

EMPLOYMENT PROTECTION, BONUS PAY, AND LABOR MARKET PERFORMANCE

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ABSTRACT. All industrialized countries have some form of employment protection legislation (EPL) limiting the ability of an employer to fire a worker. The appropriate level of protection is a contentious policy issue that is hampered by the difficulty of evaluating the impact of specific legislation. Competing intuitions suggest that EPL may harm market efficiency, or that it may have zero effect if parties have sufficiently flexible contractual instruments for making appropriate side payments. This paper shows how one may use a laboratory experiment to study this tension explicitly. An experiment provides a convenient, cost effective method for evaluating EPL in a context where market interactions are too complex to be adequately evaluated from a purely theoretical perspective. We show that EPL has a strong negative impact on efficiency when contracts involve only up-front wages. Consistent with the Coasian prediction, however, simply adding the option for firms to offer bonuses almost entirely undoes the negative impact of EPL. Thus our findings highlight a reason why EPL may have mixed or weak effects in the field. We also explore underlying mechanisms, and provide a theoretical model that can rationalize heterogeneity in individual behavior by allowing for non-standard preferences for fairness or integrity.

1. INTRODUCTION

All industrialized nations have some form of employment protection legislation (EPL) that limits the ability of an employer to dismiss a worker (OECD (1999)). The impact of EPL on labor market outcomes is one of the most intensely debated economic policy issues today. Even in the absence of EPL institutions, relationship-specific investments increase the cost of worker turnover (see Mincer (1962) for a classic early study). Hence, the more general question is: how does making firing more costly affect market performance?

On the one hand, firing costs would seem to have strong negative implications for market efficiency, by preventing the breakup of bad matches, and by removing the threat of firing as a device for discouraging worker shirking. On the other hand, the Coasian intuition articulated by Lazear (1990) suggests that given sufficiently many contractual instruments, parties will be able to arrange side payments that circumvent any negative effects of barriers to dismissal. This paper shows how one can use a laboratory experiment to provide sharp results on these predictions, something which is hard to do in the field.

Evaluating the impact of EPL or firing costs in the field is extremely difficult. First of all, legal change is a complex process in which it is very difficult to disentangle the before and after effects of EPL legislation on efficiency or other outcomes. Moreover, each country passes laws that are specific to its jurisdiction and not necessarily comparable to laws in other countries. Hence, cross country data sets always face the problem that law changes are not comparable, and that other factors other than the law may affect economic

performance.¹ A common solution to this problem has been to use US data where one can suppose that law changes in different states represent different, independent experiments.² Yet, as Krueger (1991) argues, employment protection law in US states may simply be a response to what is happening in the courts, and hence one should be cautious when making causal claims regarding the effect of the law. Firing costs from other sources, such as relationship-specific investments, are also difficult to study in the field, because firing costs are typically not observed directly. Finally, studying the impact of firing costs on efficiency is hampered by the fact that one generally has at best rough proxies for efficiency in the market, in the proper economic sense of total gains from trade.

An experiment offers a useful complementary approach. A key benefit of an experiment is that the treatment is truly exogenous. Also, the relevant variables are observed with zero measurement error, including market efficiency. Moreover, laboratory experiments with human subjects are much less expensive than attempting to evaluate legislation in a field context. Of course, the cost is that one may argue that the laboratory setting is quite artificial, with stakes that do not come close to those that we observe in practice. Our analysis shows, however, that in the experimental setting one can explore carefully a number of standard labor economics questions surrounding EPL that would be impossible to explore in a field setting.

We build upon the framework developed in Brown, Falk, and Fehr (2004) who provide an experimental implementation of the relational contract model developed by MacLeod and Malcolmson (1989). This experiment allows for a complex labor market setting in which workers and firms can meet and endogenously enter into long term relationships. Alternatively, firms can hire workers on a one-shot basis. The employment contract is incomplete in the sense that one cannot enforce performance with court enforced sanctions.³

Given the finite length of the market game, the unique Nash equilibrium entails no trade or production each period. As we know from work in behavioral economics, however, in practice parties do achieve some cooperation in these games (see, e.g., Fehr, Gächter, and Kirchsteiger (1997)). Experimental research also typically reveals strong heterogeneity in individual behavior, which is not captured by standard theory. In particular, a sub-population of individuals exhibits a preference for reciprocity, rewarding a high wage with high effort, even in one-shot games. Note that a sufficient number of reciprocal types could make the barriers to firing completely irrelevant for performance. The point here is that the impact of dismissal barriers is an empirical question, which cannot be answered by relying upon theory alone.

We implement a 2x2 design, varying exogenously whether there is an EPL institution in the market, and varying the menu of labor contracts available to firms. EPL takes the form such that after a firm hires a worker beyond an initial probationary period, the worker can no longer be dismissed. This corresponds to a case where firing costs are sufficiently high that dismissal is not profitable. The labor contract varies in terms of whether firms are restricted to offering a fixed wage and asking for a desired effort level (the type of contract studied extensively in the efficiency wage literature), or whether firms also have the additional option to offer (unenforceable) bonuses, to be paid after observing worker performance.

¹See Botero, Djankov, La Porta, Lopez-de Silanes, and Shleifer (2004) for a nice example of this approach.

²See Miles (2000), Autor, Donohue, and Schwab (2004), Autor, Donohue, and Schwab (2006). and MacLeod and Nakavachara (2007).

³See Williamson, Wachter, and Harris (1975) for a classic study of this issue.

We show that exogenously introducing EPL does have a strong negative impact on aggregate efficiency, in the market with wage contracts. Simply adding the bonus option, however, is sufficient to almost completely undo the negative impact of barriers to dismissal, consistent with the prediction of Lazear (1990). The resilience of the market is striking, given that it requires adding only one additional contract instrument to substantially ameliorate a relatively strong form of EPL. The important message of the experiment is that the impact of firing costs in the field may depend strongly on a specific contingency, namely the presence or absence of performance pay. Given the prevalence of some form of performance pay in many employment relationships, either in the form of bonuses, or promotions, or other deferred compensation, the implication is that efficiency costs of EPL or other firing costs are generally likely to be weak.

How do EPL, and bonus pay, affect firms? We show that EPL harms firm profits, whereas bonus pay strongly benefits firms, especially in the presence of EPL. This suggests that absent restrictions on contract form, firms should be observed to endogenously adopt bonus pay in response to the introduction of EPL, further reducing the impact of the institution. This prediction is born out in our analysis of behavior at the micro level, discussed below, where we observe firms making greater use of bonus pay in the treatment with EPL than in the treatment without. It is also broadly consistent with secular trends in the US, which show concomittant increases in the strength of EPL (Autor, Kerr, and Kugler (2007)) and the prevalence of performance pay (Lemieux, MacLeod, and Parent (2007)).

For workers, EPL has a slight negative impact on payoffs. This is consistent with the view that EPL may actually harm workers, by lowering efficiency, even though EPL is typically adopted based on arguments about improving worker welfare. Bonus pay has a strong negative impact on worker earnings, which implies that attempts by firms to adopt this type of compensation may be met by worker resistance. This is consistent with the traditional opposition of unions to performance pay.⁴ It also implies that regions such as Europe, which have strong EPL but also strong unions, may have relatively little performance pay, with a cost in terms of aggregate efficiency.

Turning to inequality, in terms of variance of worker payoffs, we find that EPL has little effect but that bonus pay does significantly increase inequality. This suggests another reason why performance pay may be unpopular, and is consistent with recent evidence that rising wage inequality in the US is strongly related to the increasing prevalence of performance pay (Lemieux, MacLeod, and Parent (2007)).

After presenting our main findings on the aggregate effects of EPL, we provide additional results on the ways that dismissal barriers affects various micro level behaviors. These results tell a consistent story explaining *why* we observe our aggregate results. They are also strikingly consistent with various pieces of empirical evidence from the field on EPL and labor market behavior, and show how these different findings fit together in one framework.

Summarizing briefly, we show that threat of firing is a key incentive device, due to the presence of a substantial number of workers who are motivated by material self-interest rather than reciprocity, and that the moral hazard problem created by EPL leads to worker shirking and explains the drop in market efficiency. Notably, not all workers are motivated by firing threat; consistent with reciprocity, some exert non-minimal effort even in the final period. Field evidence on worker shirking is scarce, but our findings are consistent

⁴See Taylor (1911) for an early study.

the sharp increase in worker absenteeism that tends to occur after the probation period for EPL (Ichino and Riphon (2005)). We also find suggestive evidence that firing costs harm efficiency by interfering with the ability of firms to repeatedly try out workers and screen out those who tend to shirk. The impact of EPL on adverse selection has been studied in the literature, in terms of worker ability (see, e.g., Kugler and Saint-Paul (2004)); our findings highlight the potential for an additional, important type of selection, namely in terms of worker propensity to shirk.

