types of Sellers



a seller should not systematically set his real prices in late rounds either consistently lower or consistently higher than the real price in round 10; hence, Π should be more concentrated than Π_t. But this is *not* the case. Figure 4 (a) displays the distributions Π_{16}, Π_{20}, and Π. It is quite striking how closely the three distributions trace each other; that is, Π_t settles as t approaches 20 while Π_t is no more dispersed than Π. This striking pattern suggests that there is persistency in the individual price adjustment process.

Because we observe only 10 prices for each seller following a token injection, we do not directly test the individual persistency. Instead, we take the hint from Figure 4 (a) and classify a seller's *response type* according to his position in the distribution Π. Specifically, seller j is an *under-responding* (UR hereafter) seller if $\bar{\pi}_j$ is in bottom 1/3 of Π, an *over-responding* (OR hereafter) seller if $\bar{\pi}_j$ is in top 1/3 of Π, and a *normal* seller otherwise. Figure 4 (b) displays the paths of $\pi_t(S)$ for the three sets of sellers in $S_{0.5}$: the OR sellers have the smallest drop in the real price at round 11 and keep raising the real price as time goes on; the UR sellers make the largest drop in the real price at round 11 and do not move much from there; and the Normal sellers adjust the real price back the pre-injection level quickly and maintain it to the end.

Table 1 reports the differences in average real prices before and after injections for each type and the cross-type differences in the average real prices before and after injections (using the two-sample t-test). In the table, the *pre-injection average* for a given type is the average of real prices from *rounds 6 to 10,* and the *post-injection average* is the average from *rounds 16 to 20.* We use rounds 6 and 16 as the starting rounds for pre-injection and post-injection averages, respectively, because the

Table 1: Average real prices

Average	Pre-injection	Post-injection	Post-Pre
UR	95.14	93.93	-1.21***
OR	95.14	97.07	1.93***
Normal	96.43	96.47	0.04
Difference in average	**Pre-injection**	**Post-injection**	**Post-Pre**
UR vs. Normal	-1.29***	-2.53***	-1.25***
OR vs. Normal	-1.29***	0.60***	1.89***
UR vs. OR	0.01	-3.14***	-3.14***

Notes: Here and below, a statistic with *, **, *** denotes a corresponding p-value no greater than 10%, 5%, and 1 %, respectively; Post-Pre stands for the difference between a post-injection value and the corresponding pre-injection value.

Table 2: Average payoffs

Average	Pre-injection	Post-injection	Post-Pre
UR	7.37	7.35	-0.02
OR	7.37	7.50	0.12***
Normal	7.51	7.57	0.07
Difference in average	**Pre-injection**	**Post-injection**	**Post-Pre**
UR vs. Normal	-0.14***	-0.22***	-0.08*
OR vs. Normal	-0.14***	-0.08*	0.06
UR vs. OR	-0.01	-0.15***	-0.15***

distributions of prices are stable from round 6 prior to injections and from 16 after injections. Observe that (i) the UR, Normal, and OR sellers, respectively reduce, maintain, and raise real prices in the long run after injections, (ii) there are cross-type differences in real prices before and after injections, and (iii) injections increase the cross-type differences in the long run.

In terms of welfare, payoffs instead of prices provide a direct measurement. Table 2 reports the differences in the average payoffs before and after injections for each type and the cross-type differences in the average payoffs (also using the two-sample t-test), where pre-injection and post-injection averages cover the same rounds as those in Table 1. In the long run, injections are non-neutral in payoffs for the OR sellers and increase the difference in payoffs between the UR and non-UR sellers.

The proportions of the UR and OR sellers in $S_{0.5,\delta}$ are $(0.30, 0.28)$, $(0.43, 0.39)$, and $(0.32, 0.23)$ when δ is equal to 2, 6, and 10, respectively. The general messages in Tables 1 and 2 carry over for cross-type comparisons within each $S_{0.5,\delta}$. Referring to the path of change rates $\pi_t(S_{0.5,2})$ of average real prices in Figure 3 (b), one can see that even when the aggregate-level data suggest both short-run and long-run neutrality, long-run nonneutrality can occur at a disaggregate level.

Result 2 In the frictionless market, pricing behaviors are largely consistent with theory with the exception of the two or three rounds following token injections, and, in particular, neutrality is largely regained in the long run. In the frictional market, there exist cross-type differences in real prices both before and after token injections, and injections are nonneutral in the long run as they lead to significant changes in the cross-type differences.

4.2 Pricing and visiting patterns

Here we examine the pricing and visiting patterns by three regressions. For all these regressions, we use the data from round 6 to 20 (i.e., t ranges from 6 to 20). To distinguish between the short-run and long-run effects of injections, each regression contains two period dummies, D_1 and D_2: D_1 is equal to 1 if $11 \leq t \leq 15$ and 0 otherwise; D_2 is equal to 1 if $16 \leq t \leq 20$ and 0 otherwise. To capture the effects due to different sizes of injections, each regression also contains two size dummies, Size06 and Size10: Size06 is equal to 1 if j is in a treatment with $\delta = 6$ and 0 otherwise; Size10 is equal 1 if j is in a treatment with $\delta = 10$ and 0 otherwise. Thus, effectively, each regression contains 6 dummies, namely, D_1, D_2, $D_1(06) \equiv \text{Size06} \times D_1$, $D_1(10) \equiv \text{Size10} \times D_1$, $D_2(06) \equiv \text{Size06} \times D_2$, and $D_2(10) \equiv \text{Size10} \times D_2$. Without saying otherwise, the findings present below all pertain to the regressions for the frictional market (i.e., for all j in $S_{0.5}$).

The buyer's visiting pattern

Our first regression is an OLS regression of seller j's *matching stage* n_{jt} (i.e., j is matched with a buyer at stage n_{jt}) in round t on the constant, the real price ϕ_{jt} (see (1)), the real-price change $\gamma_{jt} \equiv \log(\phi_{jt}/\phi_{jt-1})$, the square of the real-price change γ_{jt}^2, and the interactions between these variables with the six dummies. The outcome of this regression is reported in Table 3. Notably, the higher real price leads a seller to stay longer in the market. Moreover, if the seller makes a larger increase in the real

Table 3: Determinants of seller's matching stage

	Dependent variable: Seller's matching stage n_{jt}
Real Price	0.0445***
	(0.0085)
Real Price Change	2.3659***
	(0.6674)
[Real Price Change]^2	-1.1521***
	(0.3552)
$D_1(06)\times$Real Price Change	-4.7564**
	(1.9163)
$D_2(06)$	-3.4267*
	(2.0619)
$D_2(06)\times$Real Price	0.0355*
	(0.0214)
$D_1(10)$	-3.9896*
	(2.0468)
$D_1(10)\times$Real Price	0.0431**
	(0.0214)
Constant	-2.8385***
	(0.8136)
Control	Yes
Observations	2,518
R-squared	0.0771

Notes: Other variables, which have p-values greater than 10% and are not reported above, include D_1, D_2, and the interactions of each of these two dummies with ϕ_{jt}, γ_{jt}, and γ_{jt}^2; $D_1(06)$ and its interactions with ϕ_{jt} and γ_{jt}^2; the interactions of $D_2(06)$ with γ_{jt} and γ_{jt}^2; $D_2(10)$ and its interactions with γ_{jt} and γ_{jt}^2. Recall that statistical significance refers to a p-value no greater than 5%.

Table 4: Determinants of buyer's stage-1 visit

	Dependent variable: Buyer's stage-1 visit v_{jkt}
Previous Stage-1 Matching	1.0517***
	(0.0423)
Previous Stage-1 Matching$\times$ [Real Price Change]^2	-26.3326**
	(12.7451)
$D_1\times$[Real Price Change]^2	-70.8882**
	(35.0420)
$D_1(06)\times$[Real Price Change]^2	70.9870**
	(35.1006)
Constant	-0.6539**
	(0.3100)
Control	Yes
Pseudo R-squared	0.0655
Observations	20,112

Notes: Other variables, which have p-values greater than 10% and are not reported above, include D_1, $D_1(06)$, and the interactions of each of these two dummies with ϕ_{jt}, γ_{jt}, and m_{jkt-1}; $D_1(10)$, D_2, $D_2(06)$, $D_2(10)$, and the interactions of each of these four dummies with ϕ_{jt}, γ_{jt}, γ_{jt}^2, and m_{jkt-1}.

price, she has a longer stay (although the coefficient for γ_{jt}^2 is negative, quantitatively the contribution of γ_{jt}^2 is dominated by the contribution of γ_{jt} because the absolute value of γ_{jt} is small).

