

# COOPERATION IN THE WORKPLACE: EVIDENCE FROM THE FIELD\*

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## Abstract

This paper uses personnel data on workers' productivity to identify the determinants of and motives for cooperative behavior in the workplace. The rationale for cooperation arises because workers are paid according to a relative incentive scheme under which individual effort imposes a negative externality on the pay of co-workers. We find that workers cooperate more when they work in smaller groups, with co-workers of the same nationality, and with co-workers of similar ability. We also show the ability of workers to monitor each others' performance is a key determinant of cooperative behavior. Finally, we find that none of these factors affect the performance of the same workers under piece rates, when there are no incentives to cooperate. Further analysis reveals that collusion, rather than pure altruism, drives cooperative behavior under relative incentives. The findings have implications for the design of incentive schemes and the organization of the workplace.

**Keywords:** altruism, collusion, cooperation.

**JEL Classification:** J33, M52.

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# 1 Introduction

The ability to cooperate, namely to abstain from individually profitable actions for the sake of the common good, is often a key determinant of economic performance. In settings where individual and social optima do not coincide – such as in the provision of public goods and management of common resources – the ability to cooperate determines how far outcomes diverge from the group optimum.

A burgeoning body of evidence from experiments and from field studies of common property resources indicates that, in spite of the individual profitability of opportunistic behavior, individuals are sometimes able to cooperate. However, evidence on the determinants of, and the motives behind, cooperation remains scarce.<sup>1</sup>

In this paper we use personnel data to analyze cooperative behavior in the workplace. We present evidence on the characteristics of the group of co-workers that determine the extent to which they cooperate, and on the underlying motive that drives cooperative behavior. In particular, we seek to establish whether workers cooperate because they are altruistic towards their colleagues or because they are able to design and enforce implicit collusive agreements. Distinguishing between these hypotheses is important because the implications for the design of incentive schemes and the organization of the workplace differ in each case.

The personnel data records the daily productivity of farm workers, whose main task is to pick fruit. Workers are paid according to a relative incentive scheme, namely individual pay depends on the ratio of individual productivity to the average productivity of the group of co-workers. Individual effort thus imposes a negative externality on all co-workers by raising average productivity and lowering co-workers' pay, other things equal. This relative compensation scheme therefore creates incentives for cooperation. The welfare of the group is maximized when workers fully internalize the negative externality their effort places on others and exert the minimum feasible level of effort.

We also observe the *same* workers working under a piece rate compensation scheme where individual pay depends only on the worker's own productivity. Under piece rates, there are no incentives for workers to choose their effort cooperatively.<sup>2</sup>

In earlier work using the same data (Bandiera *et al* (2004)), we find that the productivity of the average worker increases by 50% when the relative incentives scheme is replaced by piece

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<sup>1</sup>Ledyard (1995) and Fehr and Fischbacher (2002) review the experimental evidence from public goods games, gift exchange games, dictator games, and ultimatum games. This consistently suggests individuals do not always behave opportunistically in the laboratory. Outside of the laboratory, similar evidence is found in field studies on the management of common resources (Ostrom (1990)).

<sup>2</sup>Workers would have incentives to choose efforts cooperatively under piece rates if they have ratchet concerns. We later discuss in more detail why this is unlikely to be the case in this particular setting, and present empirical evidence to support this assertion.

rates. Calibration of the first order condition of the workers' effort choice problem shows that the observed change in productivity is too large to be consistent with workers choosing their efforts to maximize individual welfare while ignoring the externality their effort imposes on others. At the same time, the change in productivity is too small to be consistent with workers choosing their effort to maximize the group welfare. These earlier results indicate that under relative incentives workers are able to cooperate to *some* extent.

In this paper we identify the group characteristics that drive cooperation, and the underlying motive for cooperative behavior. The data has three key features that allows us to identify the determinants of cooperative behavior.