Allowing bonus pay undoes both of these effects of dismissal barriers by giving firms an alternative incentive device. That bonus pay would be a credible incentive is not obvious *a priori* from the perspective of standard theory (MacLeod and Malcolmson (1998)). Credibility of bonus pay can be explained by the presence of firms with a taste for reciprocity, revealed by rewarding high effort with a bonus even in the final market period. Whereas EPL has a negative impact on efficiency in the case of wage contracts, in spite of the presence of some reciprocal workers, EPL has little impact in the presence of bonus pay, precisely because bonus pay helps enlist the reciprocity of firms as well. The results on bonus pay contribute to a recent literature comparing the efficiency of bonus pay versus wage contracts (Gneezy (2005); Fehr, Kellin, and Schmidt (2007); Wu and Roe (2007)), and are the first to show how the relative benefits of bonus pay versus wages are magnified in the presence of firing costs. We also find that EPL discourages firms from entering long-term relationships in the first place, pushing them instead to use the spot market for labor, consistent with field evidence showing that EPL is associated with greater reliance of firms on temp agency workers (Blanchard and Landier (2002); Cahuc and Postel-Vinay (2002); Autor (2003)).

A recurrent theme in our micro analysis is the presence of substantial heterogeneity in individual behavior, which is not captured by the standard model of economic behavior. In many cases standard economic contract models widely used in the literature make predictions that are simply inconsistent with the facts. Behavior is consistent with incentive theory, however, especially in a coasian sense, once one takes into account non-standard preferences such as reciprocity. The analysis concludes with a theoretical interpretation of the results, which allows for heterogeneous motives. The goal of the theory is not so much to provide tight predictions as to organize the data and provide comparative results that explain our observed treatment effects.

The rest of the paper is organized as follows. Section 2 describes the experiment, and Section 3 presents our main results on the aggregate impact of EPL. Section 4 investigates how EPL affects micro level behaviors, and how this leads to our aggregate results. Section 5 provides the theory, and Section 6 concludes.

2. THE EXPERIMENT

Our experiment builds upon the work of Brown, Falk, and Fehr (2004). They implement a complex labor market setting that captures many realistic features of actual labor markets. Firms and workers meet repeatedly in a market where they can form and break relationships. They find that when contracts are complete, then parties can achieve an efficient outcome. In Lazear (1990) observes that in such a setting, employment protection should not affect performance.

The more interesting setting is when contracts are incomplete. In this case, Brown, Falk, and Fehr (2004) find that the potential to form, and then break a long term relationship is essential for market performance.

We extend this experiment in two ways. On the one hand, we allow firms to offer more complex contracts that entail an end-of-the-period bonus. On the other hand, we look at what happens to market performance if workers have a probationary period during which the worker may be dismissed, but after which the firm cannot fire the worker. The next section describes this design in detail.

2.1. Design. The labor market operated for 18 trading periods. In each period a firm could hire at most one worker, and a worker could have at most one job. An individual period involved two or three phases, depending on the treatment. The first phase was always a market phase, in which the firms made contract offers and workers could only accept or reject. Firms could make as many contract offers as they wanted during the time limit of three minutes; if one of a firm's contracts was accepted, all of the other offers by that firm were immediately removed from the market.⁵ In the case that a firm and a worker agreed on a contract, they entered a second phase in which the worker could decide how much effort, e to exert. In treatments where the contract offer could include an offered bonus, there was a third phase in which the firm was informed about the worker's effort choice and could decide how much of a bonus, b , to pay. Importantly, neither the worker's effort level or the firm's bonus payment were restricted by the initial contract agreement, whereas a wage specified in the agreement was binding. After the second (third) phase, the firm and worker were informed about their profits and earnings, respectively, and then a new period began.

Contract offers consisted of a wage, w , a desired effort level, \tilde{e} , and in some treatments an offered bonus, \tilde{b} . The offer also included the firm's ID number. Firms could make two types of contract offers during the market phase: public offers or private offers. Public offers were observed by all workers, and thus were available to any worker. Private offers were observed only by a worker specified by the firm, and thus were available only to that particular worker. In the case that a firm made a private offer, the firm specified a worker's ID number, in addition to the contract terms. Worker and firm ID numbers remained constant over the entire 18 periods, so it was possible for a firm to intentionally make a private offer to the same worker over multiple periods, and for a worker to recognize offers coming from a specific firm. This design made it possible for a firm and worker to endogenously form a long-term relationship, by choosing to repeatedly engage in private-offer contracts with each other over multiple periods. Public offers were a way for firms to engage in a spot market for labor rather than engaging in long-term relationships. During the market phase, firms were kept constantly informed about which workers had already accepted a contract, so as to avoid having firms make a private offer to a worker that was no longer available.

In treatments with EPL, the firm lost the ability to fire a worker after making a second private offer in a row to the same worker. This design captures a common feature of EPL institutions, which is a specified probation period during which the firm is still able to fire the worker. In practice firms can at a cost fire workers after they are permanently employed. In our experiments we effectively set this price at infinity. In future research it would be interesting to explore the consequence of costly, but finite dismissal costs. On the other hand, a strong form of EPL such as the one we implement is useful for providing a tough test of the ability of bonus pay to overcome the effects of EPL.

⁵If all firms had contracts, there was no potential for further trades. Thus, the market phase was designed to end automatically after three minutes, or after the last firm had a contract offer accepted, whichever came first.

Having chosen to hire the worker again, after the initial private offer, EPL took effect and the firm had to make an offer to that same worker at the beginning of each subsequent period until the end of the game or until the worker decided to quit. Firms in EPL contracts made their offers in a special phase before the market phase. Importantly, once EPL was initiated, the wage offer had to be at least as high as in the previous period. Some rigidity of the wage is required for an EPL institution to work, otherwise a firm could effectively fire a worker by reducing the wage to zero.⁶

After firms had made their EPL offers, the market period began and workers on EPL could see the standing offer from the firm, in addition to the other market activity. At any time, the worker could accept the standing offer, in which case the firm was informed. Alternatively, the worker could reject the offer by accepting another contract in the market. As soon as the worker rejected the standing offer, the firm was informed, and allowed to make offers during the remainder of the market phase.

In our design we abstract away from several issues sometimes discussed in the literature on EPL. The effort cost function for workers, described below, is the same across all individuals so there are no differences in ability. This allows us to focus on the moral hazard problem in terms of effort and bonus payment, without the complication of adverse selection. We also do not implement cyclical shocks to market conditions, or worker redundancies (multiple workers at one firm). This simplifies an already complex inter-temporal choice environment, and makes it possible to first understand the impact of EPL on the strategic behavior surrounding the contract enforcement problem.

We implemented four exogenous treatments. In a treatment called T-Baseline, contracts were wage-only. There was no EPL institution in the market, so firms could engage in as many private offers in a row with a worker as they wanted, while always having the option to fire the worker, i.e., not make the worker a private offer in the next period. This treatment is very similar to the ICF treatment Brown, Falk, and Fehr (2004), except that their design involved only 15 trading periods rather than 18. In T-Bonus, there was no EPL, but firms had the option to offer bonuses. In T-EPL, contracts were wage-only, but we introduced our EPL institution. In T-EPL-Bonus, the EPL institution was in effect, but firms had the option to offer a bonus, in addition to or instead of a wage.

2.2. Parameters, Information Conditions, Procedure, and Subject Pool. All market sessions lasted 18 periods, and had 7 firms and 10 workers. The material payoff to a firm was given by the function

$$(1) \quad \pi_f = \begin{cases} 10 \cdot e - w - b & \text{if a contract offer was accepted} \\ 0 & \text{if no contract offer was accepted} \end{cases}$$

and the payoff function for a worker was given by

$$(2) \quad \pi_w = \begin{cases} w + b - c(e) & \text{if a contract offer was accepted} \\ 5 & \text{if no contract offer was accepted} \end{cases}$$

⁶This is known as *constructive dismissal*, and is considered illegal in any jurisdiction with employment protection. See Black's Law Dictionary.