Our second regression is a Probit regression of the *buyer's stage-1 visit* v_{jkt} on the constant, the real price ϕ_{jt}, the real-price change γ_{jt}, the square of the real-price change γ_{jt}^2, the previous stage-1 matching m_{jkt-1}, $m_{jkt-1}\times\gamma_{jt}$, $m_{jkt-1}\times\gamma_{jt}^2$, and the interactions between these variables with the six dummies. Here $v_{jkt} = 1$ if k visits j in stage 1 in round t and 0 otherwise for each pair of buyer k and seller j belonging to the same group; and $m_{jkt-1} = 1$ if buyer k is matched with seller j in stage 1 in round $t-1$ and 0 otherwise. [9]

The outcome of this regression is reported in Table 4. Notably, a buyer is more likely to visit a seller in stage 1 if they are matched in stage 1 in the last round; the likelihood to visit is reduced if the seller posts a real price different from the price

[9]If we define previous matching by including sellers who are matched with the buyer in stage i with $i \geq 2$ in the last round instead of just stage 1, the relevant independent variable on previous matching is not significant.

in the last round, irrespective of whether the change is positive or negative. The buyer's reduction in the visiting probability when the seller raises her real price may be attributed to that the buyer understands the direct impact on his payoff from a higher real price; this reduction is consistent with the finding in Table 3 that the seller stays longer when she increases the real price more. But why does the buyer also reduce his visiting probability when the seller cuts her real price? A plausible rationale is that he sees a lower real price as an incentive for other buyers to visit the seller more, causing a higher miss-matching probability for him.

Putting the findings from the two regressions together, one may see that a buyer uses the last-round stage-1 matching to connect himself to a seller in the present round. Specifically, when a seller raises her real price, the buyer matched with her at stage 1 in the last round reduces his current stage-1 visiting probability, effectively extending the seller's stay in the market (when the seller cuts her real price, the buyer also reduces the visiting probability but this reduction does not extend the seller's stay).

Injections disturb this connecting means in the short run but not in the long run. Indeed, the pre-injection average time (in terms of the number of matching stages) that a seller stays in the market is 1.73 and is 1.93 in round 11; it takes 3 rounds for the staying time to return to the pre-injection level. It is worth noting that for the short-run effects, the magnitude of the real-price change unconditional on the previous matching outcome becomes a factor that contributes to the buyer's stage-1 visit and the injection size is relevant.

Result 3 In the frictional market, a higher real price and a larger increase in the seller's real price lead a seller to stay longer; stage-1 matching in the last round serves as a coordination device for the buyer's present-round visiting; and the buyer relies less on the coordination device the more the seller changes the real price. The above patterns are affected by injections of tokens in the short run and the injection size is relevant for the short-run effects.

The seller's pricing pattern

Our third regression is an OLS regression of the seller's *real price change* γ_{jt} on the constant, the previous real price ϕ_{jt-1}, the previous price difference $\phi_{jt-1} - \overline{\phi}_{\text{-}jt-1}$, and the interactions between these variables with the six dummies. Here $\overline{\phi}_{\text{-}jt-1}$ is the average real price for all five other sellers in the same group as seller j at $t-1$.

Table 5 reports the outcome of this regression separately applied to UR, OR, and Normal sellers. Notably, the coefficients for Previous Real Price and Previous Price Difference are of the same order of magnitude. Because the absolute values of ϕ_{jt-1} and $\phi_{jt-1} - \overline{\phi}_{\text{-}jt-1}$ are around 100 and 1, respectively, the seller's current real-price change (i.e., the level of γ_{jt}) is largely determined by ϕ_{jt-1} and the constant. Removing $\phi_{jt-1} - \overline{\phi}_{\text{-}jt-1}$ from the regression and omitting all dummy related terms, we have $\gamma_{jt} = \alpha + \beta\phi_{jt-1}$, saying that j stops changing the real price (i.e., $\gamma_{jt} = 0$) when the previous price attains $-\alpha/\beta$. This suggests a simple *pricing rule*—the seller has a real-price target $-\alpha/\beta$ and adjusts the current real price at a rate equal to the β proportion of the previous real price. Not jumping to the target immediately can be understood as an adaptation to the environment where a great increase in the real price would prolong the seller's stay in the market (see Result 3).

To continue, we focus on the targets of the real price. Before injections of tokens, the target values implied by the estimated coefficients are 95.40, 94.92, and 97.00 for the UR, OR, and Normal sellers, respectively.[10] Recall that the pre-injection average real prices of these three types are 95.14, 95.14, and 96.43 (see Table 1). All these values are seemingly consistent with our position that sellers may use $M - c/e$, which is equal to 96, as the reference price. Injections exhibit many type-specific and size-specific effects in the short run and in the long run. What concerns us the most are the long-run price targets.

Employing the Wald test, we compare the long-run post-injection target with the pre-injection target for each type, taking the size effect into consideration; the outcome is reported in Table 6. For the Normal sellers, the long-run target is not significantly different from the pre-injection target. For the OR sellers, the long-run target is significantly different from the pre-injection target. For the UR sellers, the long-run target is significantly different from the pre-injection target except for the treatment with the injection size $\delta = 02$ (that exception is weakly significant).

Result 4 In the frictional market, there is a pre-injection real-price target consistent with the reference price $M - c/e$ for each type of sellers; injections affect the real-price targets for OR and UR sellers in the long run.

[10] Because of rounding, these values may differ from the values computed by the coefficient values displayed in Table 5.

Table 5: Determinants of seller's real-price change

	Dependent variable: Seller's real-price change γ_{jt}		
	(1) UR	(2) OR	(3)Normal
Constant	1.2122***	0.4137***	1.1216***
	(0.0596)	(0.0821)	(0.0872)
Previous Real Price	-0.0127***	-0.0044***	-0.0116***
	(0.0006)	(0.0009)	(0.0009)
Previous Price Difference	-0.0022***	-0.0019***	-0.0089***
	(0.0006)	(0.0007)	(0.0009)
D_1	-0.8827***	0.0409	-0.8728***
	(0.1748)	(0.1611)	(0.1826)
D_2	-0.7473***	0.0382	-0.6900***
	(0.1611)	(0.1759)	(0.1797)
$D_1\times$Previous Real Price	0.0092***	-0.0003	0.0090***
	(0.0019)	(0.0017)	(0.0019)
$D_2\times$Previous Real Price	0.0078***	-0.0002	0.0072***
	(0.0017)	(0.0018)	(0.0019)
$D_1\times$Previous Price Difference	-0.0004	0.0005	0.0043**
	(0.0019)	(0.0011)	(0.0021)
$D_1(06)$	0.1762	1.3135***	0.6436**
	(0.2146)	(0.1650)	(0.2675)
$D_1(06)\times$Previous Real Price	-0.0019	-0.0137***	-0.0066**
	(0.0023)	(0.0017)	(0.0028)
$D_1(06)\times$Previous Price Difference	0.0019	-0.0022*	0.0034
	(0.0021)	(0.0013)	(0.0025)
$D_2(06)\times$Previous Price Difference	0.0003	0.0032*	0.0000
	(0.0017)	(0.0019)	(0.0033)
$D_2(10)\times$Previous Price Difference	0.0011	0.0044*	0.0027
	(0.0019)	(0.0024)	(0.0023)
Control	Yes	Yes	Yes
Observations	823	870	825
R-squared	0.7486	0.8521	0.8003

Notes: Other variables, which have p-values greater than 10% and are not reported above, include $D_1(06)\times\phi_{jt-1}$; $D_2(06)$ and $D_2(06)\times\phi_{jt-1}$; $D_1(10)$, $D_1(10)\times\phi_{jt-1}$ and $D_1(10)\times(\phi_{jt-1}-\bar{\phi}_{jt-1})$; $D_2(10)$ and $D_2(10)\times\phi_{jt-1}$.