First, the *same* worker is assigned to different groups of co-workers each day. We thus identify the effect of group characteristics using variation in the productivity of the *same* worker when working in different groups, while controlling for unobserved time invariant sources of individual heterogeneity that drive worker productivity. Key to the identification strategy is that workers themselves do not choose who to work with. In particular, the allocation of workers to groups is, as documented in more detail later, exogenous to these group characteristics that affect their ability to cooperate.

Second, there is daily variation in group characteristics along dimensions that may affect cooperation, such as group size, the national composition of the group, the distribution of ability across the group, and the personal stakes individuals have to behave cooperatively.<sup>3</sup>

Third, we observe the productivity of the same individual each day they work under relative incentives *and* under piece rates. Since there are no incentives for workers to cooperate under piece rates, analyzing the effect of group characteristics on productivity under piece rates allows us to separate their effect on cooperation from their effect on productivity *per se*.

Our first set of results relating to the determinants of cooperation are as follows. We find that a given worker cooperates more – namely her productivity is significantly lower under relative incentives – when she works in smaller groups, when the share of workers of her same nationality is higher, when the group is more homogeneous in terms of their ability and when there is a greater share of co-workers that have exogenously higher stakes to cooperate.

None of these group characteristics affect productivity under piece rates, when there is no incentive to choose effort levels cooperatively. This lends support to the hypothesis that these group characteristics affect productivity only through their effect on cooperation.

Our second set of results shed light on the underlying motive for cooperative behavior. We contrast two hypotheses – that cooperation stems from pure altruism, and that cooperation results from an implicit collusive agreement enforced by transfers or punishment threats. To

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<sup>3</sup>Olson's (1971) classic study highlighted the importance of the size and heterogeneity of groups in determining whether individuals in the group are able to cooperate or not.

distinguish between the hypotheses, we note they have different predictions on three determinants of cooperation – the ability of workers to monitor the performance of co-workers, the ability of workers to communicate with each other, and the social ties between workers.

We first pursue the intuition that while workers must be able to monitor each other’s behavior on the field to sustain a collusive agreement, the ability to monitor co-workers is irrelevant if workers’ behavior is driven by altruism. To investigate the effect of monitoring on cooperation we exploit variation in the physical characteristics of the fruit type that workers pick and analyze workers’ behavior when they pick a fruit with physical characteristics that makes monitoring the performance of others impossible. We find no evidence of cooperation under the relative incentive scheme when workers cannot monitor one another.

Second, we find that workers cooperate more when a larger share of their co-workers, both in their own national group and of other nationalities, speak a common second language – Russian. More importantly, these effects are qualitatively and quantitatively the same for those that *cannot* themselves speak Russian, as for those that can. This suggests the effect on cooperation of being able to communicate to co-workers via a third party is as strong as direct communication. While communication might proxy for social ties and hence the degree of altruism, the fact that both direct and indirect communication are equally important in fostering cooperation is more in line with collusion. The result can be reconciled with altruism only if workers are more altruistic towards co-workers who speak Russian, even when they themselves cannot.

Third, we proxy for the strength of social ties between workers by the number of days in common they have spent on the farm. We find the extent to which individuals cooperate when they first start picking, does *not* depend on the social ties they have developed towards co-workers. In particular we find that when workers first arrive on the farm and begin picking, they do not behave differently to those that have been working longer on the farm. This is in contrast to the intuition of the altruism hypothesis, that when workers first arrive, because they have weaker ties with co-workers, they should initially be less altruistic towards co-workers and hence cooperate less.

Taken together, the balance of evidence in this particular setting is against the hypothesis that cooperative behavior stems from altruism, and is more in line with collusion among workers.

