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ABSTRACT

In labor markets, the ratchet effect refers to a situation where workers subject to performance pay choose to restrict their output, because they rationally anticipate that firms will respond to higher output levels by raising output requirements or cutting pay. We model this effect as a multi-period principal-agent problem with hidden information, and study its robustness to labor market competition both theoretically and experimentally. Consistent with our theoretical model, we observe substantial ratchet effects in the absence of competition, which is nearly eliminated when competition is introduced; this is true regardless of whether market conditions favor firms or workers.

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“Most of the work in one of the largest tire-building plants in the country is on a piece-rate basis. In one department, the piece workers pushed their earnings up to \$12 a day. Said an employee in this department, ‘The rate was immediately cut. Now we know that the maximum paid for this work is \$7 a day. It would be possible for us to do much more but we are careful not to.’ ”

“And a girl who was experienced in a wide range of employment explained: ‘I have learned through sad experience that the more your superiors find they can get out of you the more they come to expect. The only way to protect yourself is never to work at anything like full capacity. I know that most restriction is due to the worker’s desire to save and protect herself and not to any other motive.’ ”

Quotes are from Mathewson (1931), chapter 3.

1. Introduction

While piece rates and other forms of performance-based pay have been used successfully in many contexts (e.g. Lazear 2000), one potential problem affecting such schemes, labelled the ‘ratchet effect’, has been recognized since at least the 1930s. At that time, industrial sociologists like Stanley Mathewson found that piece-rate workers were often careful to limit their output. Based on bitter experience, such workers were confident that choosing a higher output might yield short-term gains but would soon result in either a cut to their rate or an increase in production quotas that would leave them worse off in the long run. Thus, in contrast to the predictions of a static model (such as Lazear’s), piece rates might actually *reduce* worker performance when workers consider the dynamic implications of current effort choices.

To the best of our knowledge, formal models of ratchet effects did not emerge until shortly after the development of asymmetric-information models of principal-agent interactions in the early 1980s. At that time, it was recognized that these effects can be modelled as a situation where a principal contracts with an agent more than once, the agent takes a non-contractible action and has private information, and binding multi-period contracts are not enforceable (Freixas, Guesnerie and Tirole, 1985). In such situations, actions taken by the agent

early in the relationship can reveal information to the principal, who then uses the information to the agent's disadvantage. In addition to the economics of worker incentives within firms (Gibbons 1987, Ickes and Samuelson 1987, Carmichael and MacLeod 2000), other interactions that have been modelled using a ratchet effect framework include the setting of production targets for branches of a multidivisional firm or a nationalized economy (Weitzman 1980), contracting between a social-welfare-maximizing regulator and firms under his or her jurisdiction (Laffont and Tirole 1988, Litwack 1993, Dalen 1995), managers' incentives to innovate (Dearden, Ickes and Samuelson 1990), procurement contracting (Laffont and Tirole 1993), optimal income taxation (Dillen and Lundholm 1996), the economics of corruption (Choi and Thum 2003), and environmental regulation (Puller 2006).

A common feature of the above models is the presence of pooling equilibria early in the principal-agent relationship. For example, in the case of worker compensation, abler workers tend to mimic workers with less talent (or to act 'as if' the firm's technology is less productive than it truly is) by restricting their output early in the employment relationship. This benefits abler workers (or workers lucky enough to discover that the production technology is 'good') by preventing the firm from extracting their rents later in the relationship, but it is socially inefficient. Another, less well-known, feature of existing ratchet models is their tendency to model agents' participation constraints very simply, typically by requiring the utility of all agent types to exceed the same threshold in each period. While this may make sense in some cases, we see it as applicable to labor markets only in cases where market frictions are high: essentially, workers' only alternative to their current match is modelled as an activity (such as unemployment) that is equally valuable to all workers, no matter how able they are. The option

of putting one's talents to work at another firm with the same technology as the current one is typically not modelled.

The robustness of the ratchet effect to the competitive environment thus strikes us as an important but understudied question, both in labor economics and in the economics of incentives and information more generally. For example, while ratchet effects may be common in labor markets characterized by substantial mobility costs or firm-specific capital, the importance of ratchet effects in markets characterized by a high degree of competition and (at least potential) mobility is less clear. One model of ratchet effects that expands the treatment of agents' labor market alternatives is Kanemoto and MacLeod (1992).¹ Kanemoto and MacLeod show that, even when the labor market does not observe the agent's early-career performance, the presence of *ex post* market opportunities for agents can eliminate the ratchet effect, allowing first-best effort levels to be attained.² This suggests that *ex-post* markets for agents may be a more powerful force in eliminating ratchet effects than previously realized.

While numerous case studies, including the famous *Lincoln Electric* case discussed in Carmichael and MacLeod (2000), strongly suggest that ratchet effects can be important in real workplaces, such effects are difficult to study econometrically, or using field experiments. Theoretically, ratchet effects are predicted to occur in specific informational and contractual environments: both hidden action and hidden information must be present, and the parties must

¹ Another such model is Roland and Sekkat (2000), who consider a situation where agents' first-stage performance is observed by an outside labor market. Arguably, this puts Roland-Sekkat's model into a different class of models from the ratchet-effect literature, often referred to as "career-concerns" models (see for example Dewatripont, Jewitt and Tirole 1999). In addition to making agents' output public information, career-concerns models typically do not incorporate private information about an agent's type. In contrast to ratchet-effect models, career-concerns models are capable of generating socially *excessive* effort early in the agent's career.

² Intuitively, this is because abler agents are able to generate more total surplus from the employment relationship than less-able agents. Even when the market does not observe first-stage performance, this generates type-specific participation constraints in the second stage, which protect abler agents from exploitation. Note that this reasoning does not apply to the case modeled by Gibbons (1987), where the asymmetric information applies to the firm's technology rather than the agent's type.

be in a repeated relationship yielding some quasi-rents to both parties where binding multi-period agreements are *not* feasible. Measuring, and either manipulating or isolating plausibly exogenous natural variation in these features of the environment presents significant challenges. Taking the additional step of manipulating the amount of labor market *competition* affecting workers and firms, which is the focus of this paper, adds to these challenges. On the other hand, all of these economic, informational and contractual features can be precisely measured and exogenously manipulated in the laboratory.

Given the above discussion it is not surprising that, aside from the case studies discussed above, the only empirical evidence of ratchet effects we know of is experimental in nature.³ Chaudhuri (1998) conducted a laboratory experiment in which principals and agents interacted for two periods, and agents were one of two types that were unobserved by the principal. There was little evidence of ratcheting: most agents played naively, revealing their type in the first period even when an informed principal would use this information to the agent's disadvantage, and principals often did not exploit agents' type revelation. Possible explanations for this result include the relative complexity of the game, and the lack of context provided to the subjects that might have impeded the learning process.

The only other experimental study of the ratchet effect of which we are aware is Cooper, Kagel, Lo and Gu (1999). Cooper *et alii* frame their experiment in a context-rich way, as a game between central planners and firm managers, use both students and actual Chinese firm managers as subjects, and implement experimental payoffs with high stakes relative to the participants' real-world incomes. They also simplify the interactions between principals and agents, focusing

³ An exception is Allen and Lueck (1999), who use cash rent and crop-share contract data to test for the presence of the ratchet effect in the context of moral hazard in agricultural contracts, but do not consider the role of competition. They find limited evidence for the ratchet effect within share contracts and conclude that it cannot explain the choice of contracts.

the experiment only on the stages of the game where information revelation matters: the agent's effort choice in the first period, and the principal's choice of a payoff schedule in the second.

Cooper *et alii* do find evidence of ratchet effects, though even in their context it took some time for the players to learn the consequences of type revelation. Their artefactual field experiment with managers supports the external validity of ratchet effects found in the laboratory.

This paper reports the results of some simple laboratory experiments on the ratchet effect, in the context of piece-rate compensation. It differs from existing experimental studies in two main ways. First, motivated by our discussion of competition above and by Kanemoto and MacLeod's theoretical insight, our study is the first to introduce market competition for agents and principals, by allowing players to choose between their current partner and an alternative partner at the start of each stage of the game.⁴ Second, motivated by Cooper *et alii*'s experimental approach, we simplify the interactions between principals and agents even further. Specifically, we focus only on the strategic interactions at the heart of the ratchet effect, i.e. those between the *high-talent* agents' first-stage efforts and the principals' second-stage rewards, and we reduce both parties' strategy sets to two choices (high or low output, and high or low pay). Combining this with a simple context that makes sense to our subjects (principals are firms and agents are workers), we believe that this significantly improves our subjects' understanding of the game.

Our main findings are twofold. First, we observe substantial and significant ratchet effects in the baseline case of our model (which, like Chaudhuri and Cooper *et alii*, does not

⁴ We are aware of only two other principal-agent experiments that include treatments featuring both excess firms and excess workers. Brandts and Charness (2004) find only minor effects on the ratio of 'effort' to 'wage', according to the direction of market imbalance in a gift-exchange experiment, and also find deleterious effects on effort when a minimum wage is introduced. Cabrales, Charness, and Villeval (2006) show that in a static game with informational asymmetry and hidden information, inefficiencies remain when principals compete against each other to hire agents. In contrast, when agents compete to be hired, efficiency improves dramatically, and it increases in the relative number of agents because competition reduces the agents' informational monopoly power.

incorporate labor market competition). We believe that this is in large part due to our simple design that focuses on the main strategic issues. Second, we find that ratchet behavior is significantly reduced by competition; interestingly this is true regardless of whether market conditions favor workers (our ‘excess firms’ case), or firms (our ‘excess workers’ case). In the excess-firms case, it appears (as Kanemoto and MacLeod’s analysis predicts, and consistent with the Perfect Bayesian Nash equilibrium --henceforth PBNE-- of the game played by our subjects) that talented workers are willing to reveal their types in the first stage, because market alternatives allow them to escape the incumbent firm’s desire to exploit them *ex post*. In the excess-workers case (again consistent with the two pure-strategy PBNE of the game), most talented workers once again are willing to reveal their type in the first stage. Now this is because they are no longer assured of remaining employed with the same firm in the second stage; this matching uncertainty reduces the returns to concealment.

Taken together, while the precise game played by our subjects is hardly general, we conjecture that our results illustrate two simple, general principles regarding the effects of competition on ratchet effects, both in labor markets and more generally. First, an improvement in *either* party’s outside options (which, holding the other party’s outside options fixed, necessarily reduces the quasi-rents associated with the parties’ relationship) tends to reduce the effect that information revelation can have on the payoffs of the informed party. Since the difference in the agent’s payoffs when the principal thinks the agent is a ‘high’ type and when the principal thinks the agent is a ‘low’ type is smaller when competition is present, the informed party’s incentives to conceal private information are reduced. Second, informed parties’ incentives to conceal private information are reduced whenever there is turnover in equilibrium: there is no point in making a costly investment to conceal private information from someone

with whom one is unlikely to interact in the future.⁵ Thus, it appears that either institutional barriers to competition among both agents *and* principals, or a high level of labor market frictions, may be more important to the existence of ratchet effects than previously realized.