Effort	1	2	3	4	5	6	7	8	9	10
Cost	0	1	2	4	6	8	10	12	15	18

TABLE 1. Effort Cost Schedule

where $c(e)$ was a cost of effort function, and 5 was the unemployment benefit in the case that a worker did not engage in a trade. The wage, w , the offered bonus, \tilde{b} , and the bonus actually paid, b , could each take on an integer value 0, 1, 2, ..., 100. The desired effort level, and the actual effort level chosen by the worker could take on integer values 1, 2, ..., 10. The effort cost function is shown in Table 1.

The cost function is increasing and convex. Because the marginal cost of effort is at most 3, while the marginal benefit to a firm is always 10, the efficient effort level is 10.

Payoff functions for workers and firms, including the effort cost function, were common knowledge. Participants were aware that the market would last 18 periods. Reputations could form bi-laterally: firms learned about the effort choices of workers that they traded with, but did not observe the effort choices, or firm bonus decisions, in interactions that they were not a part of; workers learned about the bonus decisions of firms that they encountered, but not about worker effort choices or firm bonus decisions in other market interactions. Firms observed all public offers on the market during the market phase. Workers were informed not only about private offers they had received, but also about all public offers on the market.

The experiment was computerized using Z-Tree software (Fischbacher (2007)). There was a practice period before the experiment began, which consisted only of a market phase, in which subjects had the experience of making and accepting offers. After the practice period, the first period of the paid experiment began. At the end of each period, a subject's period profits were summarized, along with the profits of the trading partner in the case of a trade. Subjects were also reminded of the partner's ID number, the terms of the initial contract, the actual effort choice, and the actual bonus paid. Subjects could record this information on a separate sheet of paper, ensuring that subjects were fully informed about their own trading history over the course of the experiment. The experiment was framed neutrally, in terms of buyers and sellers rather than workers and firms. Effort was referred to as quality. We adopted this framing for purposes of comparison with Brown, Falk, and Fehr (2004). See Fehr, Kelin, and Schmidt (2007), however, for evidence that whether one uses framing as buyers and sellers, or workers and firms, is essentially behavior in our type of experimental market setting.

There were 408 participants in the experiment. We conducted six market sessions for each of the four treatments, for a total of twenty-four sessions. Subjects were students at the University of Bonn, from various fields of study. No subject participated in more than one session. On average, a session lasted roughly 100 minutes, and a subject earned 25 Euros (32 USD).

3. AGGREGATE EFFECT OF EPL

We have a 2x2 design that allows us to explore separately the effect of bonus pay and employment protection on aggregate market performance, and how the two interact. Given that our treatments are

exogenous, we can reasonably assume that the treatments are not correlated with the characteristics of individuals in the market. This allows us to establish a causal relationship between our treatments and market performance, something that is extremely difficult to achieve with field data (see Holland (1986)).

It is worth emphasizing that if the worker and the firm are assumed to maximize their material payoffs then the theory of relational contracts, such as MacLeod and Malcolmson (1989), predicts that no trade would ever occur in our setting with repeated interactions. This follows immediately from the familiar backwards induction argument. In the last period of the game, period 18, the firm would never pay a bonus. This implies that the worker would choose effort equal to zero. This in turn implies that in period 17 there are no gains from continuing the relationship, and hence output would be zero in that period, and so on. This argument applies to all the treatments.⁷ However, as Fehr, Gächter, and Kirchsteiger (1997) have shown, when contracts are incomplete, reciprocity concerns leads to a higher level of trade *in practice*. At the moment, we do not yet have a complete theory of how individuals behave in these complex relationships, hence in order to evaluate the consequence of employment protection one must explore the question empirically.

Figure 1 provides a first indication that EPL can have a strong impact on market performance, but that the impact depends crucially on details of the labor contract. The figure shows efficiency (averaged over sessions for a given treatment) as a fraction of the maximum possible.⁸ For T-Baseline and T-Bonus, efficiency is close to 70 percent, despite the contract enforcement problem in the market. If anything, efficiency is slightly higher in T-Bonus than in T-Baseline, although this is driven mainly by the first few market periods, after which T-Baseline catches up.

Efficiency is clearly substantially lower, however, in T-EPL. Moreover, this difference is growing over time; efficiency diverges even more strongly after the first few market periods. On the other hand, the figure indicates that the negative impact of EPL is substantially reduced by giving firms the additional option to pay bonuses. Efficiency in T-EPL-Bonus is only slightly lower than in T-Baseline and T-Bonus, and well above T-EPL. Notably, efficiency drops sharply for all treatments in the final market period, indicating that the potential for future interactions is important for maintaining cooperation.

[Figure 1 about here]

These results for aggregate efficiency are summarized in regression form in Table 2, along with the impact of the different treatments on firm profits and worker earnings. The left hand side variables, expressed as fractions of the total potential gains from trade, are regressed on treatment dummies with T-Baseline as the omitted category. Motivated by Figure 1, we also include interaction terms between treatment dummies and the final market period, in order to take out the end game effect. We also condition on market period greater than 5, in order to capture the long run effects of the treatments. Coefficients in Column (1) and Columns (3) to (5) are OLS estimates, and coefficients in Column (2) are interval regression estimates that

⁷In the fixed wage treatment there is no bonus pay. In that case, the worker moves last and would rationally choose zero effort.

⁸The efficient effort is 10, producing a gain of 100. Subtracting the effort cost of 18, and 5 for the opportunity cost of a worker, the maximum possible net gain from trade is 77.)

account for censoring at minimum and maximum values (0 and 77, respectively). Standard errors are robust and adjusted to allow for correlation of the error term between observations from the same session.

[Table 2 about here]

Consistent with Figure 1, Column 1 shows that efficiency is comparable (and not statistically different) in T-Baseline and T-Bonus, but is significantly lower in T-EPL. The difference between T-EPL and T-Baseline is large, equal to about 16 percentage points. The coefficient for T-EPL_Bonus is not statistically significant, indicating that EPL does not have a significant impact on efficiency relative to T-Baseline if firms have the option to pay bonuses. Column 2 shows that results are similar if we adjust for censoring, although differences are even larger. We also find similar results using conservative non-parametric tests that treat each session as one independent observation. If we do not exclude the first five market periods the results are qualitatively similar, although because the difference between T-EPL and T-Baseline takes a few periods to become pronounced it is only significant at the 6 percent level. Including the first five periods also causes efficiency in T-Bonus to become significantly higher than in T-Baseline, at the five percent level, due to the higher level in the first few periods before T-Baseline catches up.

Column (3) shows that EPL is bad for firm profits. Firms suffer a loss of 13 percentage points relative to T-Baseline. Adding bonus pay, however, restores firms to the level of profits observed in T-Baseline. In the absence of EPL, bonus pay makes firms even better off, leading to a 10 percentage point improvement above T-Baseline. Notably, the constant in the regression for firm profits shows that the firm receives on average 35 percentage points of the potential gain. Notice that it is statistically the same as the constant for the worker's payoff in Column (4). This is evidence for strong rent-sharing in this market, which suggests the presence of at least some individuals with fairness concerns. Given that the firm has all the bargaining power, a competitive solution would leave the worker with no surplus.

Column (4) shows that if anything, EPL makes workers slightly worse off, although the difference relative to T-Baseline is not statistically significant. This result is consistent with the view that adopting EPL based on concerns for worker well being may be misplaced, due to negative effects of EPL on incentives and efficiency. Workers are also harmed by bonus pay. Whereas bonus pay made firms better off, it leads to a statistically significant drop in worker earnings relative to T-Baseline. It is particularly interesting that bonus pay helps the firm at the expense of the worker, with firms gaining 9 percentage points in T-Bonus relative to T-Baseline and workers losing 7 points. This provides a reason in addition to the ratchet effect (see Gibbons (1987)) that can help explain why unions have traditionally resisted bonus pay systems.

Column (5) shows the impact of the treatments on inequality in worker payoffs, measured as the within-period variance normalized by maximum surplus. The main conclusion is that the treatments with bonus pay exhibit significantly higher inequality. This makes sense given that bonus pay makes the contract more flexible, and allows firms to condition more easily on worker performance. It is also consistent with recent field evidence that links the rise in inequality in the US to the increasing prevalence of performance pay (Lemieux, MacLeod, and Parent (2007)).