This paper contributes to two strands of the literature. First we provide new evidence on the interplay between social effects and incentives within firms. Our results emphasize the need to jointly consider the design of incentive schemes and the organization of the workplace. The results also complement earlier evidence that workers do not free-ride within firms and case studies from the organizational behavior literature.<sup>4</sup>

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<sup>4</sup>Evidence of the ‘ $\frac{1}{N}$  problem’, whereby individuals appear to overestimate the impact of their actions on their pay has also been found in the literature on team incentives (Hamilton *et al* (2003)), employee stock option plans

Second, by shedding light on individuals’ underlying motives for cooperative behavior, we build on a burgeoning literature in experimental economics. Unlike much of the experimental evidence however, we observe the same individuals in two different strategic environments, there is daily variation in the composition of the group of co-workers that the game is played with, the stakes involved correspond to large monetary amounts, and individuals are not limited in how they may deviate from self-interested behavior (Charness and Rabin (2002)).<sup>5</sup>

The paper is organized into six sections. Section 2 describes the relative incentive and piece rate schemes adopted on the farm and develops a stylized model of effort choices under each scheme. Section 3 describes the data. Section 4 explores the group level determinants of cooperation. Section 5 presents evidence to distinguish between whether altruism or collusion is the underlying motive for cooperative behavior. Section 6 concludes.

## 2 Incentive Schemes and Cooperation

We analyze workers’ productivity and cooperative behavior in a large UK farm for the 2002 harvest season. Workers’ main task is to pick fruit and they are paid according to a relative incentive scheme in the first half of the season and according to piece rates in the second half. This section describes how the two incentives schemes work in practice and makes precise the rationale for cooperation under relative incentives.

Under both incentive schemes, workers face a compensation schedule of the form;

$$\text{compensation} = \beta K_i \tag{1}$$

where  $\beta$  is the picking rate and  $K_i$  is the total kilograms of fruit picked by worker  $i$  on the field in the day.<sup>6</sup> Under relative incentives, the picking rate  $\beta$  is *endogenously* determined by the average productivity of all workers in the same field on the same day, where we define productivity  $y$  as the number of kilograms of fruit picked per hour. In particular  $\beta$  is set equal to;

$$\beta = \frac{\bar{w}}{\bar{y}} \tag{2}$$

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(Jones and Kato (1995)), and firm wide performance bonuses (Knez and Simester (2001)). The organizational behavior literature, reviewed in Rotemberg (2002), has since the ‘Hawthorne Experiments’ of the 1930s, recognized the interplay between altruism, reciprocity, and individual responses to incentives.

<sup>5</sup>Our sample also includes individuals with a broad range of academic backgrounds - 63% study agriculture, 24% social sciences, 11% natural sciences, and 2% arts.

<sup>6</sup>To comply with minimum wage laws, workers’ compensation is supplemented whenever  $\beta K_i$  falls below the pro-rata minimum wage. In practice the farm management makes clear that any worker who needs to have their compensation increased to the minimum wage level repeatedly would be fired. Indeed, we observe less than 1% of all worker-field-day observations involving pay increases to meet the minimum wage requirements. Of these, 46% occurred under relative incentives, 54% occurred under piece rates.

where  $\bar{w}$  is the minimum wage plus a positive constant fixed by the management at the beginning of the season, and  $\bar{y}$  is the average hourly productivity of all workers in the same field on the same day. At the start of each field-day, the field supervisor announces an *ex ante* picking rate based on her expectations of worker productivity. This picking rate is revised at the end of each field-day to ensure a worker with productivity  $\bar{y}$  earns the pre-established hourly wage,  $\bar{w}$ .

Under piece rates, the picking rate is set *ex ante*, based on the supervisor's expectation of productivity that field-day, and is not revised.

The key difference between the two schemes is that under relative incentives an increase in worker  $i$ 's effort increases the average productivity on the field-day and thus imposes a negative externality on her co-workers by reducing the picking rate  $\beta$  in (2). The relative incentive scheme creates a wedge between individual and group optima, thus providing a rationale for cooperation. The workers' incentives to cooperate under relative incentives are formalized in the following stylized model.