2. A Model

Let worker utility be given by $U = w - \gamma V(e)$, where w is total compensation and $\gamma V(e)$ is the cost of effort, e .⁶ $V(e)$ has the usual form ($V(0) = 0$, $V' > 0$, $V'' > 0$); we shall use $V(e) = e^2$ to generate parameter values for the experiment. There are two types of workers, high and low ability (or “talent”), differentiated by their effort costs, $\gamma^H < \gamma^L$. Output is given by the production function $y = e$; since this is the same for both workers we henceforth use y exclusively to denote both output and effort. Firms’ profits are given by $\Pi = y - w$. Firms offer piece rate contracts of the form $w = -\alpha + \beta y$ to workers. In fact, we set $\beta = 1$, since this achieves the first best in a full information environment. This means that firms’ only decision is over the value of α . One interpretation of $\alpha \geq 0$ is as the rental fee the firm charges the worker for the right to use its plant and equipment; as is well known when $\beta = 1$ these fees will constitute the firm’s only source of profit.⁷

⁵ Ickes and Samuelson (1987) and Dearden, Ickes and Samuelson (1990) show theoretically that an *ex ante* commitment by an employer with market power to transfer employees between jobs can reduce ratchet effects when hidden productivity is job-specific. While similar in some aspects to our theoretical and empirical result about turnover, we note that our result applies to hidden information about *worker* productivity, and that turnover is an *ex post* optimal response to market competition rather than an *ex ante* commitment in our setup.

⁶ We focus on a single-task principal-agent setting throughout the paper. The entire analysis generalizes in a straightforward manner to a multi-task setting if the principal can observe a sufficient statistic for the agent’s performance on all tasks. If this is not the case, optimal incentives may be zero or very weak (Farrell and Shapiro 1989) in which case ratchet effects will not occur. As we have already noted, ratchet effects are, in some sense, an unintended consequence of performance pay systems; thus they are unlikely to be an issue when performance pay is not an important component of compensation.

⁷ Although explicit job ‘rental’ or entry fees are rarely observed in reality (and are in some cases illegal), it is well known that a variety of contracting mechanisms are equivalent to these fees. One example is a positive level of base compensation combined with a minimum output quota that must be reached in order to qualify for a piece rate; in

Under the above conditions, the socially optimal output levels are given by $y = \frac{1}{2\gamma}$. With $\beta = 1$ these are also the output levels that we expect workers to choose in a one-stage interaction with firms, as long as the level of α satisfies both workers' participation constraints. High-talent workers will produce more output than low-talent workers; in consequence both workers' types are revealed and a first-best outcome is attained.

Now imagine that workers and firms engage in a two-stage interaction, with the stages of potentially different length. The worker's type is still unknown to the firm at the start of the first stage, but the firm now might infer something about it from the worker's stage-one output. In particular, high stage-one output might signal high worker talent, implying that the firm can charge a higher rental fee in stage two without violating the worker's participation constraint. Anticipating this, high-talent workers might 'masquerade' as low-talent workers in the first stage. As a result, firms will continue to offer the low rental fee in the second stage, since they were unable to identify the worker's type in the first stage. Thus, we would observe inefficient pooling at the low output level in the first stage, especially if the first stage is short relative to the second. Finally, suppose that at the start of the second stage, all workers can receive a competing wage offer from another firm. As Kanemoto and MacLeod (1992) have shown, this can prevent 'incumbent' firms from exploiting high-talent workers who reveal their type in the first stage, even when outside firms do not observe workers' stage-one outputs. Our experiment tests this prediction, implementing the exact model described above with parameter values as described below.⁸ As noted, we also test the effect of firms' options to hire alternative workers.

this interpretation a higher rental fee (α) corresponds to a higher output quota. Note also that the 100% piece rates assumed refer to 100% of the firm's net revenues, not of gross sales. See Lazear (1998), chapter 5 for a discussion.

⁸ Kanemoto and MacLeod's model allows for a much more general menu of contracts than is feasible to implement in a laboratory experiment; nevertheless the same fundamental factors explain the effect of competition on the ratchet effect in their model as in our simpler version.

In our experiment, we set $\gamma^L = .010$ and $\gamma^H = .005$, which yields socially optimal output levels of ($\frac{1}{2\gamma} =$) 50 and 100 for the low- and high-talent workers respectively. Throughout the experiment, we restrict workers' output choices to these two levels, which we shall call 'low' and 'high' output respectively. Thus, if each worker type chooses his 'type-appropriate' output, a first-best outcome will be achieved. Asymmetric information-induced inefficiencies will take the form of workers choosing the type-inappropriate output level.

Workers receive a baseline payoff of zero if they do not work for any firm. If workers choose type-appropriate outputs, the total surplus generated by the employment relationship ($e - \gamma W(e)$) is 25 and 50 respectively for the low- and high-talent workers respectively; 25 and 50 are thus the maximum rental fees the firm can charge each type of worker if it knows the worker's type. In our experiment, we will allow firms to choose between two rental fees only: $\alpha^L = 15$ ("low") and $\alpha^H = 33$ ("high").⁹ We set the *ex ante* probability that a worker has high talent at $\frac{1}{3}$.¹⁰ Finally, in our implementation, workers and firms interact over either one or two stages, with the second stage twice as 'long' as the first (thus all the payoffs listed above are doubled, and the rental fees adjusted accordingly to 30 and 66). As noted, raising the relative

⁹ Most theoretical treatments of principal-agent and ratchet problems appear to assign all the bargaining power to principals, in the sense that equilibrium contracts are assumed to maximize the principal's utility subject to the agent's participation constraint. In most contexts this is only apparent, however, since this approach is simply a mathematical device for finding a Pareto-optimal contract. We take a different approach here by simply dividing the rents between the two parties in a way that ensures both will want to participate. Aside from realism --it seems likely that bargaining will lead to rent sharing in most cases--, allocating a nonzero share of the surplus to both parties has a practical purpose in our experiment: allowing firms to make 'take-it-or-leave-it' offers that claim the entire surplus would introduce ultimatum-game effects that are beyond the scope of the current paper.

¹⁰ If this probability is too high, a firm that does not know its worker's type will optimally choose the high rental fee, thereby shutting low-talent workers out of the market. Thus, in this case the pooling equilibrium at the heart of the ratchet model cannot exist, given our payoff parameters. A value of 1/3 gives firms adequate incentives to set the low fee and retain both worker types when the worker's type is unknown.

importance of the second stage raises workers' incentives to manipulate the information the firm has about them in that stage, making ratchet effects more likely.¹¹

Under the above assumptions, low-talent workers' stage-one payoffs if the firm posts the low rental fee are 10 and -15 at low and high effort levels respectively; if the firm posts the high fee they are -8 and -33 respectively. Thus, in a one-shot game with no strategic interactions, we expect low-talent workers to choose low effort if the firm posts the low rental fee, and to refuse to work for the firm at the high rental fee. Similar calculations show that high-talent workers find it optimal to supply high effort regardless of the rental fee (though they are obviously better off at the low fee). Their stage-one payoffs are 22.5 and 35 at low and high effort levels respectively if the fee is low, and 4.5 and 17 respectively if the fee is high. Thus we would expect to observe an efficient separating equilibrium if the game has only one stage. However, in a two-stage game with no competition – the baseline case in our experiment, and the case studied most frequently in the theoretical literature – a forward-looking high-talent worker will conceal his type in the first stage (thus sacrificing some current payoff) if he believes this will lead the firm to charge a lower second-stage fee, leading to an inefficient pooling equilibrium. In terms of pure strategies, this is demonstrated formally in Appendix A, which demonstrates that separating equilibria do not exist in this situation, while the pooling equilibrium does.¹²

Appendix A also characterizes the PBNE in our laboratory experiment when we introduce competition between firms or between workers. Briefly, Appendix A shows that pooling equilibria cannot exist in either the excess-firms or the excess-agents treatments. We

¹¹ The vast majority of theoretical ratchet effect models have only two periods; a noteworthy exception is Weitzman's (1980) infinite-horizon treatment. As Weitzman's analysis makes clear, what is important for the size of the ratchet effect is not the number of periods, but simply the importance of the future at any given time (in his case this reduces to the discount rate). We conjecture but have not proved that equilibrium in finite horizon ratchet models with more than two periods involves a transition from pooling in early periods to separation in later ones.

¹² Unless otherwise specified, we shall confine our discussion of predicted equilibria to pooling and separating equilibria in pure strategies. Thus, we generally omit the qualifier 'in pure strategies' unless we make specific reference to mixed strategies.

also show that equilibria in which agents' first-stage behavior fully reveals their type do exist in both cases (there is a unique PBNE in the excess-firms treatment and two PBNE in the excess-workers treatment). The precise structure of those equilibria and the intuition behind them is discussed in more detail in the following section.

3. Experimental design

Our experiment was conducted at the Groupe d'Analyse et de Théorie Economique (GATE), CNRS, France. The students were recruited from undergraduate courses in local Engineering and Business schools, by means of the ORSEE software (Greiner, 2004). In total, 159 people took part in the nine sessions (three for each treatment) of this experiment. Each participant was assigned a computer upon arrival, by a draw from an opaque bag. No one could participate in more than one session, and the same experimenters conducted all of the sessions. In each treatment, there was a conversion rate of 100 experimental units to €1. Average earnings were €14, including a €5 show-up fee.

Each participant was given the role of either a high-talent worker or a firm. The number of participants allocated to each category depended on the treatment. We decided to have automated low-talent workers, as their decisions were quite trivial (reject contracts with a high rental fee, as this would generate negative earnings for the worker, and accept contracts with low rental fees, since this provides positive earnings for both firms and workers).¹³ Since we expected virtually no deviation from this strategy, we programmed the low-talent workers to follow it. It was made common information in the instructions that the low-talent workers were

¹³ Even negative reciprocity could not come into play with human low-talent workers, as the firm is clearly not being antagonistic by choosing the lower rental fee.

robots and that they were programmed to follow this strategy.¹⁴

In order to facilitate comprehension, we couched our experiment in terms of the firm being the owner of a food concession stand on campus.¹⁵ Note that the firm's income is only what is received in rental fees. In the first part of a period, the firm is willing to rent the stand (to a worker) for one week; we restrict the firm to charging the low rental fee of 15 in order to provide a high-talent worker with a real choice, thus enabling us to gather useful data.¹⁶ If a firm or a worker does not end up in a contractual relationship, he or she receives nothing for that stage. If a high-talent worker accepts a contract, he or she chooses either low or high output, with the worker earning more in that stage from high output. Each firm is then informed about the output level (reject, low, or high) of the worker in the first stage, but not the worker's type.