Table 6: Pre v.s. post-injection long-run real-price targets

	Pre-injection Target	Post-injection Long-run Target	Wald Test p-value (Pre=Post)
$\delta = 02$			
UR	95.40	93.98	0.08*
OR	94.92	98.34	0.01***
Normal	97.00	97.86	0.45
$\delta = 06$			
UR	95.40	93.91	0.00***
OR	94.92	98.84	0.00***
Normal	97.00	99.03	0.12
$\delta = 10$			
UR	95.40	94.33	0.02**
OR	94.92	96.76	0.03**
Normal	97.00	97.85	0.43

Notes: Here the post-injection long-run statistics for δ=02, 06, and 10 are computed from coefficients associated with dummies D_2, $D_2(06)$, and $D_2(10)$, respectively.

Discussion

Here we relate Results 3 and 4 to our section-2 hypotheses and Results 1 and 2.

First, Result 3 indicates that if a buyer is matched with a seller at stage 1 in the last round, then the buyer tends to revisit the seller in the current round, conditional on that the seller does not change the real price, and Result 4 indicates that a seller has a targeted price and he gradually adjusts the price to the target. These findings support our section-2 hypothesis; that is, subjects in the frictional market may establish certain coordinating patterns so that they may appear to coordinate on a Proposition-4 equilibrium.

Next, Results 3 and 4 indicate that the pricing and visiting patterns are disturbed by an injection of tokens. In our section-2 hypothesis, we attribute the disturbance to the absence of common knowledge among subjects regarding how each other may respond to the injection. We can say a bit more here by referring the root of the disturbance to a money-illusion channel revealed by Fehr and Tyran [13]; that is, subjects know the difference between the nominal and real terms but they doubt that other subjects know. Two pieces of evidence support that subjects in our experiment know that nominal terms differ from real terms: (a) most subjects correctly calculate the real payoffs in the pee-experiment test which involves conversion of the nominal terms into the real terms; and (b) most sellers quickly post prices following injections in the frictionless market according to theory.

For how money illusion works in our experiment, consider a subject who sees the nominal change in round 11 and believes that other subjects may not be capable of distinguishing between nominal and real terms. When the subject is a seller, she may think that an increase in her round-11 nominal price would be misread as an increase in the real price by others. By her own pre-injection observations, she knows that a larger increase in the real price may lead to a longer stay in the market; consequently, she may choose to only partially adjust her nominal price in round 11 to avoid miss-matching, leading to the short-run nominal rigidity reported in Result 1.[11]

When the subject is a buyer, he may depart from the existing visiting pattern

[11] Applying the same regressions in Tables 3 and 4 to the frictionless market, we find that a higher real price leads a seller to stay longer and a buyer is more likely to visit a seller in stage 1 if they are matched in stage 1 in the last round, indicating that subjects coordinate by the same manner in the frictionless market as in the frictional market. Thus money illusion may also explain the short-run nominal rigidity in the frictionless market.

in round 11. Why? Think of that the buyer sees the full nominal adjustment made by a seller. Wondering whether his peers would interpret the adjustment as no real change or as a large real change, the buyer may feel the existing visiting pattern less useful to avoid miss-matching; this may be a reason behind the aforementioned short-run effect of injections on the buyer's stage-1 visit—the magnitude of the real-price change unconditional on the previous matching outcome becomes a factor and the size of an injection is relevant for the buyers' responses to the injection.[12]

Lastly, Result 4 indicates that the disturbance has persistent effects as it affects the real-price targets. This supports our section-2 hypothesis and is consistent with the long-run nonneutrality reported in Result 2. Recall that the price target is part of a seller's pricing rule: the seller has a reference price but he chooses an actual target around this reference which balances the tradeoff between an advantageous price and the expected miss-matching cost from his perspective. On a general level, the pricing rule is shaped by one's individual response to his own experience. The pre-injection target for each type of sellers is consistent with that sellers treat $M - c/e$ as the reference price; the cross-type differences in the pre-injection target value may be attributed to the cross-type differences in the subjective miss-matching probabilities.

The disturbance on matching in the short run seems to be the important shaping experience for both OR and UR sellers and either type has a type-specific response to this experience. A plausible rationale for the type-specific response is that different types update the subjective miss-matching probabilities for the same change in the real price differently. In the post-experiment questionnaire, we elicit sellers' reasoning on price setting after an injection. One reported reason is to set the price based on based on the historical prices of others; the sellers who report this reasoning turn out to be less likely to be UR, and more likely to be Normal.

[12] With money illusion, the injection size may play a complicate role in a subject's thinking. For example, facing the full nominal adjustment following a small-size injection, a buyer may reason that I shall treat the adjustment as a nominal change because even when peer buyers treat it as a real change, they may not be sensitive to the adjustment; but, then, the buyer may go a step further to reason that because others are not sensitive to the adjustment, I shall treat it as a real change. We cannot tell the exact reasoning process in the lab. Given that most previous research does not include the size of the nominal change as a factor in the experiment design, our finding does suggest to reconsider this practice.

5 Concluding remarks

Our experiment pertains to a game with matching friction. Our study reveals that one-shot nominal shocks have persistent real effects because they disturb the rules by which subjects coordinate to mitigate the expected miss-matching cost. Such nonneutrality presents when nominal shocks only have transient effects on the average prices, i.e., when the quantity theory holds in the long run. In his Nobel lecture, Lucas [21] argues that the quantity theory "needs to be a central feature of any monetary or macroeconomic theory that claims empirical seriousness." What we find in the lab is that following nominal shocks, different groups of sellers change their pricing rules differently and the quantity theory holds only because group-specific pricing targets (instead of individual specific random errors) average out. The different price response in the disaggregate level bears welfare implications as after the nominal shock, UR sellers are significantly worse than OR sellers in the long run. The non-experimental literature has greatly employed heterogeneous-agents models (see Moll [22] for a recent discussion), and policy makers are paying more attention to heterogeneous responses to policy and non-policy shocks (see, e.g., Yellen [25]). Our study belongs to one of the effort in this direction by investigating the role of matching friction in different price responses following a money supply shock. Future research may further explore generality, robustness, and significance of the heterogeneity in price responses.

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Appendix A. Variant treatments

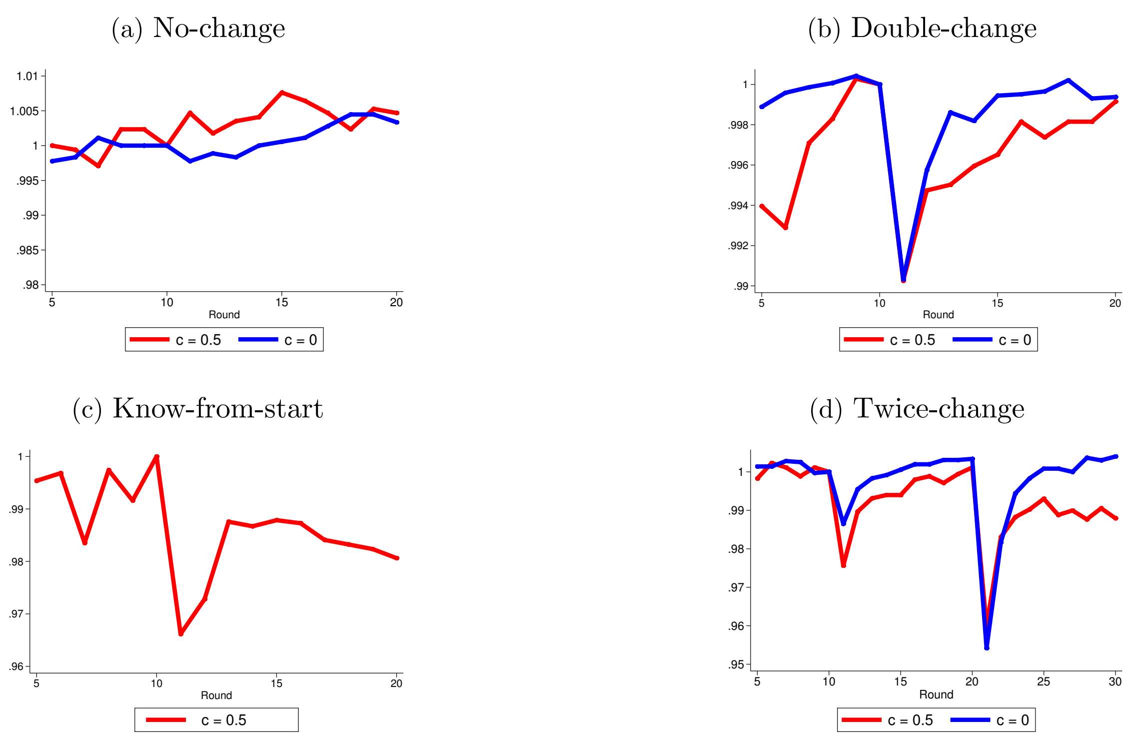
We run four types of variant treatments. A *No-change* treatment keeps the nominal stock constant. Such a treatment serves to confirm that the nominal change causes the systematic real-price movement after round 10 in the corresponding main treatment. In no-change treatments, we set $T = 20$ and $M_t = 100$ all t for both (frictional and frictionless) markets. A *Double-change* treatment sets the monetary unit twice as the unit in main treatments. Such a treatment serves to confirm that a nominal change is neutral if it occurs at the start of the experiment (as it has no effect on the established coordination); notice that a nominal change in our experiment can always be interpreted as a change in the monetary unit. In double-change treatments, we set $T = 20$, $M_t = 200$ for $1 \le t \le 10$, and $M_t = 200+\delta$ for $11 \le t \le 20$ with $\delta \in \{12, 20\}$ for both markets.