An final important point is that these aggregate results reflect substantial underlying heterogeneity. In particular, the impact of the treatments differs across individuals. For example, we estimated a quantile

regression version of Column (2) of Table 2, and observed that EPL makes a larger difference for the top quartile, where the reduction in efficiency is about 20 percentage points, but has a smaller impact on the bottom quartile, where the reduction is only 10 percentage points. This reflects the fact that some individuals perform poorly even in the absence of EPL, and thus there is little scope for EPL to make things worse. We also find that the impact of T-Bonus varies across quartiles: it is larger for the bottom quartile, whereas for the top quartile there is little effect, due to the fact that these individuals perform well even in the absence of bonus incentives. In the next section we explore heterogeneity at the micro level in more detail.

4. THE EFFECT OF EPL ON MICRO LEVEL BEHAVIOR

The rest of the analysis investigates the impact of EPL on micro level behavior, in order to lay bare the mechanisms underlying our aggregate results. We exploit the fact that we can observe in detail all types of individual behavior, with zero measurement error.

4.1. Threat of firing. In this section we briefly explore the role of firing threat in our two treatments without EPL, in order to understand how the limits on firing imposed by EPL affect aggregate outcomes.

In T-Baseline, high effort levels, which are necessary for high efficiency, tend to be observed in long-term relationships where the same firm and worker engage repeatedly in private offer contracts. Long-term relationships are prevalent in the market, with 67 percent of all offers being private offers, and 64 percent of all private offer relationships lasting longer than 5 periods. The correlation between effort level and ultimate relationship length is 0.64 (Spearman; $p < 0.001$). Why is this the case? The first part of the story is that firms pay efficiency wages (rents) in order to make relationships valuable to workers. Wages in long-term relationships are about 57 percent higher than wages in one-shot interactions. The second part of the story is that firms fire workers who do not choose a high effort level. Figure 2 shows that the probability of not rehiring a worker, conditional on being in a long-term relationship in the previous period, is close to 1 if the worker performed poorly in the previous period and close to zero if the worker put in a high effort level. Thus, firms credibly threaten to fire workers who do not perform, and pay a rent so that this threat provides an incentive. The sharp drop in efficiency in the final market period is another indication that threat of firing is important for deterring worker shirking. On average, as we see in Table 2, there is a reduction in efficiency of 27 percentage points in the last period of the T-Baseline treatment.

[Figure 2 about here]

Although it is true that on average worker effort levels drop when the threat of firing is removed, this conceals an important type of heterogeneity in worker motives. In particular, some workers in T-Baseline are willing to put in high effort even in the absence of future rents. 10 percent of workers in long-term relationships put in an effort level of 10 in period 18. A clue as to the source of this behavior comes from the wage-effort relation: worker effort levels increase strongly in response to the wage, even in the final period, and effort levels of 10 are realized in those contracts with relatively high wages. This non-strategic effort provision is consistent with a large body of experimental evidence showing that some individuals in the population have reciprocal inclinations, such that they reward kind actions, or fair wages, with high effort

(for a review see Fehr and Gaechter (2000)). It suggests that some of the effort provision throughout the game may be driven by fairness, rather than strategic considerations, although there is clearly a substantial fraction of workers who are strategic. Reciprocal workers are attractive types for firms, given that they work hard in response to a fair wage, independent of future considerations. Later in the analysis we investigate how EPL affects the selection of selfish versus reciprocal workers into long-term relationships.

Turning to T-Bonus, we observe that the combination of rents and firing threat plays less of a role than in T-Baseline. Total offered compensation, including wages and offered bonuses, are only about 5 percent higher in long-term relationships than in one-shot interactions. Figure 2 shows that the probability of firing responds to worker performance, but is substantially less sensitive than for T-Baseline. Notice that the probability of firing is substantially higher overall than in T-Baseline. Taken together these results indicate that the option to pay bonuses substantially changes the way that firms enforce contracts. Firms appear to make less use of relational incentives in general. Nevertheless, as we saw above, efficiency is at least as high as in T-Baseline, and is actually higher in early market periods before relationships have time to form in T-Baseline and efficiency begins to improve. This latter finding is consistent with Wu and Roe (2007), whose work is contemporaneous with our own, and shows that bonus pay improves efficiency using treatments that are similar to ours. The finding contrasts with MacLeod and Malcolmson (1989) who show that theory predicts there would be no efficiency gain from bonus pay that when firms are on the short side of the market (as in this experiment). On the other hand the finding provides some support for reciprocity theory, as discussed in Fehr, Gächter, and Kirchsteiger (1997), in which bonus pay can be beneficial because it gives firms an instrument for reciprocating high effort levels by workers. We discuss firms' use of bonus pay, and why it is an effective incentive device, in more detail below.

4.2. Worker Effort Choice. Our results on the differing roles of firing threat, with and without bonus pay, suggest that the diverging effects of EPL on aggregate efficiency are likely to work through differential impacts on worker effort levels. In particular, given the importance of firing threat as an incentive device in an environment with efficiency wage contracts, we expect that the negative impact of EPL on efficiency in such a setting works through the channel of more pronounced worker shirking. In a setting with bonus pay, firms do not condition firing so strongly on performance, and the aggregate impact of EPL is much more mild, suggesting that bonus pay must help firms to deter worker shirking even in the absence of the incentives provided by firing threat.

A first piece of evidence supporting these predictions comes from a comparison of average effort levels in long-term relationships (relationships consisting of at least two consecutive private offers) across treatments. Average effort is 8.4 in T-Baseline and 8.78 in T-Bonus, but only 5.5 in T-EPL. On the other hand, average effort in long-term relationships in T-EPL-Bonus is 8.2, essentially the same as in the treatments without EPL. Figure 3 shows that the effort differences in long-term relationships are large and stable across market periods. In one-shot interactions, where EPL has no impact on ability to dismiss a worker, we observe similar effort levels across all four treatments.

[Figure 3 about here]

Figure 4 compares effort levels in the initial, probationary period of relationships to effort in later relationship periods, restricted to individuals who are ultimately rehired after the probationary period. Thus, the figure sheds light on whether the observed drop of average effort levels in long-term relationships in fact reflects a within-individual change, or whether it could reflect a composition effect, namely different types of workers being hired in long-term versus one-shot interactions. The boxes indicate the interquartile ranges of the effort distributions, and the circular marker indicates the median effort level. Median effort levels increase moving from probationary to later periods in T-Baseline and T-Bonus, and effort variances decrease. In T-EPL, by contrast, median effort drops sharply relative to the probation period, and there is a substantial increase in the variance of effort. Interestingly, median effort in the probation period in T-EPL is actually appears somewhat higher than in T-Baseline, a result to which we return below. For T-EPL-Bonus, on the other hand, the pattern is very similar to T-Baseline and T-Bonus, with increasing effort and decreasing variance. Notably, although on the whole individuals exert much lower effort levels after the probation period in T-EPL, there are still some who choose close to maximum effort. This points to the importance of heterogeneity, such that some types are not solely motivated by firing threat and material self-interest.

[Figure 4 about here]

Table 3 provides a regression analysis assessing whether these impacts of EPL on effort levels are statistically significant, and whether they are robust to controlling for potential differences in contract terms across relationship periods or treatments. In Columns (1) and (2) the dependent variable is the worker's effort level in the first private offer contract, and in Columns (3) and (4) it is effort in later relationship periods. Coefficients are marginal effects from interval regressions that correct for the fact that effort is measured in intervals and is thus left and right censored. Columns (5) and (6) present marginal effects from probit regressions where the sample is restricted to second private offer contracts and the dependent variable is equal to 1 if the worker reduced effort relative to the first contract and 0 otherwise.

[Table 3 about here]

The results in Columns (1) and (2) show that effort is in fact significantly higher in T-EPL than in T-Baseline, in the initial private offer contract. We find a similar result if we look instead at effort levels in all one-shot interactions, including public-offer contracts, which could also play some role as a probation period if a firm first tries out workers using public offers. Elevated effort levels in the probation period suggest that workers behave strategically, trying to lure firms into committing to a long-term relationship. As discussed above, workers have a strong material incentive to do so. This result is also consistent with field evidence on probation effects, for example evidence that worker absenteeism increases after the probation period for EPL in Italy (Ichino and Riphon (2005)). Effort in the first relationship period is also significantly higher in T-EPL-Bonus than in T-Baseline, but this is also true for T-Bonus, and thus does not appear to be an effect of EPL. Whereas contract enforcement in T-Baseline appears to involve a buildup of trust and worker

performance, in settings with bonus pay worker effort is high even on first contact. This may help explain why Efficiency in T-Bonus is higher than in T-Baseline in the first few market periods.