Consider a group of  $N$  workers. Each worker  $i$  exerts  $e_i \geq 0$  units of effort which determines her productivity,  $y_i$ . We make the simplifying assumption that effort equals productivity. Each worker's payoff is  $\phi(\cdot) - \frac{\theta_i e_i^2}{2}$ , where  $\phi(\cdot)$  is the benefit derived from pay (which depends on effort), and  $\frac{\theta_i e_i^2}{2}$  is the cost of effort. We assume  $\phi(\cdot)$  is a differentiable concave function, with  $\lim_{x \rightarrow 0} \phi'(x) = \infty$ . The parameter  $\theta_i$  is interpreted as the inverse of the workers innate ability. We assume workers are heterogeneous in ability, and can be ordered such that  $\theta_1 < \theta_2 < \dots < \theta_N$ , where  $\theta_i > 0$  for all  $i$ .

Under relative incentives, each worker's pay depends on how she performs relative to her peers. More specifically, workers' benefit from pay takes the form  $\phi\left(\frac{e_i}{\bar{e}}\right)$  for all  $i$ , where  $\bar{e} = \frac{1}{N} \sum_i e_i$  is the average effort of all  $N$  workers. The relative scheme has the key characteristics that other things equal, an increase in worker  $i$ 's effort – (i) increases her pay; (ii) increases average effort and hence reduces the pay of everybody else. Hence each worker's effort imposes a negative externality on co-workers.

If workers are non-cooperative and ignore this externality, the payoff maximizing effort level solves the following first order condition;

$$\phi'\left(\frac{e_i}{\bar{e}}\right) \frac{1}{\bar{e}} \left(\frac{\sum_{j \neq i} e_j}{\sum_i e_i}\right) = \theta_i e_i \quad (3)$$

If on the other hand workers choose their efforts cooperatively to maximize the sum of workers' payoffs, they fully internalize the externality their effort imposes on others. The effort level then

solves the following first order condition;

$$\frac{1}{\bar{e}} \left[ \phi' \left( \frac{e_i}{\bar{e}} \right) \left( \frac{\sum_{j \neq i} e_j}{\sum_i e_i} \right) - \sum_{j \neq i} \phi' \left( \frac{e_j}{\bar{e}} \right) \frac{e_j}{(\sum_i e_i)} \right] = \theta_i e_i \quad (4)$$

The unique fully cooperative outcome is for all workers to exert the minimum feasible effort.<sup>7</sup> In this context then, cooperation leads to *lower* productivity.

Under piece rates individual effort is paid at a fixed rate  $\beta$  per unit. The optimal effort choice solves the following first order condition;

$$\phi'(\beta e_i) \beta = \theta_i e_i \quad (5)$$

As any given worker's effort does not determine the pay of others, there is no reason for workers to choose their effort levels cooperatively under piece rates.

This stylized static model does not, by definition, account for intertemporal effort choices. This has practical relevance if workers have ratchet concerns, namely if they believe their current behavior affects future piece rates. If so, there may be gains to them choosing their efforts cooperatively under piece rates as well. Three features of this work environment, however, make cooperation under piece rates unlikely. First, this requires workers across different fields to cooperate with each other even though they cannot observe each others' behavior. As shown later, the observability of co-workers' performance is however key for cooperation. Second, workers face uncertainty over which fields they will be assigned to in the future – the probability

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<sup>7</sup>Denote by  $e_{\min}$  the minimum feasible level of effort below which (i.e. if  $e_i < e_{\min}$ ) the worker is fired. The Lagrangian for the cooperative effort choice is;

$$L = \sum_{i=1}^N \left( \phi \left( \frac{e_i}{\bar{e}} \right) - \frac{\theta_i e_i^2}{2} \right) + \sum_{i=1}^N \lambda_i (e_i - e_{\min})$$

The first order condition with respect to  $e_i$  is;

$$\frac{\partial L}{\partial e_i} = \phi' \left( \frac{e_i}{\bar{e}} \right) \frac{1}{\bar{e}} - \phi' \left( \frac{e_i}{\bar{e}} \right) \frac{e_i}{N\bar{e}^2} - \sum_{j \neq i} \phi' \left( \frac{e_j}{\bar{e}} \right) \frac{e_j}{N\bar{e}^2} - \theta_i e_i + \lambda_i = \frac{1}{N\bar{e}^2} \left( \sum_{j \neq i} \left( \phi' \left( \frac{e_i}{\bar{e}} \right) - \phi' \left( \frac{e_j}{\bar{e}} \right) \right) e_j \right) - \theta_i e_i + \lambda_i = 0$$