A *Know-from-start* treatment announces the nominal change at the start of the experiment. Such a treatment serves to confirm that an anticipated nominal change is nonneutral (as it affects the established coordination). In the know-from-start treatment, we set $T = 20$, $M_t = 100$ for $1 \le t \le 10$, and $M_t = 110$ for $11 \le t \le 20$. Finally, a *Twice-change* treatment adds one more round of nominal change following round 20. Such a treatment serves to confirm that even when subjects have experienced one change previously, another change is nonneutral (as it affects the established coordination). In the twice-change treatments, we set $T = 30$, $M_t = 100$ for $1 \le t \le 10$, $M_t = 110$ for $11 \le t \le 20$, and $M_t = 120$ for $21 \le t \le 30$.

In the experiment, there were 72 subjects in no-change treatments (2 sessions), 288 in double-change treatments (8 sessions), 72 in the know-from-start treatment (2 sessions), and 144 in twice-change treatments (4 sessions).

Figure A1 displays counterparts of Figure 1 for variant treatments. In the no-change treatments, there is no systematic downward movement in round 11 for either value of c. In all other treatments, the paths of $\pi_t(S_c)$ largely resemble the corresponding paths in the main treatments, where for each variant treatment, S_c denotes the set of sellers participating in that treatment with $c \in \{0.0.5\}$. Running tests for the main treatments here (all statistics of tests for variant treatments not present in Appendix A can be found in Online Appendix A), we reach the following.

Figure A1: Paths of change rates of average real prices



Result A1 In each type of market, overall prices respond to token injections sluggishly in the short run but prices fully absorb injected tokens in the long run, regardless of what the monetary unit is, whether or not injections are known in advance, and whether or not subjects have experienced injections previously. Meanwhile, neither type of market displays systematic real-price movements in the absence of token injections.

Next, let the distributions Π_t and Π be defined by the same way as in section 4. Moreover, for the twice-change treatment, let Π'_t denote the distribution of π_{jt} at round $t > 20$ for all $j \in S_{0.5}$; let

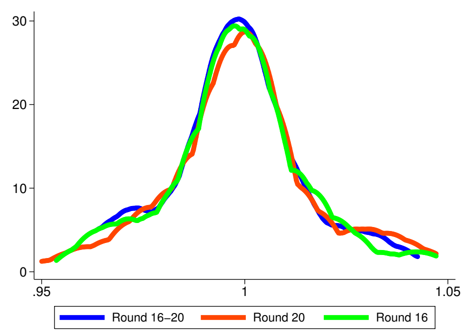
$$\bar{\pi}'_j = \sum_{t=26}^{30} \pi_{jt}/5$$

and let Π' denote the distribution of $\bar{\pi}'_j$ for all $j \in S_{0.5}$. We apply the classification of seller response types in section 4 to sellers in the double-change treatments, the know-from-start treatment, and the twice-change treatment for the frictional market. (We can also apply the classification to sellers in the no-change treatment, but the different types do not behave differently.) There are two groups of response types in the twice-change treatment, classified based on the distributions Π (see (4)) and Π'; we refer to them as the first-injection and second-injection response types, respectively.

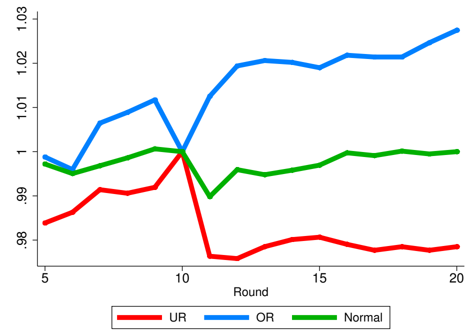
From the top row to bottom row, Figure A2 displays counterparts of Figure 4 for the double-change treatments, the know-from-start treatment, the twice-change treatment by the first-injection response types, and the twice-change treatment by the second-injection response types in order. In each of the first three rows of Figure A2, the left displays the three distributions Π_{16}, Π_{20}, and Π, and the right displays the paths of $\pi_t(S)$ for the three sets sellers (UR, OR, and Normal) classified according to their positions in Π; in the last row of Figure A2, the left displays three distributions Π'_{26}, Π'_{30}, and Π' and the right displays the paths of $\pi_t(S)$ for three sets sellers classified according to their positions in Π'.

We apply the comparisons in Tables 1 and 2 to the response types in the double-change treatments, the know-from-start treatment, and the twice-change treatment. The results are consistent with those of the main treatments. Table A1 presents the results for the know-from-start treatment.

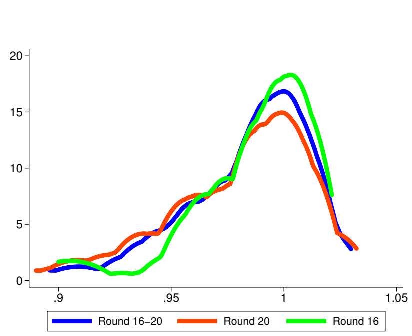
Figure A2: Classification of seller response types in variant treatments: Base and consequence



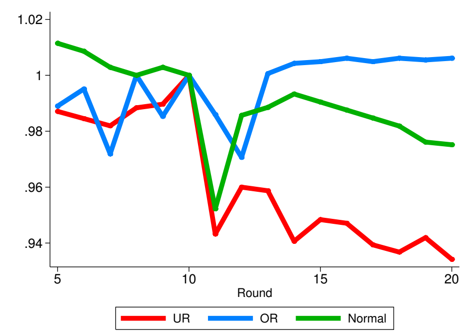
(a) Double-change



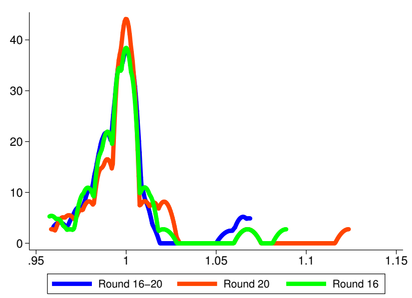
(b) Double-change



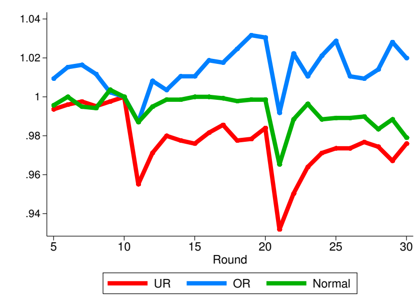
(c) Know-from-start



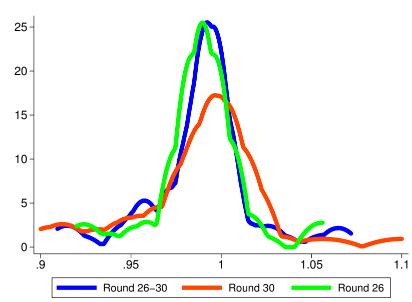
(d) Know-from-start



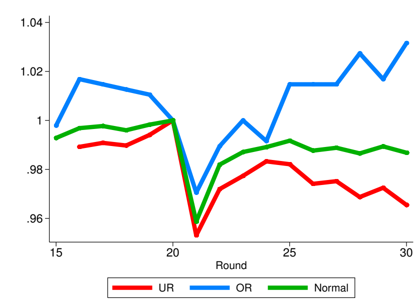
(e) Twice-change First Injection



(f) Twice-change First Injection



(g) Twice-change Second Injection



(h) Twice-change Second Injection

Table A1: Price and payoffs by response types (know-from-start treatment)