Columns (3) and (4) show that effort in long-term relationships is significantly lower in T-EPL than in T-Baseline, whereas effort levels in T-EPL-Bonus and T-Bonus are actually significantly higher than in T-Baseline once differences in contract terms are held constant. Columns (5) and (6) confirm that workers are significantly more likely to reduce effort in the second private offer contract in T-EPL, compared to T-Baseline, consistent with EPL creating a moral hazard problem. In T-EPL-Bonus and T-Bonus, on the other hand, the probability of a drop in effort is not statistically different from T-Baseline.

In summary, introducing EPL into an efficiency wage setting causes workers to shirk once they are past the probation period, consistent with a moral hazard effect of removing the threat of firing, which thereby leads to lower market efficiency. The presence of bonus pay, on the other hand, prevents worker shirking even when EPL is introduced, explaining why EPL has less of an impact on aggregate efficiency in such a setting.

4.3. Adverse Selection. The presence of EPL in the market makes it more difficult for a firm to screen workers, because it is no longer possible to try the same worker multiple periods in a row. As a result of this distortion in the market, one might expect firms to have more trouble screening out types who are selfish in the sense that they care only about their own material payoffs and always shirk given the opportunity. Note that the tendency for selfish workers to exaggerate their willingness to perform in the probation period makes it all the more difficult for firms to separate types in T-EPL. To the extent that more selfish workers select into long-term relationships in T-EPL, this is a type of adverse selection that reduces the efficiency of relationships.

In order to assess the proportion of selfish versus reciprocal types who end up getting through the screening process, and into long-term relationships, we look at worker behavior in the final market period. In period 18, selfish workers have no incentive not to reveal themselves by choosing an effort level of 1. Our findings are suggestive of more selfish types ending up in long-term relationships in T-EPL, compared to T-Baseline. One indication is that in the final market period, 44 percent of workers in long-term relationships choose an effort level of 1 in T-EPL, compared to 28 percent in T-Baseline. Some of the workers choosing minimum effort could, however, be fairness-minded types who are responding to a low or unfair wage offer. In order to control for potential differences in final-period wages across treatments, we also regressed effort levels in the final market period on a treatment dummy and the wage (interval regression). The sample included all private-offer contracts that were part of a long-term relationship going into the final period. We find that final-period effort levels are significantly lower in long-term relationships in T-EPL, compared to in long-term relationships in T-Baseline, controlling for the wage ($p < 0.03$; robust s.e., clustering on session).

Another way to assess the proportion of selfish versus reciprocal types is to look at the sensitivity of effort levels to wages in the final period. The greater the fraction of selfish workers, the less sensitive effort should be to high wage offers. Using all private offer contracts in the final period, which were part of an on-going relationship in the previous period, we regressed effort on wages (interval regression; robust s.e. clustering on session). The impact of wages is positive but not significant in T-EPL ($p < 0.124$), whereas the impact of wages on effort is positive and significant in T-Baseline ($p < 0.001$). It turns out that in T-EPL

and T-Baseline, the number of private-offer contracts in long-term relationships in period 18 is exactly the same, so the difference in significance levels does not simply reflect different number of observations.

Given an equal proportion of reciprocal types across treatments, the previous results would imply a greater proportion of non-selfish workers in one-shot interactions in the final period in T-EPL. In fact, the proportion of workers choosing an effort level of 1 in period 18, in one-shot interactions, is only 0.35 in T-EPL, compared to 64.71 in T-Baseline. Using all one-shot interactions in period 18, and regressing effort on a treatment dummy and wages (interval regression; robust s.e. clustering on session), we find that effort is also significantly higher in T-EPL than in T-Baseline, conditional on wages ($p < 0.015$). Using regressions to assess the sensitivity of effort to wages tells a similar story (interval regression; robust s.e. clustering on session): in one-shot interactions in period 18, wages have a positive and significant impact on effort in T-EPL ($p < 0.001$), whereas in T-Baseline the relationship between effort and wages in period 18 is positive but not significant ($p < 0.297$). Thus, the number of reciprocal types in one-shot interactions in the final period appears to be greater in T-EPL than in T-Baseline, which is again supportive of the presence of a reverse pattern for selfish types.

An interesting possibility is that the presence of more reciprocal types in one-shot interactions could potentially explain part of the better performance of initial private offer contracts in T-EPL, shown in Columns (2) of Table 3. This is because some of these probation period contracts are one-shot interactions that never lead to a long-term relationship. If we do estimate the regression in Column (2) of Table 3, however, eliminating private-offer contracts that do not turn into a long-term relationship, we still observe significantly higher performance in T-EPL, and the effect is even stronger. This shows that the probation period results are not driven by the selection of non-selfish types into one-shot interactions.

Looking at final period effort in T-EPL-Bonus and T-Bonus, for contracts that are part of a long-term relationship, we observe no significant difference compared to T-Baseline, conditional on compensation. It is more difficult to interpret final period behavior of workers in treatments with bonus pay, however, because the firm is the final mover in period 18. This makes it less clear whether a worker putting in an effort greater than 1 is non-selfish behavior.

4.4. Compensation. Introducing bonus pay gives firms an additional contractual instrument. Our results so far suggest that bonus pay must serve as an alternative incentive device to threat of firing, which allows firms to deter worker shirking even in the presence of EPL. For bonus pay to be an effective incentive, the critical point is that bonus pay must be used to credible reward high effort levels. Whether or not the promise to pay a bonus will be perceived as credible by workers is not obvious *a priori*. Figure 5 shows, however, that actual bonus payments are strongly increasing in worker effort levels, implying that bonus promises are credible. We also regressed actual bonus payments in period t on worker effort levels in t (interval regression), and found that the relationship between effort and bonus payments is also highly statistically significant ($p < 0.001$; robust s.e., clustering on session).

[Figure 5 about here]

What can explain the credibility of bonus pay? In the final period, firms have no material incentive to pay a bonus, and by backwards induction workers would never expect firms to pay a bonus in any earlier period. We observe, however, that there is a positive and significant relationship between bonus payments and effort even in the final market period. This indicates the presence of some reciprocal firms, who reward high effort with a high bonus, despite the lack of any reputation or other strategic motives to pay. Intuitively, the presence of these firms helps sustain cooperation, because if workers anticipate some probability of being rewarded for good performance even in the final period, then they have an incentive to in period 18. This creates an incentive for even those firms who care only about their own material payoffs to maintain a reputation for reciprocity until the final period, in order to realize the final period benefits of good worker performance. In our theory section at the end of the paper we make this point formally.

Given the benefits to firms, one would expect to observe firms making more use of bonus pay in the presence of EPL. Table 4 shows that this is the case. The dependent variable in all columns is the offered bonus. In Column (1), the treatment dummy for T-EPL-Bonus is not significant. However, once one controls for differences in wage levels across treatments in Column (2), which are negatively related to offered bonuses, offered bonuses are significantly higher in T-EPL-Bonus. This is robust to controlling for other contract terms in Columns (3) and (4). These results show that EPL affects the type of contracts offered in the market, and that firms endogenously shift to contracts that help minimize the effects of the institution. As we have seen, this response is sufficient to substantially ameliorate the effect of EPL on aggregate efficiency.

[Table 4 about here]

4.5. Relationship Formation and Turnover. Given the ways that EPL affects performance in long-term relationships, we would expect it to have an impact on the willingness of firms to enter long-term relationships in the first place. This has implications for efficiency, and also rates of turnover in the market, an outcome that is frequently studied in connection with EPL.

In T-EPL, average performance in long-term relationships suffers, and thus we would expect firms to be more cautious about entering relationships than in T-Baseline. Consistent with this prediction we find that 58 percent of all contracts were in one-shot interactions in T-EPL, compared to 48 percent in T-Baseline. Even more tellingly, we observe 23 percent of all firms in T-EPL pursuing a strategy of strict relationship-avoidance, never making two private offers in a row to the same worker, during the entire game. This contrasts with only 9 percent of firms exhibiting this strategy in T-Baseline. Put another way, the probability that a firm has never been in a long-term relationship as of market period t is consistently higher in T-EPL than in T-Baseline. We estimate a Cox proportional hazard model, and find that the survival probability of a firm never having been in a long-term relationship, as of market period t , is higher in T-EPL compared to T-Baseline. This difference is only marginally significant overall ($p < 0.07$), mainly because the two hazards are relatively similar in the first few market periods. Considering market periods beyond period 5, however, the difference becomes significant at the five-percent level ($p < 0.043$). The finding that EPL pushes firms into the spot market for labor is consistent with field evidence. Various studies find that strong EPL is associated with greater reliance by firms on temp agency workers ().