Each first order condition is satisfied if  $e_i = e_{\min}$  for all  $i$ . To show this equilibrium is unique, assume there exists  $e_1 > e_2 \dots \geq e_N \geq e_{\min}$  such that this vector of efforts is also an equilibrium. Then the first order condition for worker 1 is;

$$\frac{\partial L}{\partial e_1} = \frac{1}{N\bar{e}^2} \left( \sum_{j \neq 1} \left( \phi' \left( \frac{e_1}{\bar{e}} \right) - \phi' \left( \frac{e_j}{\bar{e}} \right) \right) e_j \right) - \theta_1 e_1 = 0$$

where  $\lambda_1 = 0$  as the minimum effort constraint does not bind for this worker. Note that each term in the summation is negative as  $\phi''(\cdot) < 0$ . The first order condition is then only satisfied if  $\theta_1 < 0$  which is a contradiction.

a worker works on the same field on two consecutive days is .25. Third, workers are uncertain about the identity of their future co-workers. In contrast, under relative incentives workers can observe the behavior and identity of the co-workers they have incentives to cooperate with, namely those on the same field and day.

### 3 Context and Data Description

We combine the farm’s personnel data with information from questionnaires we administered to each worker. The workers’ main task is to pick fruit and the personnel data contains daily information on workers’ productivity, defined as kilos picked per hour. Workers’ productivity is recorded electronically. A unique bar code is assigned to each worker, which allows us to track the quantity of fruit they pick each day they work with little or no measurement error. The questionnaires contain information on each workers’ background characteristics.

#### **Farm Organization and Workers’ Characteristics**

Picking takes place across a number of fields in the farm. We analyze productivity for one type of fruit at the peak of the 2002 harvest season, from mid-May until the end of August. We restrict the sample to only include workers that pick for at least ten field-days under both relative incentives and piece rates. Our main sample then contains 10215 worker-field-day level observations, covering 142 workers, 22 fields and 108 days in total.

The incentive scheme changed halfway through the season for all workers at the same time. Relative incentives are in place for the first 54 days in the sample, piece rates are in place for the remaining 54. The change was first announced to workers on the first day it was implemented. Workers therefore have no opportunities to behave strategically to be allocated to a given scheme. No other organizational changes took place during the season, as documented in Bandiera *et al* (2004).<sup>8</sup>

Under both schemes, on each field-day, workers are assigned to their own row of fruit and pick independently from others. Namely, each worker’s productivity depends on her effort and field conditions only. There are no complementarities in production.

Workers are hired on a casual basis, so work is offered on a daily basis with no guarantee of further employment.<sup>9</sup> Most workers in our sample are university students from eight different countries in Eastern and Central Europe. As a consequence, workers picking together on a given

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<sup>8</sup>Interviews with management revealed that the relative incentive scheme was adopted because it allowed to difference out common productivity shocks, such as those deriving from weather and field conditions, that are a key determinant of productivity in this setting. The management decided to change to piece rates because productivity had been lower than they initially expected at the start of the season.

<sup>9</sup>In order to qualify, individuals must be full-time university students aged between 19 and 25. Workers must return to the same university in the autumn and have at least one year before graduation.

field-day are generally from different countries and this ethnic composition changes each field-day. The probability that on a given field-day two randomly chosen workers are of different nationalities is .67, with a standard deviation of .14.<sup>10</sup>

The real value of earnings for workers on the farm is high. Gross monthly earnings at the UK minimum wage (€1105) are 5 times as high as at the minimum wage in Poland (€201) and almost 20 times higher than in Bulgaria (€56).<sup>11</sup> Not surprisingly, three quarters of workers in our sample report coming to the farm for financial reasons. In addition, 15% of workers come to the farm on an internship programme as part of their university course.