In the second stage, the firm is free to choose either the low rental fee (30) or a high rental fee (66). Low-talent workers make their programmed choices, while high-talent workers choose to either reject the offer or to provide low or high output. In order to increase the cost of inefficiencies, this second stage 'lasted' two weeks, rather than one week. Firms were informed about the output chosen. Table 1 below presents the game and all possible payoffs.

¹⁴ The robots were programmed with different response times and a session proceeded to the next step only once all the players had entered their decisions. Therefore, the only evidence that a firm had concerning the nature of the worker was the choice made by the worker.

¹⁵ We attempted to insure comprehension by requiring participants to complete a "Comprehension questionnaire," which can be found in Appendix B, along with a sample of the experimental instructions.

¹⁶ Choosing the high fee of 33 gives an expected gain of only 11 for the firm in that stage. In addition, allowing the firm to choose the high fee in this stage might allow it to uncover the type of its worker but at the risk of having its offer rejected if matched with a low-talent worker. This would not test the ratchet effect.

Table 1 – The Game

First stage				
Worker's type	Rental fee	Worker's choice	Firm's payoff	Worker's payoff
High talent	15	Reject	0	0
		Low output	15	22.5
		High output	15	35
Low talent	15	Reject	0	0
		Low output	15	10
		High output	15	-15
Second stage				
Worker's type	Rental fee	Worker's choice	Firm's payoff	Worker's payoff
High talent	30	Reject	0	0
		Low output	30	45
		High output	30	70
	66	Reject	0	0
		Low output	66	9
		High output	66	34
Low talent	30	Reject	0	0
		Low output	30	20
		High output	30	-30
	66	Reject	0	0
		Low output	66	-16
		High output	66	-66

A key feature of this design is that if a high-talent worker chooses high output, he will receive 35 in the first stage, but will face a high rental fee if the firm realizes that she is matched with a high-talent worker, and will receive 34 in the second stage (so 69 in total). By comparison, the high-talent worker who chooses low output in the first stage earns 22.5 then and, if the firm subsequently chooses a low rental fee, earns 70 in the second stage (so 92.5 in total). Notice that, by construction, the choice of type-appropriate outputs by both worker types corresponds, literally, to a first-best allocation under the assumptions of the model.

Each session included five periods consisting of only the first stage, in order to familiarize the players with the environment of the game and to verify that the workers

understand that the high-output choice is best when there is no continuation. The instructions for the dynamic game were distributed only after completion of these five periods. The participants then played 20 periods of the two-stage game, with firms and workers randomly re-matched in each new period.

Baseline treatment

We had equal numbers of firms and workers in this treatment. There were 20 live participants in two sessions and 16 in the third session (due to no-shows). Fifteen participants were assigned to be ‘firms’ and five participants were assigned to be ‘high-talent workers’; there were also 10 automated low-talent workers.¹⁷ Each firm was matched with one worker of unknown type. There was no competition in this treatment, since the worker was paired with the same firm for both stages of every period. As already noted, and as Appendix A shows, pooling is a PBNE in this treatment, while no pure separating equilibrium can exist.

Excess-firms treatment

Both this treatment and the next one (excess workers) introduce labor market competition by giving subjects the opportunity to trade with more than one partner-- something that is not possible in the baseline model and treatment. The difference between them is which party (firms or workers) is on the long side of the market: the excess-firms treatment allows workers to consider employment offers from more than one firm; the excess-workers treatment allows firms to choose which of two workers to employ.¹⁸ Importantly, however, both these treatments expand the role of the participants’ outside opportunities relative to the baseline case, where

¹⁷ In the session with 16 participants, there were only four high-talent workers and 12 firms.

¹⁸ We could, of course, also have studied competition in ‘balanced’ markets with equal numbers of firms and workers; this would however raise a number of modelling issues involving the order in which offers are made and accepted/rejected (which could then have many permutations), or whether to model markets using some form of auction mechanism. The advantage of our excess-workers and excess-firms cases is both their simplicity and the fact that they bracket a large range of labor market conditions, thereby illustrating that the qualitative effects of competition on the ratchet effect do *not* depend on those market conditions.

labor market frictions are so high that not being matched with *any* trading partner is firms' *and* workers' best alternative to trading with their current partner.

In this treatment, we had twice as many firms as workers, with 21 live participants in each session. Eighteen participants were assigned to be 'firms' and three participants were assigned to be 'high-talent workers'. The other six workers were automated low-talent workers. In the first stage, each worker is paired with two firms. In the first stage, both firms make the offer of a low rental fee (15) to their paired worker.¹⁹ The worker chooses to accept at most one of these (identical) offers.

As before, the 'incumbent' firm learns the output level. In the second stage, both firms offer either a low or a high rental fee to the worker. If the worker accepts an offer, then he chooses either low or high output. If both offers involve the low rental fee, low-talent workers accept one at random; the high-talent worker is completely free to choose which (if either) offer to accept and to choose any output level. Indeed, the worker receives both offers at the same time and he can identify the firm with whom he contracted in the first stage. A firm is not informed about the offer of the other firm.

As Appendix A shows, the only PBNE for this treatment is a separating equilibrium, i.e. an equilibrium in which workers choose type-appropriate outputs in the first stage and where the incumbent firm correctly infers the workers' types. Importantly, however, the incumbent firms do not take advantage of this information; instead they offer low second-stage fees to all workers, regardless of the worker's first-stage effort choices. Firms offer low fees because if

¹⁹ To isolate the effects of market competition on information revelation (and also to keep the game simple enough for the laboratory context), we maintained the same menu of fees in all treatments. In an expanded model, competition is of course also likely to affect the level of the 'high' and 'low' fees. Since competition among workers will drive workers' utilities towards workers' non-market alternatives (zero) and competition among firms will do the same for firms, these endogenous changes in fees are likely to accentuate the effects that are both predicted and observed here: competition reduces the effects of information revelation on payoffs.

they do not, the risk that the worker will take the other firm's offer is too great. Finally, since workers of both types expect they will receive low-fee offers from both firms in the second stage *regardless* of workers' first-stage actions, workers simply maximize their first-stage utilities.

Excess-workers treatment

We had twice as many workers as firms in this treatment, with 15 live participants in two sessions and 10 in the third session (due to no-shows). Nine participants were assigned to be 'firms' and six participants were assigned to be 'high-talent workers' (respectively, six and four in the third session). The other 12 workers were automated low-talent workers. In the first stage, each firm is paired with two workers of unknown type. In the first stage, the firm makes an offer to one of the two workers; if this worker rejects the offer, then the same offer is made to the second worker. The second worker is not informed that the first worker has turned down the offer. As before, the firm learns the output level. In the second stage, the firm chooses to approach first either the worker who had accepted the contract in the first stage or the unemployed worker, and chooses either a low or a high rental fee.²⁰ Once again, if the offer is rejected, the same offer is then made to the other worker.

Appendix A shows that, once again, pooling is not a PBNE in this treatment, while two separating equilibria do exist. These equilibria share two key features: (a) firms make high second-stage fee offers when they see high first-stage output and low second-stage offers otherwise, but (b) there is turnover in equilibrium; specifically, incumbent workers who choose low first-stage effort are re-employed by the firm with probability strictly less than one. (The only difference between the two equilibria is in the structure and amount of turnover.) Even though low first-stage effort causes firms to choose a low second-stage fee, high-talent workers

²⁰ In principle, if neither worker accepted the contract in the first stage, the worker who first received the offer in the second-stage was randomly matched. However, this never happened.

are discouraged from choosing low first-stage effort because their low probability of future employment with the firm at this low fee does not justify this investment in concealment.²¹

4. Experimental results

Table 2 summarizes for each treatment the decisions made by the subjects in the five single-stage periods and then in each stage of the 20 two-stage periods.

Effort decisions

We first note that high-talent workers appear to have understood the basic game in the five single-stage periods. Across treatments in these periods, high-talent workers accepted a low-fee contract offer in 149 out of 151 occasions (98.68%), and chose high output on 144 occasions out of 151 (95.36%). In the baseline treatment, they accept a low-fee contract in 68 out of 70 occasions (97.14%) and chose high output in 65 out of 70 observations (92.86%). In the excess-firms treatment, the corresponding proportions are 100% and 95.6% and in the excess-workers treatment, they are both 100%. Thus, we see the predicted effort level an overwhelming proportion of the time in the static game with only one stage.

²¹ Predictions would differ somewhat if firms could change their offer after a rejection. Once again, high-talent workers will choose type-appropriate first-stage outputs in any PBNE, so there is no ratchet effect. The equilibrium firm strategies supporting this worker behavior differ however: Now, firms' equilibrium second-stage strategy, regardless of their beliefs about the worker's type, is a high-fee offer to the incumbent worker followed by a low offer to the unemployed worker. Anticipating this, high-talent workers are now discouraged from choosing low first-stage effort by the fact they will face the high second-stage fee regardless of their first-stage actions. Whether or not firms can make different offers to different workers, the underlying intuition is that improved outside options for firms reduce the scope for workers' first-stage actions to affect their second-stage payoffs.

Table 2 - Summary statistics

	Baseline (1)	Excess firms (2)	Excess workers (3)
<u>Single-stage periods</u>			
<u>Employees' choices</u>			
Accepted contracts	68 of 70 (97.1%)	45 of 45 (100.0%)	36 of 36 (100.0%)
High output	65 of 70 (92.9%)	43 of 45 (95.6%)	36 of 36 (100.0%)
<u>Two-stage periods</u>			
<u>Stage 1-Employees' choices</u>			
Accepted contracts	277 of 280 (98.9%)	180 of 180 (100.0%)	164 of 164 (100.0%)
High output	106 of 280 (37.9%)	180 of 180 (100.0%)	145 of 164 (88.4%)
<u>Stage 2 – Firms' offers</u>			
High fee after low output	87 of 731 (11.9%)	17 of 360 (4.7%)	154 of 334 (46.1%)
High fee after high output	98 of 106 (92.5%)	54 of 180 (30.0%)	136 of 145 (93.8%)
<u>Stage 2 - Employees' choices</u>			
Accepted contracts	274 of 280 (97.9%)	180 of 180 (100.0%)	211 of 231 (91.3%)
High output	265 of 280 (94.6%)	180 of 180 (100.0%)	211 of 231 (91.3%)
<u>Stage 2 - Switches</u>			
After High fee/output	-	By agents: 51 of 54 (94.4%)	By firms: 13 of 145 (9.0%)
After Low fee/output	-	54 of 136 (39.7%)	125 of 334 (37.4%)

Note: The table only reports the agents' decisions for the high-talent (human) agents. In the excess-workers treatment, the difference between the number of observations in stage 1 and in stage 2 is due to the fact that the firms do not necessarily offer contracts to the same agent in the two stages. In column 2, agents determine whether to switch; in column 3, firms determine whether to switch.