	Pre-injection	Post-injection	Post-Pre
		Avg. Real Price	
UR	95.48	91.15	-4.33***
OR	95.52	96.68	1.16***
Normal	95.41	95.74	0.33
		Avg. Payoff	
UR	7.32	7.14	-0.18***
OR	7.57	7.59	0.02
Normal	7.42	7.49	0.07
		Avg. Real Price	
UR vs. Normal	0.07	-4.59***	-4.66***
OR vs. Normal	0.11	0.94*	0.83**
UR vs. OR	-0.04	-5.53***	-5.49***
		Avg. Payoff	
UR vs. Normal	-0.10	-0.35***	-0.25***
OR vs. Normal	0.15*	0.10	-0.05
UR vs. OR	-0.25***	-0.45***	-0.20**

Result A2 In the frictional market, sellers of different types display different pricing behaviors before and after token injections, and injections are non-neutral in the long run, regardless of what the monetary unit is, whether or not injections are known in advance, and whether or not subjects have experienced injections previously.

We also apply regressions in Tables 3-5 to the double-change treatments, the know-from-start treatment, and the twice-change treatment.

In the OLS regression of the matching stage (n_{jt}), as in the main treatments, sellers posting higher real prices stay longer in the market. Also, sellers stay longer with a large real-price change in the known-from-start treatment and in late rounds of the second change of the twice-change treatment, as in the main treatments. Although the regression does not exhibit a significant effect of the real-price change on the matching stage in double-change and the first change of twice-change treatments, we find from direct correlation analysis that the correlation coefficients between the matching stage and the real-price change rate are 0.06 (p-value=0.06) and 0.09 (p-value=0.04) in double-change and the first change of twice-change treatments, respectively.

In the Probit regression of the buyer's stage-1 visiting (v_{jkt}), as in the main treatments, a buyer is more likely to visit a seller in stage 1 if they were matched in stage

1 in the last round. Also, in the double-change treatment and the second change of twice-change treatment, a higher real-price change rate leads to a lower chance of being visited in stage 1 for those who were matched in stage 1 in the last round, as in the main treatments; but the buyer's stage-1 visiting does not respond to the seller's real-price change in the know-from-start treatment and the first change of twice-change treatment.

In the OLS regression of the seller's real-price change (γ_{jt}), except for the twice-change treatment, each type's pre-injection real-price target is consistent to the reference $M - c/e$ and the Wald test shows that injections affect type-specific targets for UR and OR sellers in the long run.

In summary, the findings reported in Results 3 and 4 are largely valid in the variant treatments.

Appendix B. Statistics of tests in section 4

This appendix supplements statistics of tests indicated in section 4. Table B1 reports the outcomes of the tests for equality of means and distributions of real prices for round $t \neq 10$ vs round 10. Table B2 reports the outcomes of the tests for equality of means and distributions of payoffs for round $t \neq 10$ vs round 10. Tables B3 and B4 report the outcomes of the tests in Table B1 conditional on the injection size δ.

Tables B5, B6 and B7 report the outcomes of the tests in Tables 1 and 2 conditional on the injection size $\delta = 02$, 06, and 10, respectively.

Accompanying Table 6, Table B8 reports the outcomes of the Wald test that compares the post-injection long-run real-price targets by injection sizes.

Table B1: Test for equality of mean and distributions of real prices: round t vs round 10

	Mean		Distribution	
t	S_0	$S_{0.5}$	S_0	$S_{0.5}$
6	0.21	0.20	0.29	0.97
7	0.67	0.22	0.53	0.79
8	0.75	0.49	1.00	0.99
9	0.57	0.74	1.00	1.00
11	0.00***	0.00***	0.00***	0.00***
12	0.00***	0.00***	0.00***	0.01**
13	0.22	0.00***	0.08*	0.00***
14	0.88	0.09*	0.63	0.05***
15	0.88	0.24	0.98	0.15
16	0.54	0.29	0.89	0.29
17	0.38	0.43	0.95	0.19
18	0.57	0.78	0.89	0.29
19	0.88	1.00	0.95	0.36
20	0.28	0.94	0.72	0.36

Notes: This table reports the comparison of mean of real price in round t versus round 10 using the two-sample t-test, and the distribution of real price in round t versus round 10 using the two-sample Kolmogorov-Smirnov test.

Table B2: Test for equality of mean and distributions of payoffs: round t vs round 10

	Buyers				Sellers			
	Mean		Distribution		Mean		Distribution	
t	S_0	$S_{0.5}$	S_0	$S_{0.5}$	S_0	$S_{0.5}$	S_0	$S_{0.5}$
6	0.24	0.86	0.30	0.99	0.24	0.08*	0.30	0.43
7	0.75	0.45	0.56	0.93	0.75	0.01**	0.56	0.09*
8	0.69	0.24	1.00	0.97	0.69	0.94	1.00	0.84
9	0.54	0.59	1.00	0.89	0.54	0.26	1.00	0.76
11	0.00***	0.72	0.00***	0.03**	0.00***	0.00***	0.00***	0.00***
12	0.01***	0.70	0.00***	0.04**	0.01***	0.00***	0.00***	0.00***
13	0.40	0.15	0.09*	0.15	0.40	0.26	0.09*	0.11
14	0.89	0.90	0.64	0.29	0.89	0.33	0.64	0.36
15	0.63	0.89	0.20	0.52	0.63	0.73	0.20	0.52
16	0.76	0.38	0.85	0.29	0.76	0.47	0.85	0.04**
17	0.43	0.97	0.15	0.52	0.43	0.91	0.15	0.29
18	0.82	0.75	0.28	0.09*	0.82	0.81	0.28	0.07*
19	0.58	0.73	0.10*	0.27	0.58	0.75	0.10	0.12
20	0.34	0.68	0.46	0.09*	0.34	0.73	0.46	0.05**

Notes: This table reports the comparison of mean of payoffs in round t versus round 10 using the two-sample t-test, and the distribution of payoffs in round t versus round 10 using the two-sample Kolmogorov-Smirnov test.

Table B3: Comparing average price conditional on injection size: round t vs round 10

t	(02, 0)	(02, 0.5)	(06, 0)	(06, 0.5)	(10, 0)	(10, 0.5)
6	0.06*	0.51	0.04**	0.13	0.80	0.80
7	0.05**	0.47	0.46	0.33	0.44	0.61
8	0.13	0.89	0.66	0.26	0.39	0.67
9	0.30	0.87	0.23	0.80	0.50	0.87
11	0.06*	0.69	0.00***	0.00***	0.00***	0.00***
12	0.36	0.70	0.00***	0.06*	0.08*	0.00***
13	0.69	0.70	0.01***	0.09*	0.62	0.00***
14	0.25	0.84	0.02**	0.19	0.70	0.16
15	0.09*	0.64	0.04**	0.23	0.61	0.20
16	0.25	0.87	0.01***	0.48	0.90	0.20
17	0.07*	0.50	0.17	0.23	0.83	0.40
18	0.24	0.75	0.03**	0.52	0.94	0.87
19	0.13	0.97	0.01**	0.72	0.19	0.77
20	0.24	0.84	0.09*	0.88	0.22	0.85

Notes: This table reports the comparison of mean of real price in round t versus round 10 using the two-sample t-test.