Workers are issued with a farm-specific work permit for a maximum of six months, so they cannot be legally employed elsewhere in the UK. Their outside option is therefore to return to their home country.

Workers live and work on the farm together. They are housed in caravans, each hosting between 4 and 6 workers. The organization of the workplace thus provides opportunities for workers to build social ties with others. It also gives workers a variety of mechanisms by which to make transfers or hand out punishments to others.

### **Incentive Schemes, Productivity and Cooperation**

As reported in table 1, workers' productivity rose significantly from an average of 5.01kg/hr under relative incentives to 7.98kg/hr under piece rates, an unconditional increase of 59%. Figure 1 shows the mean of worker productivity over time in the two fields that were operated for the most days under each incentive scheme. Together these fields contribute one third of the total worker-field-day observations. Under relative incentives, there is no discernible trend in productivity. With the introduction of piece rates, productivity rose and remained at a higher level until the end of the season.<sup>12</sup>

Figure 2 shows kernel density estimates of worker productivity by each incentive scheme. The productivity of each of the 142 workers in the sample is averaged within each incentive scheme in this figure. The mean and variance of productivity both rise significantly moving from relative incentives to piece rates. The figure highlights the considerable heterogeneity in productivity across workers within an incentive scheme.<sup>13</sup>

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<sup>10</sup>This measure of ethnic fragmentation is equal to one minus the sum of squares of the share of workers from each nationality. Workers originate from Poland (46%), Bulgaria (13%), Russia (10%), Belarus (10%), Ukraine (6%), Slovakia (6%), the Baltic Republics (4%), Romania (3%) and China (2%).

<sup>11</sup>This monthly minimum wage data is from Eurostat, January 2003. Adjusting for PPP, the differences are somewhat reduced, the corresponding figures in Euros for the UK, Poland and Bulgaria are 983, 351 and 139 respectively. Nominal comparisons are more appropriate because workers save most of their earnings to bring back home.

<sup>12</sup>The increase in productivity was entirely due to workers picking more fruit over the same time period, rather than working shorter hours. On average, workers picked 23.2 more kilograms per day under piece rates - a significant difference at the 1% level. Hours worked did not significantly change across incentive schemes.

<sup>13</sup>This is confirmed in the regression analysis in Bandiera *et al* (2004), that focuses on the effect of the change

In Bandiera *et al* (2004) we present evidence that this is a causal relationship – moving from relative incentives to piece rates caused an increase in individual worker level productivity of at least 50%, other things equal. We show this baseline result to be robust to a host of factors including natural time trends in productivity at the field or farm level, the endogenous allocation of workers to picking tasks over time, the endogenous attrition of low yield fields, the anticipation of the change in incentives by workers, and the potential endogenous timing of the change in incentives by management.

Calibration of the workers’ first order conditions for effort, reported in the appendix, reveals that the observed change in productivity is too large to be reconciled with the assumption of non-cooperative behavior but too small to be reconciled with the assumption of *fully* cooperative behavior. This suggests workers are able to cooperate to some extent. Moreover, the extent of cooperation varies across and within a given worker through time, suggesting that the difference is not solely due to workers’ time invariant characteristics.

The remainder of the paper provides evidence on the group characteristics that determine the extent of cooperation, and what are the underlying motives for cooperative behavior.

## 4 Group Determinants of Cooperation

We identify group characteristics that – (i) lead to more cooperation, and hence lower productivity, under relative incentives; and (ii) have no affect on productivity under piece rates, where there are no incentives to cooperate. The latter allows us to establish that group characteristics affect productivity exclusively through their effect on cooperative behavior.

We consider four group characteristics along which we observe variation each field–day. These are group size, and heterogeneity along three dimensions – nationality, ability, and stakes to cooperate.

To measure ability, we calibrate the first order condition for workers’ effort under piece rates (5) and retrieve an estimate of each worker’s cost of effort,  $\hat{\theta}_i$ , following the methodology described in the appendix. To proxy for the difference in stakes, we exploit the fact that some workers come on an internship programme that gives them credit in their home universities. Interns have higher stakes to cooperate in the sense that the cost of being sent home due to their low productivity is higher because academic punishment is added to the financial loss.