How do matters change when there are two stages? Table 2 shows the proportion of high-talent workers who chose the high output in the first stage, after having received an offer with a low rental fee. In the baseline treatment, we see considerable evidence that workers are aware of the consequences of revealing that they have high talent by choosing the high output in the first stage (although not all of the subjects play the PBNE of the game). In fact, 106 of 280 times that a contract was offered (37.86%),²² high-talent workers chose a high output while in 171 cases (61.07%), they chose a low output, deferring immediate gains in favor of future

²² The contract was rejected (inexplicably, since doing so also reveals high talent) three times of 280 (1.07%).

benefits. This 61.07% contrasts greatly with the 4.29% low-output rate observed in the static game of this treatment (it also contrasts with the considerably lower percentages of type-concealing observed in the previous experimental studies of the ratchet effect). A Wilcoxon signed-rank test, with each of the 14 individual high-talent workers treated as an independent observation, indicates that the proportion of periods in which workers have chosen the low output throughout the game is significantly higher in the first stage of the two-stage game than in the single-stage game ($p = 0.003$).²³ In fact, there were four workers who always chose a high output, apparently failing to grasp the basic principle involved; one other worker acted almost randomly, choosing each of the high and low outputs nine times and rejecting the low-fee rental contract twice, suggesting some degree of confusion. The other nine workers rarely chose high outputs in the first stage, and these choices were generally made in the first periods of a session.

We can also compare these choices to those of the high-talent workers in the second stage of the baseline treatment, where (at least direct) strategic considerations are absent. In 265 of the 280 instances (94.64%) where a high-talent worker was offered a contract in the second stage, he chose a high output.²⁴ This behavior is in stark contrast to the 37.86% likelihood of choosing the high output in the first stage, where the future looms large.²⁵ In sum, the dramatic change in workers' behavior between the one-period and two-period versions of our baseline model is strongly suggestive of ratchet effects. Indeed, we find it difficult to think of alternative

²³ In most of the non-parametric statistics reported in this paper, the unit of observation is the average value of each individual's decisions across periods. Indeed, in a strict sense each session is only one independent observation, since we use a random-matching protocol. Since we have only a few sessions in each treatment, we conduct most of these tests by averaging the choices of each participant in all periods to one number; while this approach ignores the interactions among the participants, we believe it is nevertheless informative. When unspecified, tests are conducted with each individual as the unit of observation.

²⁴ The contract was rejected six times (2.14%) and the low output was chosen nine times (3.21% of the contracts offered). All six rejections were of the high-rental-fee contract. Strangely, eight of the nine choices of low output came after a low-fee contract was offered.

²⁵ The average individual output is significantly higher in the second stage than in the first one in the baseline (Wilcoxon signed-rank test, $p = 0.003$).

hypotheses that might explain this behavioral change; most hypotheses that come to mind, in fact predict the opposite, or no effect. For example, a worker who (erroneously) perceives high first-period effort as conferring a direct benefit to the firm might be motivated, in a two-period model but not a one-period model, to make a gift of high effort to ‘his’ firm in the hope of securing a lower fee in period two. Myopia, on the other hand, predicts no difference between the one- and two-stage games. We believe, but cannot prove, that the main reason why ratchet effects emerge more strongly and quickly in our experiment than in previous experiments is our simpler design, which is easier for subjects to understand and focuses on the main strategic interactions (the choice of first-period effort by talented workers, and of second-period compensation by firms).

Behavior again changes greatly when there is competition between firms. All of the subjects play the PBNE of the game. High-talent workers accepted the contract offers and chose the high output in every one of the 180 instances when a high-talent worker was offered a contract in the first stage. It seems quite clear that, in line with the Kanemoto and MacLeod (1992) model, competition for scarce workers leaves high-talent workers unafraid to reveal their type early in the game. What is perhaps more surprising is that the ratchet effect also vanishes when there is a scarcity of firms, so that workers compete with each other. High-talent workers always accept an offer, and they choose the low output in only 19 of 164 instances (11.59%) in the first stage.²⁶ Put differently, 88.41% of these observations coincide with the PBNE of the game. A Wilcoxon signed-rank test indicates, however, that the average individual output is significantly lower in the first stage of the two-stage part than in the first part ($p = 0.046$), although 12 out of 16 individuals always choose the high effort in all periods. We conjecture

²⁶ An analysis of individual behavior shows that only four of the 16 high-talent workers in this treatment ever chose the low output in the first stage. One of these was responsible for 11 of the 19 low outputs observed.

that most workers realize that firms would like to be able to charge the high rental fee, so that signaling high talent is useful for obtaining *any* offer in the second stage.²⁷

Because of the longitudinal dimension of the data, we estimate random-effects Probit models to identify the determinants of the choice of a high output in the first stage of the game. The independent variables include a dummy variable for the treatment (with the no-competition baseline treatment equal to 1, and 0 otherwise), a time trend (the period number from 1 to 20), and individual characteristics, namely gender (with ‘male’ equal to 1, and 0 otherwise) and cognitive abilities proxied by the distinction obtained on the final high-school exam (that can take values 1 for no distinction, 2 for adequate, 3 for good, and 4 for very good) and by the mark obtained in mathematics in this exam (this variable can take any value between 0 and 20, inclusive). Table 3 displays the results of these estimations for data pooled across treatments in column 1 (excluding the excess-firms treatment since all subjects chose high output) and the baseline treatment alone in column 2.

²⁷ While it seems obvious that the percentage of low outputs with high-talent workers in the first stage differs across treatments, we can also provide a very conservative nonparametric test, which treats each session as one independent observation, bypassing the interaction issue; this test confirms statistical significance. The percentage of low outputs with high-talent workers in the first stage was 52%, 74%, and 59% in the three baseline sessions; these percentages were 0%, 0%, and 0% in the three sessions with excess firms, and 8%, 4%, and 27% in the three sessions with excess workers. Thus, the Wilcoxon-Mann-Whitney rank-sum test finds significant differences between the baseline and both the excess-firms treatment ($p = 0.050$) and the excess-workers treatment ($p = 0.050$), using each session as only one independent observation; the difference between the two treatments with competition is also significant ($p = 0.050$). Taking the individual as a unit of observation, the mean output in the first stage is significantly lower in the baseline than in the excess-firms treatment (Wilcoxon-Mann-Whitney test, $p = 0.002$) and than in the excess-workers treatment ($p = 0.002$). There is no significant difference in this respect between the excess-firms and the excess-workers treatments ($p = 0.111$).

Table 3 - Determinants of a high-talent worker choosing the high output in the first stage (Random-effects Probit models)

Dependent variable: Choice of a high output	Treatments	
	Baseline + Excess workers (1)	Baseline (2)
Period	-0.071 (.020) [-.015]	-0.069 (.023) [-.027]
No competition (baseline) treatment	-3.908 (1.083) [-.626]	-
Male	-.429 (.829) [-.090]	.029 (1.476) [.012]
Distinction on the High school certificate	-.360 (.919) [-.076]	1.229 (1.097) [.486]
Math skills	-.061 (.232) [-.013]	-.529 (.327) [-.209]
Constant	6.793 (2.823)	4.842 (3.921)
N	441	277
Log likelihood	-112.852	-79.592
Wald χ^2	24.65	11.71
Prob > χ^2	.000	.020

Note: Standard errors are in parentheses and marginal effects are in brackets. “No competition treatment” is a dummy variable that takes the value 1 if the treatment is the baseline, and 0 otherwise. Period takes the value of each period. “Male” is a dummy variable equal to 1 if the subject is a male and 0 otherwise. “Distinction on the High school certificate” can take values 1 for no distinction, 2 for adequate, 3 for good, and 4 for very good. “Math skills” is the mark obtained in mathematics in the final high-school exam and it can take any value between 0 and 20, inclusive. In model (1), the mean (standard deviation) value of the “male” variable is 0.669 (0.719); in model (2), it is 0.430 (0.496). In model (1), the mean (standard deviation) value of the “Distinction on the High school certificate” is 2.660 (0.779); in model (2), it is 2.726 (0.689). In model (1), the mean (standard deviation) value of “Math skills” is 14.079 (2.722); in model (2), it is 13.711 (2.432).

Table 3 confirms that, in the first stage, the high-talent workers choose high output significantly less frequently in the baseline treatment than in the excess-workers treatment (and *a fortiori* than in the excess-firms treatment). Indeed, in the first model the marginal effect of the baseline treatment compared with the excess-workers treatment is -0.567, and the coefficient of this variable is significant at the 1% level. This provides support for the qualitative predictions of the model, since the high-talent workers are significantly less likely to reveal their type in the

first stage of the game when there is no competition in the market. The coefficient of the time trend (the “period” variable) is also significant at the 1% level, suggesting that subjects learn to play strategically in the baseline.²⁸

Choice of rental fees

We next consider the behavior of the firms. Figure 1 shows for each treatment the relative frequency of the high rental fee being offered by firms in the second stage, both overall and also according to whether the paired worker chose a high or low output level in the first stage. According to Figure 1, firms in all treatments were much more likely to choose a high rental fee in the second stage when the paired worker produced a high output in the first stage than when he produced a low output. In contrast, the relative frequencies of the high fee, both overall and also according to whether the paired worker chose a high or low output level in the first stage, are significantly different across treatments.²⁹

²⁸ As a robustness test, we have estimated the same models with robust standard errors and clustering at the individual level. The only difference is that the time trend is significant at the 5% level in model (1) and at 10% in model (2). Consistent with our interpretation of this trend as a learning effect, more detailed inspection of the data reveals that the time trend in the baseline treatment stems almost entirely from the first ten periods.

²⁹ Averaging values by individual and considering each firm as an observation, Mann-Whitney tests conclude that overall, firms propose higher fees in the baseline than in the excess-firms treatment ($p < 0.001$) and that firms in the excess-workers treatment offer higher fees than in both the excess-firms ($p < 0.001$) and the baseline treatments ($p < 0.001$). Conducting these tests at the session level, the conclusions are similar for the comparison between the baseline and both the excess-firms and the excess-workers treatments ($p = 0.050$).

Figure 1- Relative frequency of firms offering a high rental fee in stage 2 depending on the worker's output in stage 1, by treatment



In the baseline treatment, firms overall chose the high rental fee 22.10% of the time. This relative frequency amounts to 92.45% after a high output was produced by the worker in the first stage, and to only 11.90% after a low output was observed. A Wilcoxon signed-rank test with each of the 36 firms as an individual observation indicates that the fee is significantly higher after a high output was produced ($p < 0.001$). This indicates that only a minority of firms are reluctant to offer the high fee when the worker has produced a high output in the first stage, probably because they fear rejection if a high fee is proposed. On the other hand, only a few firms propose the high fee after the worker has produced a low output in the first stage; these firms are willing to take a chance that it was a high-talent worker who chose the low output in the first stage. Overall, these results confirm that choosing to conceal one's ability in the first stage is *ex post* the better strategy.