Table B4: Comparing price distributions conditional on injection size e: round t vs round 10

t	(02, 0)	(02, 0.5)	(06, 0)	(06, 0.5)	(10, 0)	(10, 0.5)
6	0.69	1.00	0.59	0.76	1.00	1.00
7	0.85	1.00	0.98	1.00	1.00	1.00
8	0.69	0.89	1.00	1.00	1.00	1.00
9	0.85	1.00	0.59	1.00	1.00	0.99
11	0.00***	1.00	0.00***	0.01***	0.00***	0.01***
12	0.85	1.00	0.03**	0.44	0.01***	0.01***
13	1.00	1.00	0.09*	0.21	0.16	0.01***
14	1.00	1.00	0.09*	0.14	1.00	0.18
15	0.85	0.89	0.21	0.21	1.00	0.38
16	1.00	1.00	0.21	0.31	1.00	0.51
17	0.69	1.00	0.13	0.14	1.00	0.51
18	1.00	0.89	0.31	0.59	1.00	0.66
19	0.96	1.00	0.09*	0.31	0.96	0.66
20	1.00	1.00	0.05*	0.89	1.00	0.81

Notes: This table reports the comparison of distribution of real price in round t versus round 10 using the two-sample Kolmogorov-Smirnov test.

Table B5: Comparison of price and payoffs by response types, $\delta = 02$

	Pre-injection Avg. Real Price	Post-injection Avg. Real Price	Difference
UR	94.46	93.86	0.60
OR	95.77	96.74	-0.97***
Normal	96.14	96.49	-0.35
	Avg. Payoff	Avg. Payoff	Difference
UR	7.31	7.30	0.01
OR	7.37	7.51	-0.14**
Normal	7.56	7.56	0.002
Cross-type Differences	Pre-injection Avg. Real Price	Post-injection Avg. Real Price	Difference
UR vs. Normal	-1.67***	-2.63***	0.95**
OR vs. Normal	-0.37	0.25	-0.62**
UR vs. OR	-1.31***	-2.88***	1.57***
	Avg. Payoff	Avg. Payoff	Difference
UR vs. Normal	-0.25***	-0.25***	0.01
OR vs. Normal	-0.19***	-0.05	-0.14**
UR vs. OR	-0.06	-0.20***	0.15***

Table B6: Comparison of price and payoffs by response types, $\delta = 06$

	Pre-injection Avg. Real Price	Post-injection Avg. Real Price	Difference
UR	94.84	93.82	1.02
OR	95.85	97.90	2.06***
Normal	98.00	97.90	0.10
	Avg. Payoff	Avg. Payoff	Difference
UR	7.33	7.38	-0.06
OR	7.44	7.55	-0.11**
Normal	7.54	7.75	-0.21
Cross-type Differences	Pre-injection Avg. Real Price	Post-injection Avg. Real Price	Difference
UR vs. Normal	-3.16**	-4.07***	0.92
OR vs. Normal	-2.15***	0.005	-2.16***
UR vs. OR	-1.00	-4.08***	3.07***
	Avg. Payoff	Avg. Payoff	Difference
UR vs. Normal	-0.22	-0.37***	0.15**
OR vs. Normal	-0.10	-0.20***	0.10**
UR vs. OR	-0.12	-0.17***	0.05

Table B7: Comparison of price and payoffs by response types, $\delta = 10$

	Pre-injection Avg. Real Price	Post-injection Avg. Real Price	Difference
UR	95.75	93.73	2.02***
OR	93.44	95.74	-2.30***
Normal	96.54	96.19	0.35
	Avg. Payoff	Avg. Payoff	Difference
UR	7.43	7.32	0.12
OR	7.29	7.43	-0.15
Normal	7.49	7.52	0.46
Cross-type Differences	Pre-injection Avg. Real Price	Post-injection Avg. Real Price	Difference
UR vs. Normal	-0.79**	-2.46***	1.67***
OR vs. Normal	-3.09***	-0.45	-2.65***
UR vs. OR	2.30***	-2.02***	4.32***
	Avg. Payoff	Avg. Payoff	Difference
UR vs. Normal	-0.05	-0.20**	0.15
OR vs. Normal	-0.20***	-0.09	-0.12
UR vs. OR	0.15***	-0.12	0.26***

Table B8: Comparison of post-injection real-price targets by injection sizes

UR	Long-run Target	Long-run Target	Wald Test p-value (Equal Targets)
02 vs. 06	93.98	93.91	0.00***
02 vs. 10	93.98	94.33	0.00***
06 vs. 10	93.91	94.33	0.52
OR			
02 vs. 06	98.34	98.84	0.91
02 vs. 10	98.34	96.76	0.87
06 vs. 10	98.84	96.76	0.04**
Normal			
02 vs. 06	97.86	99.03	0.00***
02 vs. 10	97.86	97.85	0.00***
06 vs. 10	99.03	97.85	0.48

Online Appendix A

This appendix supplements statistics of tests indicated in Appendix A. Tables OA1, OA2, and OA3 report the outcomes of the tests in Table 1 and 2 for the double-change treatments, the twice-change treatment classified based on the first injection, and the twice-change treatment classified based on the second injection, respectively.

For the double-change treatments, the known-from-start treatment, and the twice-change treatment, Tables OA4-OA6 report the outcomes of the regression in Table 3, Tables OA7-OA10 report the outcomes of the regression in Table 4, and Tables OA11-OA13 report the outcomes of the regression in Table 5; and Table OA14 reports the outcomes of the Wald test in Table 6.

Below the dummies $D_1(12)$, $D_1(20)$, $D_2(12)$, and $D_2(20)$ in the double-change treatments correspond to $D_1(06)$, $D_1(10)$, $D_2(06)$, and $D_2(10)$ in the main treatments. And, in the twice-change treatment, the dummy D_3 is equal to 1 if $21 \leq t \leq 25$ and zero otherwise; the dummy D_4 is equal to 1 if $26 \leq t \leq 30$ and zero otherwise.

Table OA1: Comparison of price and payoffs by response types, double-change

	Pre-injection Avg. Real Price	Post-injection Avg. Real Price	Difference
Price			
UR	194.71	192.25	2.46***
OR	193.41	195.98	-2.58***
Normal	196.46	196.63	-0.17
	Avg. Payoffs	Avg. Payoffs	Difference
UR	7.55	7.50	0.04
OR	7.54	7.65	-0.10***
Normal	7.61	7.71	-0.10
Cross-type Differences	Pre-injection Avg. Real Price	Post-injection Avg. Real Prices	Difference
UR vs. Normal	-1.75***	-4.38***	2.63***
OR vs. Normal	-3.05***	-0.64*	-2.41***
UR vs. OR	1.3**	-3.73***	5.03***
	Avg. Payoffs	Avg. Payoffs	Difference
UR vs. Normal	-0.06	-0.20***	0.14***
OR vs. Normal	-0.07	-0.06	-0.005
UR vs. OR	0.01	-0.14***	0.15***

Table OA2: Comparison of price and payoffs by response types, twice-change (first injection)

	Pre-first-injection Avg. Real Price	Post-first-injection Avg. Real Price	Difference
UR	95.57	93.98	1.58***
OR	95.64	97.11	-1.47**
Normal	98.59	98.56	0.03
	Avg. Payoffs	Avg. Payoffs	Difference
UR	7.49	7.44	0.05
OR	7.52	7.53	-0.01
Normal	7.47	7.69	-0.22*
Cross-type Differences	Pre-first-injection Avg. Real Price	Post-first-injection Avg. Real Price	Difference
UR vs. Normal	-3.02***	-4.58***	1.53***
OR vs. Normal	-2.94***	-1.44***	-1.50**
UR vs. OR	-0.08	-3.13***	3.05***
	Avg. Payoffs	Avg. Payoffs	Difference
UR vs. Normal	0.01	-0.25***	0.27***
OR vs. Normal	0.04	-0.16**	0.21***
UR vs. OR	-0.03	-0.09*	0.06

Notes: Here pre-first-injection refers to rounds 6 to 10, while Post-first-injection refers to rounds 16 to 20.