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in incentive schemes on individual productivity. We find that 90% of workers have an increase in productivity when moved from relative incentives to piece rates (conditional on worker and field fixed effects, worker’s picking experience, a measure of the field’s life cycle, and a linear time trend). These responses range from 2% to 98%. The mean response is 54%, the median response is 56%, and the standard deviation in responses is 29%. This heterogeneity may reflect differences in the characteristics of co-workers that each individual works alongside. This is investigated in section 4.

Denoting the productivity of worker  $i$  on field  $f$  on day  $t$ ,  $y_{ift}$ , we estimate the following panel data regression under each incentive scheme, where all continuous variables are in logarithms;

$$y_{ift} = \alpha_i + \varphi_f + \gamma G_{ft} + \delta X_{ift} + \eta Z_{ft} + u_{ift} \quad (6)$$

Worker fixed effects,  $\alpha_i$ , capture time invariant worker level determinants of productivity such as innate ability, the value of their outside option, and intrinsic motivation. Field fixed effects,  $\varphi_f$ , capture time invariant field level determinants of productivity such as soil quality or plant spacing. We also control for time varying factors at the individual ( $X_{ift}$ ) and field level ( $Z_{ft}$ ). These controls are worker’s picking experience, a linear time trend to capture farm level changes over time, and measures of each field’s life cycle.<sup>14</sup>

The disturbance term,  $u_{ift}$ , captures unobserved determinants of productivity at the worker–field–day level. Worker observations within the same field–day are unlikely to be independent since workers face similar field conditions. This is accounted for by clustering standard errors at the field–day level in all productivity regressions.

The group characteristic,  $G_{ft}$ , varies at the field–day level. The parameter of interest throughout is  $\gamma$ , which captures how variation in the group characteristic  $G_{ft}$  affects the productivity of the *same* worker on the same field over time. A comparison of the estimates of  $\gamma$  under both incentive schemes provides information on the differential effect of  $G_{ft}$  under relative incentives, when there are incentives to choose efforts cooperatively, to that under piece rates, when there are no incentives to choose efforts cooperatively.

It is important to be clear from the outset on how workers are allocated to a field. The *number* of workers allocated to a field varies both across fields, depending on their size, and within a given field through time, depending on the aggregate quantity of fruit on the field on that given day. This in turn depends on weather conditions and on the stage of its life cycle a field is at. As reported in table 1, group size on average is around 40 under both incentive schemes.

The *composition* of the group also varies each field–day. Workers themselves do not choose which field they work in and who they work with. Rather group composition is decided collectively by supervisors, taking account of the demand for workers to perform non–picking tasks, and how close workers live relative to the fields that need to be picked.<sup>15</sup>

The method by which workers are allocated to fields should then lead to no systematic relation between individual shocks to productivity ( $u_{ift}$ ) and group composition ( $G_{ft}$ ).

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<sup>14</sup>We measure the field’s life cycle as the number of days the field has been picked at any moment in time divided by the total number of days that the field is picked. Picking experience is defined as the number of field-days the worker has picked for.

<sup>15</sup>Unsurprisingly, workers are more likely to work alongside those they live with than other randomly chosen individuals. The probability that a worker works with someone they live with is .605, with a self reported friend, .549, and with neither someone they live with nor a friend, .344.

Descriptive statistics for each group characteristic considered in this section are reported in table 1. With the exception of the share of interns on the field, there is no significant difference between relative incentives and piece rates in how workers are assigned to fields on the basis of the group characteristics we consider.<sup>16</sup>

## 4.1 Group Size

In table 2 we present estimates of (6) by incentive scheme. The first group characteristic,  $G_{ft}$ , controlled for is group size – the number of workers that are picking on the field–day. There are several reasons why size might matter. In larger groups, the individual contribution to average productivity and hence the negative externality imposed on others is smaller. Moreover, larger groups may for example find it more costly to establish and enforce cooperative agreements other things equal.