When there are excess firms, firms choose a high rental fee in the second stage 13.15% of the time. This is significantly lower than in the baseline treatment (see footnote 26). Only 4.72% of the firms chose the high fee after a low output was observed and this percentage increased to 30% after a high output was observed. This difference is significant (Wilcoxon test, $p < 0.001$). Put differently, 70% of the firms who are able to identify their worker's type (since he produced the high output) play the PBNE of this game, refraining from charging the high fee. At first glance, the 30% rate of firms choosing the high fee after a high output appears to be a serious consequence of revelation; however, since the worker was paired with two firms, there was a risk that the other firm would offer a low rental fee. In fact, after revealing his type in the first stage, the high-talent worker nevertheless received at least one offer with low rental fee in the second stage in 174 of 180 cases (96.67%), so choosing high output in the first stage was actually rarely costly for the worker.

As shown by Figure 1, the picture is significantly different with excess workers, where high rental fees were chosen 60.54% of the time overall. A Mann-Whitney test indicates that this difference is highly significant with both the baseline and the excess-firms treatments ($p < 0.001$). Now workers appear more desperate to find employment, so that revealing high talent has strategic value. Recall that in this treatment, in the two pure-strategy separating Bayesian Nash equilibria of the game, firms should make high second-stage fee offers when they see high first-stage output and low second-stage offers otherwise. Indeed, the firms capitalize on the information, as firms chose the high rental fee 93.79% of the time in the second stage after observing a high output in the first stage. So here the firms capture the lion's share of the available rents. But when firms learn of low output in the first stage, they seem to be uncertain of the right course of action, and the high rental fee is chosen in the second stage 46.11% of the

time. A Wilcoxon test, with each of the 50 firms as one independent observation, indicates that firms charge more the high fee after observing a high output in the first stage than after a low output ($p < 0.001$), as would naturally be predicted.

Table 4 presents a regression that highlights the determinants of whether a firm chooses the high rental fee in the second stage. We estimate random-effects Probit models on second-stage data pooled across treatments (1) and on each treatment separately (2 to 4), taking into account the fact that firms make repeated decisions. In all models, except model (3) for the excess-firms treatment, we only consider observations when the firm was matched with a worker in the first stage. In model (1), “excess firms treatment” and “excess workers treatment” are dummy variables that take the value 1 if the corresponding treatment is in use, and 0 otherwise. The other explanatory variables include the worker’s choice of “high output in the first stage” (the variable takes the value 1 if the worker has chosen the high output in stage 1, and 0 otherwise). We interact this variable with each treatment in the first regression. In regression (3), we add a dummy for whether the firm was selected in the first stage and the same variable is interacted with the choice of a high output by the worker (the “firm selected and high output in the first stage” dummy variable) since we predict that being matched with an agent who has chosen high output in stage 1 should lead the firm to offer a high rental fee in stage 2. We also include a time trend (the period number from 1 to 20) and the same individual characteristics as in Table 3 (namely the gender, the distinction obtained on the final high-school and the mark obtained in mathematics in this exam). The mean and standard deviation of each independent variable are given in Table A in Appendix C.

Table 4 - Determinants of a firm choosing the high rental fee in the second stage

Dependent variable: Offer of a high fee	Treatments			
	All treatments (1)	Baseline (2)	Excess firms (3)	Excess workers (4)
Period	-.013 (.007) [-.004]	.003 (.011) [.0007]	-.028 (.010) [-.003]	-.014 (.013) [-.005]
Excess firms treatment	-.577 (.240) [-.147]			
Excess workers treatment	1.393 (.249) [.464]			
High output in first stage	3.327 (.253) [.903]	3.310 (.267) [.899]		2.209 (.246) [.512]
High output in first stage in Excess-firms treatment	-1.921 (.306) [-.256]			
High output in first stage in Excess-workers treatment	-1.184 (.338) [-.204]			
Firm selected in the first stage			-.515 (.154) [-.057]	
Firm selected and high output in the first stage			1.326 (.177) [.274]	
Male	.380 (.184) [.111]	.135 (.300) [.030]	.084 (.275) [.009]	1.074** (.435) [.339]
High school certificate cum laude	-.090 (.113) [-.025]	-.064 (.186) [-.014]	-.064 (.156) [-.007]	-.073 (.290) [-.024]
Math skills	.011 (.035) [.003]	-.015 (.056) [-.003]	.055 (.056) [.006]	.009 (.085) [.003]
Constant	-1.396 (.486)	-1.224 (.696)	-1.828 (.040)	-.291 (1.157)
Number of observations	1856	837	1080	479
Log likelihood	-644.250	-262.788	-332.878	-209.867
Wald χ^2	322.08	154.23	65.06	85.20
Prob> χ^2	.000	.000	.000	.000

Note: Standard errors are in parentheses and marginal effects are in brackets. “Excess firms treatment” and “Excess workers treatment” are dummy variables that take the value 1 if the corresponding treatment is in use, and 0 otherwise. The “high output in the first stage” variable takes the value 1 if the worker has chosen the high output in stage 1, and 0 otherwise. “Period” takes the value of each period. “Male” is a dummy variable equal to 1 if the subject is a male and 0 otherwise. “Distinction on the High school certificate” can take values 1 for no distinction, 2 for adequate, 3 for good, and 4 for very good. “Math skills” is the mark obtained in mathematics in the final high-school exam and it can take any value between 0 and 20 included. In model (3), the “firm selected in the first stage”

variable is a dummy taking the value 1 if the firm was matched with a worker in stage 1, and 0 otherwise. The “firm selected and high output in the first stage” variable is equal to 1 if the firm was matched with a worker in stage 1 who produced a high output, and 0 in all other cases.

The regressions show no significant time trend except in the excess-firms treatment.³⁰ Firms’ behavior is mainly influenced by the structure of the labor market and by the worker’s behavior in the first stage. In comparison to the baseline treatment, firms were substantially more likely to offer a high-fee contract in the excess-workers treatment and substantially less likely to do so in the excess-firms treatment. This can be seen from model (1), indicating that the probability of choosing the high rental fee increases by 46.4 percentage points in the excess-workers treatment compared with the baseline (this is significant at the 1% level), while it decreases by 14.7 percentage points in the excess-firms treatment (significant at the 5% level), all else equal. This shows that firms reacted strongly to the labor-market conditions, consistent with the qualitative theoretical predictions.

Overall, firms were more likely to choose the high-fee contract after having observed a high output in the first stage, as the probability of choosing the high fee is increased by a remarkable 90.3 percentage points in this case (significant at the 1% level). This is consistent with the qualitative predictions of the theoretical model when the firm can identify the talent of his worker from his first-stage choice of output. In the excess-firms treatment, being selected in the first stage reduces the likelihood of offering a high fee in the second stage by 5.7 percentage points all else equal (significant at the 1% level), but the worker choosing a high output in stage one instead of a low output increases the likelihood by 21.7 percentage points (same level of

³⁰ In the excess-firms treatment, the coefficient associated with the “period” variable is significant with a 99% confidence interval. This suggests that the individuals’ decisions evolve over time, possibly because firms learn to cope with competition as they accumulate more experience in that treatment. As a robustness test, we have estimated the same models with robust standard errors and clustering at the individual level. The only difference is that the time trend loses significance when data are pooled. In contrast the excess-firms treatment dummy gains significance at the 1% level as well as the high output in first stage in excess-workers treatment in regression (1).

significance). This marginal effect of the choice of a high output in stage 1 on the probability of offering a high fee in stage 2 is smaller than in the other treatments (it is 89.9 percentage points in the baseline and 51.2 in the excess-workers treatment). This is likely due to the fact that when firms must compete with other firms to hire a worker, they are less likely to exploit the high talent of their worker (as revealed in the first stage) than in the other labor-market conditions, as predicted. Indeed, recall that the *only* Perfect Bayesian Nash equilibrium in this treatment is a separating equilibrium, in which workers choose type-appropriate outputs in the first stage and incumbents do not take advantage of this information in the second stage—they offer low second-stage fees to all workers, regardless of the worker’s first-stage effort choices.

Switching decisions

When there is competition on the market, the switching decisions are also informative. Table 5 displays the workers’ decisions in the second stage of the excess-firms treatment depending on the second-stage fees chosen by the incumbent firm and its competitor.

Table 5 – Workers’ decisions to switch firms in the second stage, excess-firms treatment

Incumbent firm’s offer	Low 2 nd -stage fee			High 2 nd -stage fee		
	High fee	Low fee	Total	High fee	Low fee	Total
Competitor’s offer						
Decision to switch	0 (0)	54 (42.86)	54 (42.86)	3 (5.56)	48 (88.88)	51 (94.44)
Decision not to switch	12 (9.52)	60 (47.62)	72 (57.14)	3 (5.56)	0 (0)	3 (5.56)
Total	12 (9.52)	114 (90.48)	126 (100)	6 (11.12)	48 (88.88)	54 (100)

Note: Percentages are in parentheses.

This table shows that it never pays for a firm to choose the high rental fee in the second stage since doing so leads the agent to select the contractual offer of the competitor. When both firms choose the high fee, the workers are indifferent.³¹ In a random-effects Probit model analyzing the workers’ probability to switch firms (not reported here but available upon request),

³¹ Interestingly, we do not find clear evidence of inequity aversion. Indeed, inequity-averse workers would switch firms who make similar offers to reduce the difference of payoffs between the two firms across the period.

we find that being matched with two firms choosing a high fee reduces the likelihood that the worker switches firms by 41.83 percentage points, while being matched with two firms choosing the low fee reduces this likelihood by 32.70 percentage points.³²

Similarly, Table 6 reports descriptive statistics on the firms' decisions to switch agents between the first and the second stages in the excess-workers treatment.