Table OA3: Comparison of price and payoffs by response types, twice-change (second injection)

	Pre-second-injection Avg. Real Price	Post-second-injection Avg Real Price	Difference
UR	96.17	93.53	2.63***
OR	97.38	97.70	-0.32
Normal	96.47	95.90	0.57
Payoffs	Avg. Payoffs	Avg. Payoffs	Difference
UR	7.45	7.38	0.07
OR	7.66	7.60	0.06
Normal	7.59	7.60	-0.01
Cross-type Differences	Pre-second-injection Avg. Real Price	Post-second-injection Avg. Real Price	Difference
UR vs. Normal	-0.30	-2.37***	2.06***
OR vs. Normal	0.92	1.80***	-0.89
UR vs. OR	-1.22**	-4.17***	2.95***
	Avg. Payoffs	Avg. Payoffs	Difference
UR vs. Normal	-0.14**	-0.22***	0.08
OR vs. Normal	0.08	0.005	0.07
UR vs. OR	-0.22***	-0.22***	0.01

Notes: Here pre-second-injection refers to rounds 16 to 20, while post-second-injection refers to rounds 26 to 30.

Table OA4: Outcome of regression of seller's matching stage, double-change

	Dependent variable: Seller's matching stage n_{jt}
Real Price	0.0439***
	(0.0101)
$D_1(20)\times$Real Price Change	-9.0851*
	(5.1197)
	(124.8315)
Constant	-7.1492***
	(1.9608)
Control	Yes
Observations	1,079
R-squared	0.0651

Notes: In this and the next 5 tables, variables with p values greater than 10% are not reported.

Table OA5: Outcome of regression of seller's matching stage, know-from-start

	Dependent variable: Seller's matching stage n_{jt}
D_1	6.6509**
	(2.6104)
Real Price	0.0982***
	(0.0247)
$D_1\times$Real Price	-0.0682**
	(0.0273)
D_2	5.7256*
	(3.0449)
$D_2\times$Real Price	-0.0603*
	(0.0319)
[Real Price Change]^2	5.7837**
	(2.6926)
$D_1\times$[Real Price Change]^2	-9.1644**
	(3.7367)
Constant	-7.9823***
	(2.3629)
Observations	540
R-squared	0.0685

Table OA6: Outcome of regression of seller's matching stage, twice-change

	Dependent variable: Seller's matching stage n_{jt}
Real Price	0.0597***
	(0.0176)
$D_4\times$[Real Price Change]^2	43.1290**
	(21.8300)
Constant	-4.3579**
	(1.7068)
Observations	900
R-squared	0.0878

Table OA7: Outcome of regression of buyer's stage-1 visit, double-change

	Dependent variable: Buyer's stage-1 visit v_{jkt}
Previous Stage-1 Matching	1.1113***
	(0.0624)
$D_1\times$Previous Stage-1 Matching	0.3078**
	(0.1406)
$D_2\times$Previous Stage-1 Matching	0.3078***
	(0.1527)
Previous Stage-1 Matching $\times$ [Real Price Change]^2	-0.0020**
	(0.0010)
$D_1(20)\times$Previous Stage-1 Matching	-0.4172**
	(0.1831)
$D_2(20)\times$Previous Stage-1 Matching	-0.5063**
	(0.1995)
Constant	-0.9625**
	(0.4739)
Control	Yes
Pseudo R-squared	0.0948
Observations	8,634

Table OA8: Outcome of regression of buyer's stage-1 visit, know-from-start

	Dependent variable: Buyer's stage-1 visit v_{jkt}
Previous Stage-1 Matching	1.2482***
	(0.0873)
Control	Yes
Pseudo R-squared	0.090
Observations	4,320

Table OA9: Outcome of regression of buyer's stage-1 visit in the first injection periods, twice-change

	Dependent variable: Buyer's stage-1 visit v_{jkt}
Previous Stage-1 Matching	0.9668***
	(0.0891)
$D_1 \times$Previous Stage-1 Matching	0.2570*
	(0.1549)
$D_2 \times$Previous Stage-1 Matching	0.6076***
	(0.1522)
Control	Yes
Pseudo R-squared	0.1006
Observations	4,302

Notes: First injection periods refer to round 6 to round 20.

Table OA10: Outcome of regression of buyer's stage-1 visit in the second injection periods, twice-change

	Dependent variable: Buyer's stage-1 visit v_{jkt}
Previous Stage-1 Matching	1.8140***
	(0.2905)
D_3	0.3813**
	(0.1880)
Previous Stage-1 Matching $\times$ [Real Price Change]^2	-95.4603***
	(27.4319)
$D_3 \times$Real Price Change	-11.5293**
	(5.7336)
$D_4 \times$Real Price Change	-14.3120**
	(6.0252)
$D_4 \times$[Real Price Change]^2	-163.9042***
	(60.5111)
Control	Yes
Pseudo R-squared	0.1795
Observations	2,370

Notes: Second injection periods refer to round 20 to round 30.

Table OA11: Outcome of regression of seller's real-price change, double-change

	Dependent variable: Seller's real-price change rate γ_{jt}		
	(1) UR	(2) OR	(3) Normal
Constant	0.6693***	0.4849***	0.7143***
	(0.0855)	(0.0726)	(0.0735)
Previous Real Price	-0.0034***	-0.0025***	-0.0036***
	(0.0004)	(0.0004)	(0.0004)
Previous Price Difference	-0.0001	-0.0005**	0.0000
	(0.0003)	(0.0002)	(0.0002)
D_1	-0.2257	0.0084	-0.6528***
	(0.2369)	(0.1517)	(0.2000)
D_2	0.0464	0.0759	-0.4797***
	(0.2041)	(0.1406)	(0.1776)
$D_1\times$Previous Real Price	0.0011	0.0000	0.0033***
	(0.0012)	(0.0008)	(0.0010)
$D_2\times$Previous Real Price	-0.0003	-0.0003	0.0024***
	(0.0011)	(0.0007)	(0.0009)
$D_1\times$Previous Price Difference	-0.0014	-0.0002	-0.0008
	(0.0010)	(0.0007)	(0.0010)
$D_2\times$Previous Price Difference	-0.0003	-0.0004	-0.0001
	(0.0009)	(0.0006)	(0.0010)
$D_1(20)$	-0.1824	0.4664**	0.7926***
	(0.2423)	(0.1976)	(0.1978)
$D_2(20)$	-0.3293	0.1728	0.8234***
	(0.2099)	(0.2806)	(0.1730)
$D_1(20)\times$Previous Real Price	0.0009	-0.0024**	-0.0041***
	(0.0013)	(0.0010)	(0.0010)
$D_2(20)\times$Previous Real Price	0.0017	-0.0009	-0.0042***
	(0.0011)	(0.0014)	(0.0009)
$D_1(20)\times$Previous Price Difference	0.0019*	0.0015*	0.0008
	(0.0010)	(0.0009)	(0.0010)
$D_2(20)\times$Previous Price Difference	-0.0003	-0.0000	0.0002
	(0.0009)	(0.0013)	(0.0010)
Observations	360	359	360
R-squared	0.4102	0.5435	0.7243

Table OA12: Outcome of regression of seller's real-price change, know-from-start

	Dependent variable: Change rate of seller's real price		
	(1) UR	(2) OR	(3) Normal
Constant	0.6536***	0.9268***	1.1071***
	(0.1783)	(0.1764)	(0.3641)
Previous Real Price	-0.0068***	-0.0097***	-0.0116***
	(0.0019)	(0.0018)	(0.0038)
Previous Price Difference	-0.0010	0.0003	-0.0009
	(0.0015)	(0.0014)	(0.0035)
D_1	-0.0910	-0.2753	-1.2381**
	(0.2369)	(0.2767)	(0.5125)
D_2	-0.0999	-0.2888	-0.9840*
	(0.2607)	(0.2447)	(0.5408)
$D_1\times$Previous Real Price	0.0007	0.0029	0.0131**
	(0.0025)	(0.0029)	(0.0054)
$D_2\times$Previous Real Price	0.0007	0.0032	0.0104*
	(0.0028)	(0.0026)	(0.0057)
$D_1\times$Previous Price Difference	-0.0016	0.0007	-0.0072
	(0.0021)	(0.0025)	(0.0050)
$D_2\times$Previous Price Difference	0.0013	-0.0045**	-0.0023
	(0.0024)	(0.0021)	(0.0050)
Observations	180	150	210
R-squared	0.5710	0.6496	0.3214