Although there are more one-shot relationships in T-EPL, and the fraction of firms who strictly avoid relationships is larger, the impact on turnover is only very mild. This reflects the offsetting impact of EPL on the probability of relationships breaking up, contingent on formation. Whereas long-term relationships frequently break-up in T-Baseline, due to firms firing poor performers, EPL relationships essentially never breakup in T-EPL, despite low effort levels. This is because firms cannot fire workers, and workers essentially never quit EPL relationships, doing so in only 3 out of 283 EPL contracts. This is understandable given that worker earnings are about 63 percent higher on average in EPL relationships than in one-shot interactions, in T-EPL. Thus, as can also be seen from the cumulative distributions of relationship length plotted in Figure 9, EPL changes the market by fostering a more bi-modal distribution of relationship lengths. There are substantially more interactions that last only one period, and then a mass of very long-lived relationships, with few relationships of intermediate lengths. On net, the average length of a relationship is only slightly longer in T-EPL than T-Baseline: 6 periods versus 5.89.

[Figure 6 about here]

In T-EPL-Bonus, bonus pay prevents worker shirking in long-term relationships, and thus we might expect firms to be less cautious about entering long-term relationships. On the other hand, we have seen that bonus pay leads firms to be less reliant on relationship-based incentives, which suggests a factor that could actually lead to fewer long-term relationships and higher turnover, compared to T-EPL or T-Baseline. By this argument, we would expect to see high turnover in T-Bonus as well.

The fraction of firms who never enter a long-term relationship in T-EPL-Bonus is 19 percent, somewhat lower than in T-EPL but substantially above T-Baseline. The survival probability of never having entered a relationship also indicates that firms in T-EPL-Bonus are more prone to avoid relationships than firms in T-Baseline. A Cox mixed proportional hazard model shows that the survival probability is significantly higher than in T-Baseline ($p < 0.026$).⁹ Compared to T-EPL, the survival probability is not significantly different ($p < 0.79$). On the other hand, those firms who do enter long-term relationships appear to engage in more experimentation first, compared to T-EPL or T-Baseline. This is shown by the fact that the frequency of one-shot interactions is 60 percent, higher than in either T-EPL or T-Baseline, and the fraction of firms who do not enter their first long-term relationship until after the ninth period is 35 percent compared to 24 percent in T-Baseline. As a consequence, the average length of a relationship in T-EPL-Bonus ends up being somewhat shorter than in T-Baseline or T-EPL, 4.84 as opposed to 5.99 or 6, despite the fact that workers quitting EPL is just as rare as in T-EPL. As shown in Figure 9, the cumulative distribution function for T-EPL-Bonus is similar to T-Baseline, but shifted upward, indicating fewer long-term relationships than in either of the other two treatments. Turning to T-Bonus, we observe even stronger evidence of experimentation and short-lived relationships. The average length of a relationship in T-Bonus is only 2.2, and Figure 9 shows that the cdf of relationship lengths is higher than in all other treatments.

⁹The difference is also significant considering only market periods greater than 5 ($P < 0.036$).

5. IMPLICATIONS FOR THE THEORY OF INCOMPLETE CONTRACTS

We have shown empirically employment protection does result in a decrease of output in our experimental labor market. However, we also find that contract form has a major impact on overall performance. Performance pay greatly reduces the negative effect of employment protection upon output.

One of the motivations for an experimental analysis of employment protection is that there does not exist an accurate model of human behavior. As we see from the data, there is enormous variation in output, even within the same match. The goal of the theory then is not to provide a tight prediction, but to help us organize the data, and provide some comparative results that can help explain the impact of the treatment. In addition, the data have the distinctive feature, common to all repeated game experiments, that performance falls near the end of the trading relationship.

This complexity in observed behavior may be best explained by the hypothesis that individuals have different ways to think about or consider the strategic elements of the game. Given that there was no pre-play communication, nor coaching regarding how to play the game, individuals would use their personal experiences to guide them. When making a decision a player considers the consequence of her actions for the future, including the likely responses of her partner or partners.

A large volume of research has shown that when making a decision individuals do not act as if they maximize material rewards alone. Their decisions also depend upon other factors, such as equity towards their trading partner, or a taste for *integrity*. By integrity we mean that if a party has agreed to a contract, then she will suffer some disutility if she breaks her agreement. Hart and Holmström (1987) were the first to suggest that such a taste might enhance performance in a repeated contract setting. MacLeod (2007) shows that this is indeed this case, and that the introduction of a small taste for integrity can result in a high level of cooperation in a repeated contract environment. In this experiment there are two potential effects of integrity. The first is whether or not the worker suffers some disutility if she chooses effort less than the agreed upon effort. The second is whether the firm feels disutility if he pays less than the promised bonus when the worker has performed.

It is also the case that there is a great deal of variation in how much weight individuals place upon these non-standard motives. We model this by formally incorporating these other factors into a person's preferences. More precisely, suppose that individual i is going to take decision d_i^t in period t . Let $u_i^m(d_i^t)$ be the material reward from this decision. In addition there are values $v \in V$ denoted by $u_i^v(d_i^t)$ that "color" the individuals decision. For this experiment attention is it sufficient to consider only three values: a taste for equity (E), a taste for honoring an agreement or integrity (I) and finally a concern for ones' reputation and future return from the relationship (R). Accordingly we let $V = \{E, I, R\}$.

The extent to which a value v is considered in a person's preferences is given by $\gamma_i^v(H_t)$, which is a weight that positive, though it may be zero. In the case of a worker's reputation, the weight is written $\gamma_i^R(H_{t+})$, where H_{t+} represents the history including the choice d_i^t . This formula is a bit more convenient, because we can then let $u_i^R(d_i^t)$ represent *potential* reputation, a parameter that will be well defined. Given this the individuals *behavioral* preference at date t is given by:

$$U_i(d_i^t) = u_i^m(d_i^t) \times u_i^E(d_i^t)^{\gamma_i^E(H_t)} \times u_i^I(d_i^t)^{\gamma_i^I(H_t)} \times u_i^R(d_i^t)^{\gamma_i^R(H_{t+})}.$$

If all the weights are zero, then the individual acts to maximize her material payoff in the current period. The form that these value functions take depends upon the context, which we discuss in more detail below. As we shall see, the evidence suggests that there is a great deal of variation in these weights. For some individuals they are small or zero, for others they may be quite large.

If we take logs, then we suppose that the individual chooses d_i^t to maximize:

$$u_i(d_i^t) = \ln U_i(d_i^t) = \ln u_i^m(d_i^t) + \gamma_i^E(H_t) \ln u_i^E(d_i^t) + \gamma_i^I(H_t) \ln u_i^I(d_i^t) + \gamma_i^R(H_{t+}) \ln u_i^R(d_i^t).$$

In the next two sections we discuss specific functional forms for each of these values and the evidence that suggests whether or not the weight associated with the value is positive. What is nice about this formulation is that it incorporates naturally the idea that individual bring to bear a number of different value systems when making a decision. A very egalitarian person might for example set $\gamma_i^E = 1$ and $\gamma_i^I = 0$. In contrast a person who is a stickler for the rules might be better characterized by $\gamma_i^E = 0$ and $\gamma_i^I \gg 0$. The question we address is first whether there is evidence for individuals with certain values, and then whether this can explain our results.

5.1. Last Period Values. Consider first the problem faced by a worker in the last period (period 18) when on a fixed wage. In that case, the worker's decision is the amount of effort e_i . Given that there is no future to the relationship $\gamma_i^R(H_{t+})$ is zero. Consider the following value functions that capture the effects we wish to explore:

$$(3) \quad u_i^m(e_i^t) = (w_i^t - c(e_i^t)) / 5$$

$$(4) \quad u_i^E(e_i^t) = 1 + 10e_i^t - w$$

$$(5) \quad u_i^I(e_i^t) = \begin{cases} 1 + e_i^t - \bar{e}_i^t, & \text{if } \bar{e}_i^t \geq e_i^t. \\ 1 & \text{if not.} \end{cases}$$

The first expression is simply the material payoff to the worker normalized by the worker's outside option of 5. The second expression set is the material payoff to the firm, normalized to be 1 if his payoff is zero. An equity minded individuals puts some weight on the other person's material payoff, which in case is the firm. Finally, $u_i^I(e_i^t)$ represents a taste for integrity. The contract stipulates that the fixed wage treatment, and hence the individual gets disutility from choosing effort less than the agree upon effort level.