Table 6 – Firms' decisions to switch workers in the second stage, excess-agents treatment

Second stage fee	Worker chooses high effort in stage 1			Worker chooses low effort in stage 1		
	High fee	Low fee	Total	High fee	Low fee	Total
Decision to switch	10 (6.90)	3 (2.07)	13 (8.97)	66 (19.76)	59 (17.66)	125 (37.43)
Decision not to switch	126 (86.90)	6 (4.14)	132 (91.03)	88 (26.35)	121 (36.23)	209 (62.57)
Total	136 (93.79)	9 (6.21)	145 (100)	154 (46.11)	180 (53.89)	334 (100)

Note: Percentages are in parentheses

As indicated by Table 6, nearly 87% of the firms whose first-stage worker selected high effort propose the high fee in the second stage to the same worker. In a random-effects Probit model analyzing the firms' probability to switch workers (not reported here but available upon request), we find that being matched with a worker choosing a high output in the first stage reduces the likelihood that the firm switches workers in the second stage by 31.67 percentage points, a significant change.³³ Decisions are more complex when the first-stage worker selects low effort, presumably because this reveals less information to firms. Still, in both of the pure-strategy PBNE of the excess-workers treatment, firms in this situation should always make their

³² This regression is based on a random-effects Probit model in which the independent variable is the worker's decision to switch firm between stage 1 and stage 2. The independent variables include two dummy variables indicating that both firms offer the high fee or the low fee, a time trend and demographic variables (gender, cognitive abilities proxied by the distinction obtained on the final high-school exam and by the mark obtained in mathematics in this exam). The number of observations is 180. The log-likelihood is -103.885. Only the two dummy variables capturing the firms' offers of the high fee or the low fee are significant ($p = 0.049$ and $p < 0.001$, respectively).

³³ This regression is based on a random-effects Probit model in which the independent variable is the firm's decision to switch worker between the two stages. The independent variables include the output of the worker in the first stage, a time trend and demographic variables (gender, cognitive abilities proxied by the distinction obtained on the final high-school exam and by the mark obtained in mathematics in this exam). The number of observations is 479. The log-likelihood is -231.209. Only the output of the worker in the previous stage is significant.

first offer – a low fee-- to the unemployed worker. In our experiment, however, only 37% of the firms in this situation switched workers at the end of stage 1. This may be due to naive play or to a reluctance to change workers. Also in contrast with the two pure strategy PBNE, 46 percent of firms in this situation offered the high fee at the start of stage two. This should not happen in the pure strategy PBNE but might be part of a mixed-strategy equilibrium to this game.

Earnings

Finally, we examine the total earnings of firms and workers in our experiment, and assess their consistency with the theoretical predictions of our model. According to the model, firms should earn more (and workers less) when there is competition among workers than in the baseline treatment. This is confirmed by Mann-Whitney tests at the individual level, with $p = 0.002$ for firms and $p < 0.001$ for workers; at the session level, $p = 0.050$ for both firms and workers. Similarly, firms should earn less (and workers more) in the excess-firms treatment than in the baseline. For firms, this is also confirmed by Mann-Whitney tests at the individual level ($p < 0.001$); at the session level, $p = 0.050$). For workers, it is confirmed at the individual level with $p < 0.001$, and at the session level with $p = 0.050$.³⁴

To summarize, viewed through the lens of our theoretical model, a reasonable interpretation of the results from our two competitive treatments seems to be the following. The excess-firms treatment improves workers' outside options (by adding the possibility of trading with a different firm) while holding firms' outside options fixed (at zero). After this

³⁴ The model performs less well in predicting the actual levels of earnings. For firms, these are 51.21 ECU with excess workers, 47.11 in the baseline, and 22.62 with excess firms. Comparing these with the predicted levels from Appendix 1 (57, 45 and 22.5 respectively), two-sided t -tests with each individual as an observation indicate that actual earnings are significantly lower than predicted in the excess-workers treatment ($p < 0.001$), higher than predicted in the baseline ($p = 0.007$), and not different from the prediction in the excess-firms treatment ($p = 0.830$). High-talent workers' actual mean earnings are 103.80 with excess firms, 80.16 in the baseline, and 43.66 with excess workers. Comparing these to the predicted levels (105, 92.5 and 59.2 respectively), two-sided t -tests indicate that actual earnings are significantly lower than predicted in the excess-firms treatment ($p = 0.004$), the baseline ($p < 0.001$), and the excess-workers treatment ($p < 0.001$).

improvement, it is risky for firms to demand a high rental fee even if the firm knows that the worker has high talent, since the worker might choose to work for another firm instead. As a result, most firms tend to post low fees even when they observe high first-stage output, i.e. regardless of the worker's first-stage behavior. Anticipating this, high-talent workers are less motivated to undertake inefficient first-stage actions to conceal their identity.

The excess-workers treatment, in contrast, improves firms' outside options (by adding the possibility of trading with a different worker) while holding workers' outside options fixed (at zero). Now firms can offer the high fee initially to either the employed or the unemployed worker if they observe high first-stage output, and a large majority does this. More importantly, however, the fact that firms exercise their option to make offers to unemployed workers in equilibrium reduces high-talent workers' incentives to conceal their identity by picking low first-stage output. Put more succinctly, competition among firms restricts firms' *ability to take advantage* of private information if they have it, while competition among workers restricts the amount workers can hope to *gain by concealing* private information. In both cases, increased competition improves the transmission of private information.

5. Conclusion

Since at least Akerlof's (1970) classic article, it has been widely recognized that asymmetric information can create market failures that are robust to the presence of competition. Indeed, in some simple asymmetric information models (such Akerlof's "lemons" model), a reduction in trading frictions can cause markets to completely collapse, leading to significant welfare losses. In this paper, however, we provide an example from the labor market where increased competition *improves* the performance of markets with asymmetric information: output restriction by workers essentially disappears when we introduce *ex post* competition for either

workers *or* jobs. While Kanemoto and MacLeod (1992) predicted this in a more general model of competition between principals, to our knowledge no studies have predicted or tested this effect of *ex post* competition between agents.

Intuitively, ratchet effects differ from other asymmetric-information problems in that information is concealed by one party in order to prevent the other party from exploiting its *ex post* monopoly power in a repeated relationship. Thus, one might expect that reductions in such *ex post* power tend to eliminate ratchet effects. In more detail, improving *either* party's *ex post* outside options reduces the total amount of quasi-rents available to be divided between the agent and principal. This, in turn, reduces the effect that information revelation can possibly have on the informed party's payoffs, thus reducing the payoffs to concealing that information. Notably, this process does not require the market (in our case, non-incumbent principals) to observe the same things (in particular, the previous performance of the agent) that are seen by the contracting parties; thus it operates even when the outside 'market' is at an informational disadvantage relative to insiders. A second, important reason why *ex post* competition alleviates asymmetric-information problems is the fact that it introduces the possibility of turnover. Simply put, the private return to making (costly) investments in concealment is reduced if informed agents are less likely to interact with the thus-uninformed party in the future. While our model is highly specific, it seems likely that both of these principles carry over to much more general contexts.

Perhaps the above considerations help explain the increasing use of incentive pay in the U.S. economy (see for example Kuhn and Lozano 2008, Table 11; or Lemieux, MacLeod and Parent 2009), as increasing international competition, falling mobility costs, deregulation, and de-unionization have made U.S. labor markets more competitive. According to our model and our laboratory evidence, such increased competition should reduce the scope for strategic output

restriction by workers of the sort once familiar in case studies of U.S. industry. Under the assumptions of our model, this raises economic efficiency by mitigating the asymmetric information problem that gives rise to those restrictions.

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Appendix A: Equilibrium in the Three Treatments

A.1 Baseline (No Competition) Case

In the second stage, high-talent workers accept both low (30) and high (66) fees, yielding a profit to the firm equal to the fee charged. Low-talent workers accept only the low fee, yielding a profit of 30 if the low fee is charged and zero if the high fee is charged. Thus, the firm's expected profits if it charges the high fee are $66p + 0(1-p) = 66p$, where p is the firm's subjective probability that the worker is high-talent. The firm's profits from charging the low fee are 30, independent of its beliefs because both worker types accept this offer. Thus, the firm will post the high fee at the start of stage two iff $66p > 30$, or $p > 5/11$.

Now suppose a separating equilibrium in pure strategies in which both workers choose type-appropriate first-stage outputs, thus revealing their types. Firms who observe a high first-stage output will therefore set $p=1$ and charge a high second-stage fee. Thus, in equilibrium, a high-ability worker's payoff will be 35 in the first stage and 34 in the second stage for a total of 69. A high-productivity worker who deviates from this equilibrium by choosing low output in the first stage receives $22.5 + 70 = 92.5$. Thus there can be no such separating equilibrium.

In a pooling equilibrium, both worker types select low output in stage 1; thus $p = 1/3$ at the start of stage 2. The firm therefore sets a low fee in the second stage. Equilibrium payoffs are $22.5 + 70 = 92.5$ for the high-talent workers. Now consider the expected payoffs of a high-productivity worker who deviates from this equilibrium by selecting high output in the first stage. Since low-talent workers always choose low effort, firms should infer that any worker deviating to high effort is of high talent, and will impose a high second-stage fee if such deviations are observed, leading to a payoff of $35+34=69$. Thus it does not pay a high-talent worker to deviate from this pooling equilibrium. It also doesn't pay for the firm to deviate by charging a high fee in the second stage after observing low output, since $p = 1/3 < 5/11$ in our design. Therefore a pure pooling equilibrium does exist.

In sum, in the baseline (no competition) case, there is only one pure strategy equilibrium. It is a pooling equilibrium in which both worker types select low effort in the first stage. Firms learn nothing from workers' first-stage behavior and offer low fees to both worker types in stage two. These offers are always accepted. Firms' predicted payoffs are therefore 15 and 30 in stages 1 and 2, for a total of 45. Talented workers' predicted payoffs, as already noted, are $22.5 + 70 = 90$.

A.2 Excess Firms Case

In this case two firms (the "incumbent" and "outsider") play a simultaneous fee-offer game to an agent of (potentially) uncertain type in the second stage. Depending on the worker's output history at the incumbent firm, the firms might have different priors concerning the worker's type. Assuming that workers choose at random between identical offers, the payoff matrix confronting these two firms is given in the table below, where p and q respectively are the incumbent and outsider firms' beliefs (at the start of the second stage) that the worker is high talent.

	Outside Firm's Offer			
	Low fee		High fee	
Incumbent Firm's Offer:	Incumbent's payoff:	Outsider's payoff:	Incumbent's payoff:	Outsider's payoff:
Low Fee:	15	15	30	0
High fee:	0	30	$33p$	$33q$

These values are derived as follows: If both firms choose the low fee (30), this is accepted regardless of the worker's type. In this case (and whenever workers or firms face identical offers henceforth), we assume that the worker will choose at random from the two identical offers for an expected payoff of 15. If the two firms choose different offers, either type of worker will choose the low offer; thus the firm charging the high fee gets a payoff of zero, and the other firm's offer is accepted with certainty for a payoff of 30. If both firms charge a high fee, the incumbent firm's expected payoff is 66 (the high fee) times its assessment of the probability the worker is of high talent (p), times .5 (the chance that the worker picks the incumbent). The outside firm's payoff in this situation is calculated in the same manner.