Table OA13: Outcome of regression of seller's real-price change, twice-change

	Dependent variable: Seller's real-price change γ_{jt}		
	(1) UR	(2) OR	(3) Normal
Constant	0.9328***	0.2311**	0.0597
	(0.1819)	(0.1128)	(0.9129)
Previous Real Price	-0.0096***	-0.0023**	-0.0006
	(0.0019)	(0.0012)	(0.0094)
Previous Price Difference	0.0009	-0.0031***	-0.0062
	(0.0018)	(0.0010)	(0.0106)
D_1	-0.4208	-0.3441**	0.0032
	(0.2628)	(0.1708)	(1.3769)
D_2	-0.0603	0.1055	-0.0183
	(0.2478)	(0.1696)	(1.2424)
D_3	-0.2917	0.1040	2.2895**
	(0.2600)	(0.1612)	(1.1227)
D_4	-0.1419	0.1004	0.4248
	(0.2468)	(0.1641)	(1.1969)
D_1×Previous Real Price	0.0042	0.0035*	-0.0001
	(0.0028)	(0.0018)	(0.0142)
D_2×Previous Real Price	0.0006	-0.0010	0.0002
	(0.0026)	(0.0018)	(0.0129)
D_3×Previous Real Price	0.0027	-0.0011	-0.0243**
	(0.0027)	(0.0017)	(0.0116)
D_4×Previous Real Price	0.0012	-0.0009	-0.0044
	(0.0026)	(0.0017)	(0.0124)
D_1×Previous Price Difference	-0.0001	-0.0059***	-0.0023
	(0.0021)	(0.0017)	(0.0157)
D_2×Previous Price Difference	0.0007	-0.0008	0.0013
	(0.0021)	(0.0018)	(0.0138)
D_3×Previous Price Difference	0.0003	-0.0015	0.0048
	(0.0023)	(0.0016)	(0.0125)
D_4×Previous Price Difference	-0.0014	-0.0017	0.0048
	(0.0023)	(0.0017)	(0.0127)
Observations	300	300	300
R-squared	0.4994	0.5059	0.4926

Table OA14: Pre v.s. post-injection long-run real-price targets

	Pre-Target	Post-long-run Target	p-value (Pre=Post)
Know-from-start			
UR	96.02	89.95	0.00***
OR	95.59	97.63	0.01***
Normal	95.29	99.50	0.77
Double-change			
$\delta = 04$			
UR	195.73	192.14	0.00***
OR	192.92	196.81	0.00***
Normal	196.83	197.30	0.86
$\delta = 20$			
UR	195.73	193.25	0.06*
OR	192.92	198.23	0.00***
Normal	196.83	197.45	0.23
Twice-change			
First Injection			
UR	97.27	96.60	0.41
OR	98.54	100.43	0.53
Normal	104.23	110.03	0.99
Second Injection			
UR	97.27	93.91	0.00***
OR	98.54	101.16	0.39
Normal	104.23	96.81	0.95

Online Appendix B

The original experimental instruction is in Chinese. We provide the English version here for reference. To save the space, we give the instruction used in main treatments with $c = 0.5$.

Welcome to our experimental study on decision-making. You will receive a show-up fee of 40 RMB. In addition, you can gain more money as a result of your decisions in the experiment.

Your identity

You will be given a subject ID number. Please keep it confidential. Your decisions will be anonymous and kept confidential. Thus, other participants won't be able to link your decisions with your identity. You will be paid in private, using your subject ID, and in cash at the end of the experiment. When you have any questions, please feel free to ask by raising your hand, one of our assistants will come to answer your questions. Please DO NOT communicate with any other participants.

Before the start of the actual experiment, you will have 5 practice rounds. Your decisions in the practice rounds will not affect your payoff in the experiment. In the actual experiment, there are 20 rounds. In the beginning of the experiment, participants will be randomly matched into groups of 12. Each group will have 6 buyers and 6 sellers. In the beginning of the experiment, the computer will randomly determine your role and you will be informed. If you are buyer, you will be given a buyer number. If you are seller, you will be given a seller number. Your role in the experiment will remain unchanged. The group members in the experiment will also remain unchanged.

The roles of buyer and seller are described below.

Buyer

In each round, each buyer is endowed with 100 tokens (exchange rate: 100 tokens=8 RMB) which he/she can use to purchase a good from seller. The valuation of the good to the buyer is 16 RMB. If the buyer buys the good, his/her payoff will be equal to 16 RMB – price – search cost. If the buyer cannot buy the good, his/her payoff in that round will be zero, and the 100 tokens endowment will be canceled. We will explain the buyer's search cost below. Note that all transactions will be made in terms of tokens. In the end of the experiment, your payoff will be converted into RMB using the announced exchange rate.

Seller

In each round, the seller is endowed with one unit of a good, which will be perished if not being sold in the current round (i.e., the value becomes zero). If the seller sells the good, the payoff of the seller in that round equals to the price of the good-seller's search cost. We will explain the seller's search cost in below.

Procedures in the first round

We now describe how transactions will be conducted. First, sellers in each group will set the price. Then, the prices will be announced to sellers in the group. Each round has 6 trading stages in which buyers and sellers can transact. In each stage, an active buyer can visit an active seller to buy the good. An active buyer is a buyer who has not purchased the good in the current round. An active seller is a seller whose good has not been sold in the current round. An active stage is one in which there are still active buyer(s) and active seller(s). An inactive stage is one in which there no longer any active buyer and active seller.

In any active stage, an active buyer can choose to buy from any active seller. If a seller is chosen by only one buyer, the transaction will be conducted using the posted price by the seller. If a seller is chosen by two or more buyers, the computer will randomly determine which buyer can buy the good. The randomly selected buyer will buy the good using the posted price.

An active buyer can also choose not to buy from any active seller. If all active buyers in a stage choose not to buy from any active seller, the stage will end automatically. Active buyers and sellers can trade in a new stage, but they will need to pay a search cost of 0.5 RMB for trading in each new stage (except the first stage).

Results

In the end of each round, the transacted prices will be announced to group members. The computer will also inform each subject their payoffs in the round. Note that if a subject did not succeed in buying/selling the good in 6 stages, he/she still needs to pay for the search cost.

Example

Seller 1 sets the price at 96 tokens, Seller 2 sets the price at 98 tokens, seller 3 sets the price at 92tokens, seller 4sets the price at 85tokens, seller 5 sets the price at 96 tokens, seller 6 sets the price at 91tokens.

In the first stage, buyer 1 and buyer 2 do not buy from any active sellers. Buyer 3 buys from seller 4. Buyer 4 buys from seller 3. Buyer 5 buys from seller 6. Buyer 6 buys from seller 5. In the second stage, buyer 1 and buyer 2 both choose to buy from seller 2. Buyer 1 was randomly chosen by the computer to buy from seller 2. In stage 3, buyer 2 buys from seller 1. In this example, all buyers and sellers complete their transactions in stage 3. As a result, the round ends at stage 3.

Using the above example, calculate the payoff of the following subjects:

Buyer 1's payoff=16RMB – 98 tokens -0.5RMB	Seller 2's payoff=98 tokens - 0.5RMB
Buyer 2's payoff =16RMB – 96 tokens – 1RMB	Seller 1's payoff =96 tokens – 1RMB
Buyer 3's payoff =16RMB – 85 tokens	Seller 4's payoff =85 tokens
Buyer 4's payoff =16RMB – 92 tokens	Seller 3's payoff =92 tokens
Buyer 5's payoff =16RMB – 91 tokens	Seller 6's payoff =91 tokens
Buyer 6's payoff =16RMB – 96 tokens	Seller 5's payoff =96 tokens

Note: The payoff will be paid using RMB. For example, buyer 5 will receive 16 – 91 x exchange rate = RMB 8.72.

Procedures in the first round

In the beginning of each round, when there are changes the computer will make an announcement. Except this, the procedure in round 2 to 20 is the same as round 1.

Payoff

In the end of the experiment, the computer will randomly draw 7 rounds for payment. That is, each participant will receive the payoff which is equal to the sum of payoff from these 7 rounds plus the show-up fee.f from these 7 rounds plus the show-up fee.