Previous work with the dictator game has shown that individuals take in to account the preferences of the other person (see Camerer (2003)). Theoretical work, such as Rabin (1993), models this as a game in which a person rewards the other for being nice or fair, which in turn depends upon how nice a person is to you. Fehr and Schmidt (1999) show that one does not need to capture intentionality, but may capture these effects in a model in which individuals are assumed to be inequity averse.

Empirically, it is difficult to distinguish between these different models. Here, we are more agnostic, and simply capture a feature that is common to all fairness models, namely that one places some weight upon the other person's preferences. If any of these theories are correct, then this implies that some individuals will have $\gamma_i^E > 0$. Similarly, if the worker has a taste for integrity, then this corresponds to $\gamma_i^I > 0$. If a

person cares only upon their material payoff then this has some strong, testable implications for the last period of play. These are summarized in the following proposition.

Proposition 5.1. *Suppose that the worker's values satisfy:*

- (1) $\gamma_i^E(H_t) = \gamma_i^H(H_t) = \gamma_i^R(H_t) = 0$ then her optimal choice is $e_i^t = 0$.
- (2) $\gamma_i^E(H_t)$ is sufficiently large and $\gamma_i^H(H_t) = \gamma_i^R(H_t) = 0$ then her optimal choice is $e_i^t > 0$ and this is increasing with w_i^t and $\gamma_i^E(H_t)$. In this case effort is independent of desired effort \bar{e}_i^t .
- (3) $\gamma_i^E(H_t), \gamma_i^H(H_t)$ are sufficiently large and $\gamma_i^R(H_t) = 0$ then her optimal effort is positive and satisfies $e_i^t \leq \bar{e}_i^t$. If γ_i^E is sufficiently large, then we have $e_i^t = \bar{e}_i^t$ and there should be some pooling at this effort level. Moreover effort is increasing with wage w_i^t and desired effort \bar{e}_i^t .

The first result states that if the worker cares only about her current material reward, then she choose effort equal to zero in the last period. Our results above show while many workers make this decision, there are also many that choose positive levels of effort in the final period, and hence the hypothesis that all workers maximize their material reward is certainly false.

In the second case, if a worker has some taste for equity (or distaste for inequity) then we should observe effort increasing with the wage. Our results are consistent with this hypothesis. If workers have preferences only over their own and the firm's payoff, then the desired effort level should have no effect. Yet, as we found, most workers view contract terms as a constraint - that is one should not choose effort *more* than the desired effort.

5.1.1. *Bonus Pay.* Consider now how bonus pay can affect the worker's choice. It is a potential reward or rent that the worker can earn in the future that is affected by her effort today. Given that the firm has stated what is the bonus he is willing to pay in return for good performance, then the reputational rent for the worker can be defined by:

$$u_i^R = 1 + \bar{b}_t.$$

This defines the maximum that the worker might expect from good performance. In the no bonus treatment $\bar{b}_t = 0$. Given that the contract is not enforceable, then this is simply a potential gain to which the worker assigns a weight $\gamma_i^R(H_{t+})$ that can vary both with the worker's effort and the worker's beliefs regarding the integrity of the firm. We cannot directly observe this weighting function, rather we ask whether or not it is positive, and whether it varies with work effort. If it does, then an increase in \bar{b}_t will lead to an increase in effort. If it is zero, or does not vary with the promised bonus, then we should see not effect. What we find is that the promise of a bonus seems to increase effort, however the size of the bonus seems to have no effect.

5.2. **Reputation.** Coming

6. CONCLUSION

To be added

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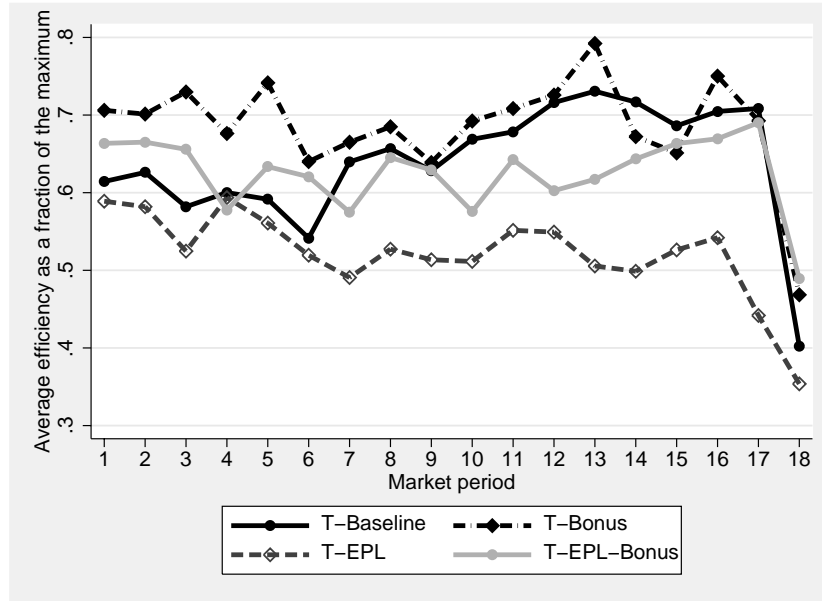


FIGURE 1.

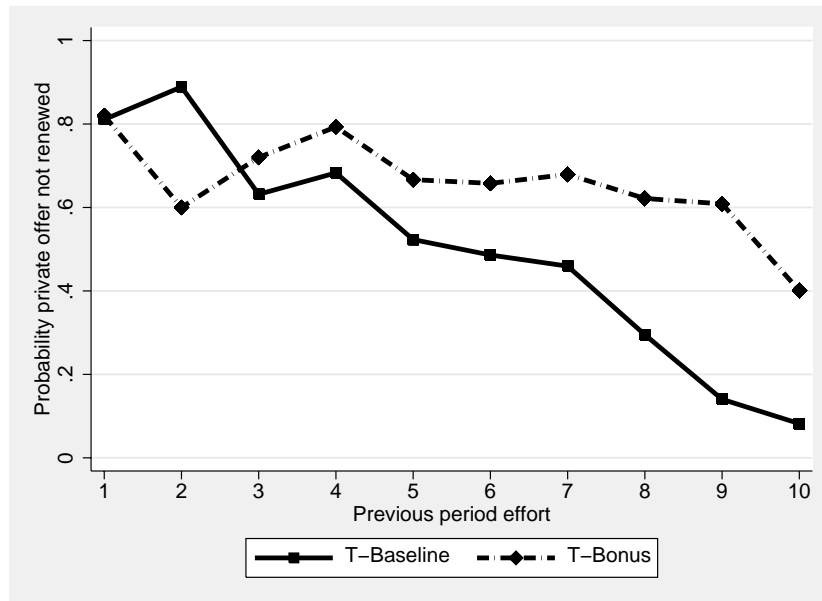


FIGURE 2.

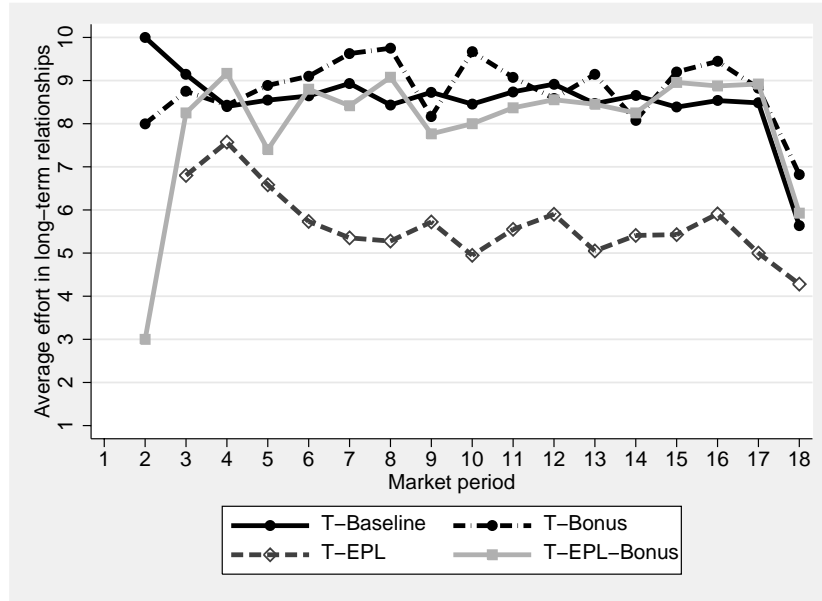


FIGURE 3.