Examination of this table reveals that the low fee is a strictly dominant strategy for the outside firm iff $q < 10/11$. Next, note that the outside firm does not observe the worker's first-stage effort decision. (It does, of course, observe which firm the worker chose to work for in the first stage, but this choice reveals no information about the worker since both firms offer the low fee in the first stage). Thus we must have $q=1/3$, and the outside firm will always offer a low fee. Given this, then according to the above payoff table the incumbent will always choose a low fee as well; in other words given Bayesian beliefs for the outside firm, the only Nash equilibrium to this simultaneous-offer second-stage subgame is for both firms to offer a low fee, *regardless* of the incumbent firm's beliefs. (Even if it is certain its worker has high talent, the incumbent firm will not demand a high fee because it should rationally anticipate that its competitor will offer a low fee).

Finally, since workers of both types know they will receive low-fee offers from both firms in the second stage *regardless* of workers' first-stage actions, workers' first-stage actions will simply maximize their first-stage utilities. The *only* Perfect Bayesian Nash equilibrium in the excess-firms case is thus a separating equilibrium, in which workers choose type-appropriate outputs in the first stage; incumbents correctly infer the workers' types in the second stage but do not take advantage of this information—they offer low second-stage fees to all workers, regardless of the worker's first-stage effort choices.³⁵ The expected payoff of firms is just $15 + 30 = 45$, since all firms make low fee offers in both periods, and these offers are always accepted by both worker types. High-ability workers face low fees and choose high effort in both periods, so their equilibrium payoffs are $35 + 70 = 105$.

A.3 Excess Workers Case

In this treatment, firms must offer the same fee to the employed and unemployed workers, but may make the offers in any order. Thus, a firm now must choose among four second-stage actions, offering the high or low fee to either the employed or unemployed worker. (Any offers that are turned down are then offered to the other worker).

Low fees are always accepted by the first worker to get them; thus the firm's payoff when it offers the low fee to either worker first is just 30 (the low fee). When the firm offers the high fee, then regardless of the order of the offers, the firm receives a payoff of zero if neither the employed or unemployed worker is able, and 66 otherwise. The probability neither worker is able is $\text{Prob}(U \text{ low talent} \mid E \text{ low talent}) \cdot \text{Prob}(E \text{ low talent}) = (11/17) \cdot (1-p)$. Thus the firm will offer the high fee iff $(11/17) \cdot (1-p) \cdot 66 > 30$, or $p > 36/121$

³⁵ This result, like Kanemoto and MacLeod's, is robust to the introduction of small worker mobility costs. To see this, imagine that, when faced with identical offers, workers choose the incumbent firm's offer with probability $r > 0.5$. Then for all $r \in [0.5, 1]$, the outside firm's strictly dominant strategy is still to set the low fee for all $q < 10/11$. Since q must still equal $1/3$ for outside firms, outside firms must offer the low fee in any equilibrium. Given this, the incumbent firm can expect $30r$ if it sets the low fee and zero if it sets the high fee. Thus, once again the only equilibrium has firms making low-fee offers to all workers in the second stage, regardless of their first-stage actions. Collusion between firms to impose the high fee when facing a high-talent worker is very unlikely due to the information structure of the game.

$\approx .296$. As with the low fee, the firm is indifferent to which worker receives the offer first; this is true regardless of its prior, p .

Given these facts, we can now check for the existence of certain kinds of equilibria.

a) *Can a pure separating equilibrium exist?*

By definition, in a pure separating equilibrium, all high-talent workers choose high first-stage output and all low-talent workers choose low first-stage output, and therefore firms set $p = 1$ when they see high first-stage output and $p = 0$ when they see low first-stage output. It thus follows from the above facts that, in any pure separating equilibrium, firms will offer a high fee in the second stage whenever the employed worker's first-stage output was high, and a low second-stage fee to both workers when the employed worker's first-stage output was low.

We need to consider four different types of pure separating equilibrium.

Case 1: Firms initially make their second-stage offer to the employed worker, regardless of the employed worker's first-stage output choice. In this proposed separating equilibrium, an able worker gets $35 + 34 = 69$. If he deviates by picking low first-stage output, he gets $22.5 + 70 = 92.5$, so he will deviate. So this separating equilibrium does not survive.

Case 2: Firms initially make their second-stage offer to the unemployed worker, regardless of the employed worker's first stage effort choice. In this proposed separating equilibrium, an able incumbent worker gets $35 + (12/17)*34 = 59$ (the firm chooses the high fee, having observed high output in stage 1). If he deviates by picking low first-stage output (in which case the firm chooses a low fee in stage 2, since the firm believes that the employed worker is less able and $(6/17)*66 < 30$) he gets $22.5 + 0 = 22.5$, so there is no profitable deviation. This is a PBNE

Case 3: Firms initially make their second-stage offer to the employed worker if the employed worker's first-stage output choice is high, and to the unemployed worker if the employed worker's first-stage output is low. In this proposed separating equilibrium, an able incumbent worker gets $35 + 34 = 69$. If he deviates by picking low first-stage output, he gets $22.5 + 0 = 22.5$, so there is no profitable deviation. This is a PBNE

Case 4: Firms initially make their second-stage offer to the unemployed worker if the employed worker's first-stage output choice is high, and to the employed worker if the employed worker's first-stage output choice is low. In this proposed separating equilibrium, an able worker gets $35 + (12/17)*34 = 59$. If he deviates by picking low first-stage output, he gets $22.5 + 70 = 92.5$, so he will definitely deviate. So this separating equilibrium does not survive.

Putting all these cases together, there are two pure-strategy, separating PBNE. In both, firms make high second-stage fee offers when they see high first-stage output and low second-stage offers otherwise, and incumbent workers who choose low first-stage effort are re-employed by the firm with probability strictly less than one.³⁶

³⁶ If we allow firms to randomize over which worker receives the second-stage offer first, additional equilibria *with these same two features* also emerge. For example, consider just the class of equilibria in which the "routing" of the firm's second-stage offer is not conditional on the observed first-stage output and let α be the probability that the firm makes its first offer to the employed worker. Then, combining cases 1 and 2, the able incumbent gets $35 + 34\alpha + (12/17)34(1 - \alpha)$ in the separating equilibrium. He gets $22.5 + 70\alpha$ if he deviates. It follows that there is also a continuum of PBNE in which the employed worker unconditionally receives the first offer with probability $\alpha < 73/120$.

b. Can a pure pooling equilibrium exist?

Recall that we force all our low-talent workers to always choose low effort, so pooling can only occur at the low level of first stage output. Further, if all workers choose low first-stage output, firm's second stage prior, p , will equal $1/3$ for all workers, both employed and unemployed. Therefore, firms will always prefer the high second-stage fee, since the likelihood that at least one of the workers will accept the offer is $1 - (2/3)*(11/17) = 29/51$, and $(29/51)*66 > 30$.

We now consider an able worker's incentives to deviate from these proposed pooling equilibria. (Low-talent workers cannot deviate, by construction, and wouldn't want to anyway because choosing high output is a dominated strategy for them no matter how firms interpret it). We therefore assume the firm interprets high output in stage 1 as a signal of high talent with probability 1.

Suppose first that a high-talent incumbent worker gets the first opportunity at the firm's second-stage offer whether he deviates or not. Then he will deviate for sure, since he gets $22.5 + 34 = 56.5$ in the pooling equilibrium and $35 + 34 = 69$ if he deviates. He is already facing a high second-stage fee in the pooling equilibrium, so why not just pick high effort in the stage?

Without explicitly going through all the cases of offer order, it is apparent from the above that the only way a pooling equilibrium can be immune to deviations is if a deviating, high-talent worker was somehow "punished" for high first-stage effort by a lower likelihood of getting the first chance at the second stage offer (recall that firms are indifferent to offer order for all p). The most extreme case possible here is where the incumbent worker always gets the first chance at the second-stage offer when he picks low effort, but is second in line when he picks high effort in a deviation.

In this case, the able worker again gets $22.5 + 34 = 56.5$ in the pooling equilibrium and $35 + (12/17)*34 = 59$ if he deviates. Thus, there is a profitable deviation even in this extreme case. We conclude that no pure pooling equilibrium can exist.³⁷

c. Expected payoffs.

Expected payoffs of firms and able workers in the two pure-strategy equilibria to the excess-workers treatment are computed in a document that is available from the authors. We find that these two equilibria are, in fact, payoff equivalent, both yielding expected payoffs 57 to firms and 59.2 to high-ability workers.

In sum, the theoretically predicted payoffs of firms and talented workers in all three treatments are summarized in the following Table.

Predicted Payoffs of Firms and High-Talent Workers, by Treatment

	Treatment		
	Baseline (no competition)	Excess Firms	Excess Workers
Firms	45	22.5	57
High-Talent Workers	92.5	105	59.2

³⁷ These predicted equilibria are also robust to the introduction of small mobility costs. To see this, imagine that inexperienced workers' payoffs, whenever they are employed, are c less than experienced workers' payoffs because such workers must incur startup costs. Then, for all $c \in [0, 20)$, unemployed workers' choices will be exactly the same in all circumstances as when $c=0$, and firms' profits are unaffected as well. Thus, the entire proof goes through unchanged. (The predicted equilibria *do* change if mobility costs are directly deducted from *firms'* payoffs - i.e. from the fees charged-- rather than workers' payoffs, but this does not seem realistic in this case where (a) workers are residual claimants on profits and (b) are in excess supply.)

Appendix B – Instructions for the excess-firms treatment

(other instructions available upon request)

We thank you for participating in an experiment on decision-making. During this session, you can earn money. The amount of your earnings depends on your decisions and on the decisions of the other participants in this session. During the session, your earnings will be calculated in Experimental Currency Units, with:

100 ECU= 1 Euro.

The session consists of two parts, each including several periods. The earnings you have made during these 2 parts will be added up and converted into Euros. In addition, you will receive € 5 for participating in the experiment. Your earnings will be paid to you in cash in private.

Your decisions are anonymous and confidential.

There are three categories of players: firms, low-productivity workers and high-productivity workers. The number of firms is twice the number of workers. The high-productivity workers represent one third of the total number of workers. For example, as in this session, 18 participants are firms, 6 participants are low-productivity workers and 3 participants are high-productivity workers. The low-productivity workers are not human-subjects but computer-subjects.

During each stage of each part, two firms are matched with one worker randomly chosen. Firms do not know the productivity of this worker. The chances are **one in three** (33%) that they are matched with a high productivity (human worker). The chances are two in three (67%) that the firms are matched with a low-productivity computer-worker. At each new period, firms and workers are re-matched randomly.

You are allocated either the role of a firm or the role of a high-productivity worker at random. You will be informed of your role at the beginning of the first part and you will keep the same role throughout the session.

You have received the instructions for the first part of the session. The instructions for the second part will be distributed after the first part has been completed.

The first part consists of 5 periods.