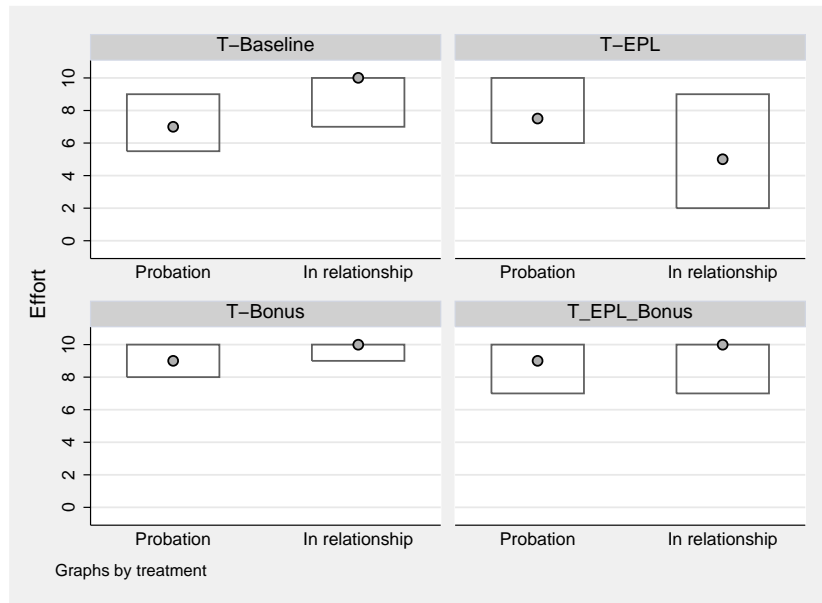


FIGURE 4.

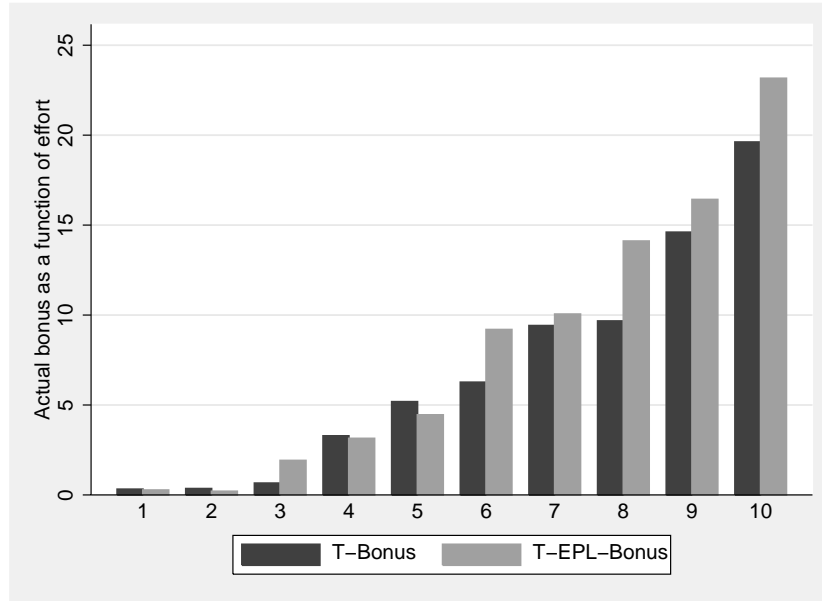


FIGURE 5.

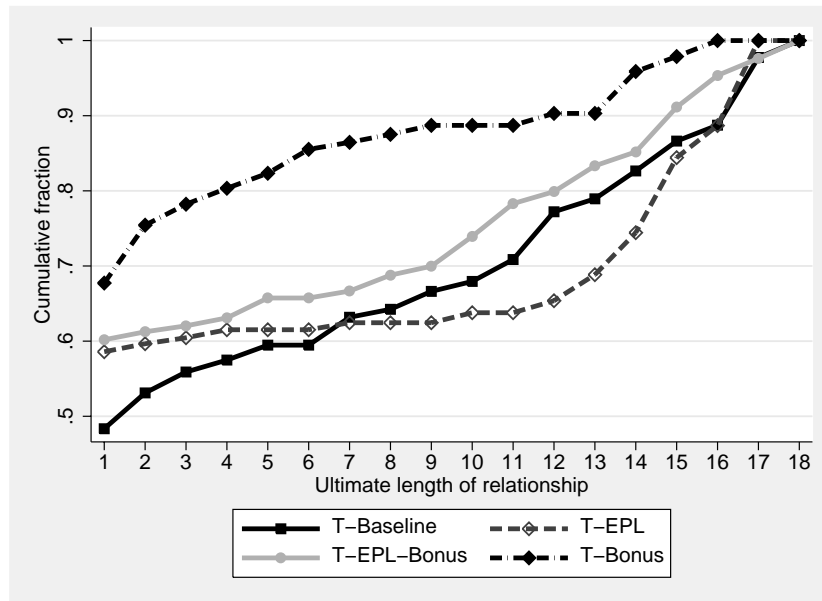


FIGURE 6.

Table 2

	Efficiency				
	OLS (1)	Intreg (2)	Firm profits (3)	Worker payoff (4)	Inequality (5)
T_Bonus	0.021 [0.046]	0.041 [0.097]	0.094*** [0.027]	-0.072* [0.036]	0.770* [0.394]
T_EPL	-0.156** [0.066]	-0.272** [0.123]	-0.132** [0.053]	-0.024 [0.038]	0.543 [0.416]
T_EPL_Bonus	-0.043 [0.052]	-0.07 [0.107]	-0.014 [0.038]	-0.029 [0.033]	1.128** [0.409]
T_Baseline*final period	-0.272*** [0.065]	-0.509*** [0.152]	-0.278*** [0.066]	0.006 [0.012]	1.549*** [0.539]
T_Bonus*final period	-0.227*** [0.078]	-0.427*** [0.164]	-0.200*** [0.068]	-0.027 [0.021]	1.531** [0.605]
T_EPL*final period	-0.164*** [0.031]	-0.290*** [0.052]	-0.225*** [0.047]	0.061** [0.024]	1.077* [0.548]
T_EPL_Bonus*final period	-0.142*** [0.027]	-0.266*** [0.059]	-0.100*** [0.030]	-0.042** [0.019]	1.331 [0.962]
Constant	0.674*** [0.044]	0.764*** [0.096]	0.346*** [0.022]	0.328*** [0.031]	1.594*** [0.195]
Observations	2177	2177	2177	2177	312
R-squared	0.06	n.a.	0.09	0.03	0.15

Table 3

	Effort during probation		Effort after probation		Effort drop	
	(1)	(2)	(3)	(4)	(5)	(6)
T_EPL	2.081*** [0.490]	1.982*** [0.392]	-4.956*** [1.210]	-3.048*** [0.706]	0.307** [0.124]	0.291** [0.103]
T_EPL_Bonus	1.700*** [0.518]	2.270*** [0.521]	-0.169 [1.056]	1.628** [0.751]	-0.023 [0.116]	-0.128 [0.107]
T_Bonus	2.353*** [0.486]	2.385*** [0.450]	1.217 [0.896]	2.153*** [0.573]	0.057 [0.104]	-0.041 [0.131]
Wage		0.087*** [0.017]		0.110*** [0.024]		-0.019* [0.008]
Desired effort		0.509*** [0.137]		0.915*** [0.182]		-0.039 [0.036]
period		-0.097*** [0.027]		-0.131*** [0.034]		0.002 [0.006]
Wage during probation						0.016* [0.007]
Desired effort during probation						-0.004 [0.027]
Effort during probation						0.069* [0.035]
Constant	4.060*** [0.381]	-1.852** [0.859]	9.754*** [0.907]	-2.379 [1.474]	n.a.	n.a.
Observations	912	912	1073	1073	173	173
Log-pseudo likelihood	-2038	-1901	-1849	-1656	-100	-89
Pseudo R-squared	n.a.	n.a.	n.a.	n.a.	0.04	0.15

Table 4

	Offered bonus after probation		
	(1)	(2)	(3)
T_EPL_Bonus	5.340*	6.529**	6.508**
	[2.953]	[2.910]	[2.930]
Wage	-0.680***	-0.708***	-0.707***
	[0.081]	[0.072]	[0.073]
desiredeffort		2.941***	2.910***
		[0.571]	[0.618]
period			0.05
			[0.132]
Constant	42.460***	15.649***	15.324***
	[2.770]	[5.272]	[5.179]
Observations	455	455	455
Log-pseudo likelihood	-1512	-1492	-1492