First part - Description of each period

Each of the two firms is the owner of a food concession stand on a University campus. They are willing to rent their stands to the worker for one week. The two firms charge a rental fee of ECU 15 to use the stand. This rental fee is the only source of earnings of the firms. The worker can accept at most one offer.

- If the worker rejects both offers, the firms and the worker earn 0 ECU.
- If the worker accepts one offer and rents a stand, he or she buys all his supplies for the week and keeps all the proceeds from sales.

The worker who rents the stand chooses to deliver either a low output (i.e. serving a low number of customers) or a high output in the week (i.e. serving a high number of customers).

As mentioned earlier, the low-productivity workers are computers in this experiment. They always accept one of the two 15-ECU offers at random and they choose the low output. Therefore, the firm who rents the stand to a low-productivity computer-worker earns automatically ECU 15 and the other firm earns 0 ECU.

A high-productivity worker earns ECU 22.5 net if he or she chooses the low output and he or she earns ECU 35 net if he or she produces the high output. In both cases, the firm who rents the stand to a high-productivity worker earns ECU 15 and the other firm earns 0 ECU.

The net payoffs in ECU (i.e. after payment of the rent and supplies) associated with each possible decision of the participants are summarized in the following Table:

<i>High-productivity worker's choice</i>	<i>Firm's payoff</i>	<i>High-productivity worker's payoffs</i>
<i>Rejects the offer</i>	0	0
<i>Low output</i>	15	22.5
<i>High output</i>	15	35

<i>Low-productivity worker's choice (*)</i>	<i>Firm's payoff</i>
<i>Low output</i>	15

(*) A low-productivity computer-worker always accepts the offer and always chooses the low output.

The timing of decisions is the following:

- The two firms offer to their worker to rent the stand for ECU 15. The worker receives both offers at the same time.
- If the worker is a low-productivity worker (i.e. a computer-worker), it accepts the offer of one firm chosen randomly and it chooses the low output.
- If the worker is a high-productivity worker, he or she chooses between accepting one of the two offers and rejecting both offers. If he or she accepts one of the two offers, he or she chooses between the low and the high output.
- The firm who has been chosen is informed of these choices but he or she is not informed of whether she was matched with a high-productivity worker or with a computer-worker. The other firm is only informed that his or her offer has not been accepted.
- One's own payoff is displayed and the period ends.

At the end of a period, a new period starts automatically. Each period is independent. Firms and workers are re-matched at the beginning of each new period.

Throughout the entire session, direct communication between participants is strictly forbidden. If you have any question regarding these instructions, please raise your hand. Your questions

will be immediately answered in private. Please answer the comprehension questionnaire that will be displayed on your screen.

Second part - Description of each period
(Instructions distributed after the first part has been completed)

The second part consists of 20 periods.

Each period now consists of two stages. These two stages represent the first week (Stage 1) and the next two weeks (Stage 2), respectively.

You keep the same role as in the first part.

Two firms are matched with the same worker, randomly chosen, during the two stages of one period. At each new period, firms and workers are re-matched randomly.

Stage 1

The rules for Stage 1 are exactly the same as for Part 1.

Each of the two firms proposes to rent his or her food concession stand in the first week. The firms charge a rental fee of ECU 15. This rental fee is the only source of earnings of the firms. The worker can accept at most one offer.

- If the worker rejects both offers, the firms and the worker earn 0 ECU in this stage.
- If the worker accepts one of the two offers, he or she chooses to deliver either a low output or a high output in the week.

As before, the low-productivity workers are computers. They always accept one of the offers and they choose the low output. The firm who is chosen earns ECU 15 and the other one earns 0 ECU.

A high-productivity worker earns ECU 22.5 net if he or she chooses the low output and ECU 35 net if he or she chooses the high output. In both cases the firm who is chosen earns ECU 15 and the other firm earns 0.

The net payoffs in ECU in the first stage associated with each possible decision of the participants are summarized in the following Table:

<i>High-productivity worker's choice</i>	<i>Firm's payoff</i>	<i>High-productivity worker's payoffs</i>
<i>Rejects the offer</i>	0	0
<i>Low output</i>	15	22.5
<i>High output</i>	15	35

<i>Low-productivity worker's choice (*)</i>	<i>Firm's payoff</i>
<i>Low output</i>	15

(*) *A low-productivity computer-worker always accepts the offer and always chooses the low output.*

The timing of decisions is the following:

- The two firms offer to their worker to rent the stand for ECU 15.
- If the worker is a low-productivity worker (i.e. a computer-worker), it accepts the offer of one firm chosen randomly and it chooses the low output.
- If the worker is a high-productivity worker, he or she chooses between accepting one of the two offers and rejecting both offers. If he or she accepts one of the two offers, he or she chooses between the low and the high output.
- The firm who has been chosen is informed of these choices. The other firm is only informed that his or her offer has not been accepted.
- One's own payoff in Stage 1 is displayed and Stage 1 ends.

Stage 2

For the right to use their concession stand for the next two weeks, the firms choose to charge a rental fee of either ECU 30 or ECU 66. The worker can accept at most one offer.

- If the worker rejects both offers, both firms and the worker earn 0 ECU in this stage.
- If the worker accepts one offer, he or she chooses to deliver either a low output or a high output in the next two weeks.

The low-productivity computer-workers always reject the high rental fee offer of ECU 66. They always accept the low rental fee offer of ECU 30 and choose the low output. If both firms offer the high rental fee of ECU 66, they reject both offers. Therefore, a firm who is matched with a computer-worker earns 30 if he or she chooses the low rental fee and 0 if he or she chooses the high rental fee.

The net payoffs in ECU for the next two weeks associated with each possible decision of the participants in Stage 2 if the firm charges the low rental fee (ECU 30) are summarized in the following Table:

<i>High-productivity worker's choice</i>	<i>Firm's payoff</i>	<i>High-productivity worker's payoffs</i>
<i>Rejects the offer</i>	0	0
<i>Low output</i>	30	45
<i>High output</i>	30	70

<i>Low-productivity worker's choice (*)</i>	<i>Firm's payoff</i>
<i>Low output</i>	30

(*) A low-productivity computer-worker always accepts the offer with the low rental fee and always chooses the low output.

Note that the above payoffs are just two times the payoffs in Stage 1.

The net payoffs in ECU for the next two weeks associated with each possible decision of the participants in Stage 2 if the firm charges the high rental fee (ECU 66) are summarized in the following Table:

<i>High-productivity worker's choice</i>	<i>Firm's payoff</i>	<i>High-productivity worker's payoffs</i>
<i>Rejects the offer</i>	0	0
<i>Low output</i>	66	9
<i>High output</i>	66	34

<i>Low-productivity worker's choice (*)</i>	<i>Firm's payoff</i>
<i>Rejects the offer</i>	0

(*) A low-productivity computer-worker always rejects the offer with the high rental fee.

The timing of decisions is the following:

- Each firm chooses between the low rental fee (ECU 30) and the high rental fee (ECU 66). The agent receives both offers at the same time.
- If the worker is a low-productivity computer-worker, it rejects both offers if both firms propose the high rental fee. It accepts one offer randomly if both firms offer the low rental fee. If only one firm offers the low rental fee, it accepts this offer and it chooses the low output.
- If the worker is a high-productivity worker, he or she chooses between accepting one of the two offers and rejecting both offers. If he or she accepts one of the two offers, he or she chooses between the low and the high output.
- The firm who has been chosen is informed of these choices. The other firm is only informed that his or her offer has not been accepted. The firms are not informed of whether they were matched with a high-productivity worker or with a computer-worker.
- Own payoffs in Stage 2 and for the whole period are displayed. The payoff of the period is the sum of the payoffs of each stage.

At the end of a period, a new period starts automatically. Each period is independent. Firms and workers are re-matched randomly at the beginning of each new period.

Please answer the comprehension questionnaire that will appear in your screen.

Comprehension questionnaire – Part 1

	Correct	Wrong
1) I can be a low-productivity agent	<input type="checkbox"/>	<input type="checkbox"/>
2) I am always paired with the same two participants	<input type="checkbox"/>	<input type="checkbox"/>
3) If I am a firm, I can choose the rental fee	<input type="checkbox"/>	<input type="checkbox"/>
4) If a high productivity worker chooses the high output, - he or she earns 22.5 - the firm earns 15	<input type="checkbox"/> <input type="checkbox"/>	<input type="checkbox"/> <input type="checkbox"/>
5) A low-productivity worker chooses one firm randomly, accepts the fee proposed by this firm and chooses a high output	<input type="checkbox"/>	<input type="checkbox"/>

Comprehension questionnaire – Part 2

	Correct	Wrong
1) I am paired with the same two participants in the two stages of each period	<input type="checkbox"/>	<input type="checkbox"/>
2) I am paired with the same two participants in all periods	<input type="checkbox"/>	<input type="checkbox"/>
3) If I am a firm, I can choose the amount of the rental fee in Stage 1	<input type="checkbox"/>	<input type="checkbox"/>
4) In Stage 1, if a high productivity worker chooses the high output, he earns 22.5	<input type="checkbox"/>	<input type="checkbox"/>
5) In Stage 2, if a high-productivity worker accepts the fee of 66 and chooses the high output , he or she earns 35	<input type="checkbox"/>	<input type="checkbox"/>
6) In Stage 1, the output chosen by a low-productivity worker depends on the rent proposed by the firms he or she is matched with	<input type="checkbox"/>	<input type="checkbox"/>
7) In Stage 2, a low-productivity worker always rejects a fee of ECU 66	<input type="checkbox"/>	<input type="checkbox"/>

Appendix C. Table A – Summary statistics of the determinants of a firm choosing the high rental fee in the second stage (see regressions in Table 6)

Dependent variable: Offer of a high fee	Treatments			
		All treatments (1)		All treatments (1)
Period	15.51 (5.80)	15.50 (5.77)	15.50 (5.77)	15.52 (5.76)
Excess firms treatment	0.29 (0.45)	-	-	-
Excess workers treatment	0.26 (0.44)	-	-	-
High output in first stage	0.23 (0.42)	0.13 (0.33)	-	0.30 (0.46)
High output in first stage in Excess-firms treatment	0.10 (0.30)	-	-	-
High output in first stage in Excess-workers treatment	0.08 (0.27)	-	-	-
Firm selected in the first stage	-	-	0.50 (0.50)	-
Firm selected and high output in the first stage	-	-	0.17 (0.37)	-
Male	0.38 (0.49)	0.33 (0.47)	0.43 (0.49)	0.46 (0.50)
High school certificate cum laude	2.58 (0.92)	2.48 (0.96)	2.65 (0.89)	2.67 (0.85)
Math skills	15.04 (3.08)	14.36 (3.22)	15.09 (2.61)	15.95 (2.97)
Nb observations	1856	837	1080	479