The result in column 1a shows that under relative incentives, worker  $i$ 's productivity is significantly higher on field–days in which she is working alongside more co–workers. The estimated  $\gamma$  coefficient implies that a one standard deviation increase in group size leads to a 16% increase in productivity.<sup>17</sup> In contrast, for the same worker under piece rates, when there are no incentives to choose effort levels cooperatively, there is no effect of group size on individual productivity (column 1b).

The result is not driven by differences in group size across incentive schemes, nor by a lack of variation in group sizes under piece rates, as reported in table 1. As with nearly all of the results we report, standard errors under piece rates are smaller than those under relative incentives. Hence the non–significance of group characteristics under piece rates is not driven by these effects being imprecisely estimated.

The results also go against the hypothesis that larger groups are allocated to fields where productivity is expected to be higher. If so, there should be a positive effect of group size on productivity under *both* incentive schemes.<sup>18</sup>

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<sup>16</sup>The reason why the share of interns is lower under piece rates (in the second half of the season) is that interns tend to arrive earlier in the season. The share (and number) of co-workers of the same nationality is averaged across all worker-field-day observations under each incentive scheme. These vary across ethnic groups, and are higher for Polish workers, who account for 45% of the observations.

<sup>17</sup>Of course the coefficient reported in table 2 measures the elasticity of individual productivity with respect to group size. This shows that evaluated at the mean, a 1% increase in group size leads to a .23% increase in productivity.

<sup>18</sup>The effect of group size on productivity may be non-linear, if for example, the costs of writing and enforcing cooperative agreements are convex in group size. To check this, we estimate the effect of group size on individual productivity semi-parametrically using the methodology of Hausman and Newey (1995). We find that individual productivity initially increases in group size, but in sufficiently large groups, the addition of an extra worker has no effect on individual productivity. This turning point corresponds to a group size of between 30 and 45 workers.

## 4.2 Heterogeneity: Nationality

We now analyze whether individuals are more able to cooperate with co-workers of the same nationality than with others. In our data, many nationalities are represented, and the composition of workers by nationality varies considerably across fields and days.

Theory suggests that more heterogeneous groups might be less able to cooperate because of different preferences, lack of punishment mechanisms across ethnicities or simply because individuals only care about people in their national group (Alesina and La Ferrara (2004)).

Column 2a in table 2 shows that the group size effect is mostly driven by the size of the national group, namely a given individual cooperates less when there are more people of his own nationality and is unaffected by the number of workers of other nationality. This is consistent with the idea that workers only cooperate within national groups, and that the cost of making a cooperative agreement within the national group increases as this group becomes larger.

Column 3a shows that as the national group of worker  $i$  becomes larger *relative* to others, namely as the share of nationality  $i$  increases other things equal, worker  $i$  cooperates more, all else equal. This is consistent with the fact that as the national group gets relatively larger, workers of nationality  $i$  have a larger effect on the average productivity and hence the picking rate as given by (2). In addition, when the share of nationality  $i$  is larger, the benefits of cooperation mostly accrue to co-nationals.

Under relative incentives, the estimated  $\gamma$  coefficient in column 3a implies that a one standard deviation increase in the share of the national group reduces productivity by 7% under relative incentives. Taken literally, the estimates also imply that moving from a group in which the share of co-workers is at its mean value, to one in which all workers are of the same nationality, individual productivity would fall by 30%.<sup>19</sup>

Columns 2b and 3b of table 2 confirm that the national composition of co-workers only has an effect on individual productivity under piece rates. The standard errors under piece rates are again smaller than those under relative incentives. Hence the non-significance of group characteristics under piece rates is not driven by these effects being imprecisely estimated.

## 4.3 Heterogeneity: Ability

In table 3 we explore the relation between the heterogeneity of the group in terms of ability and the extent of cooperation. If larger transfers need to be made to enforce cooperative agreements in more heterogeneous groups, more heterogeneous groups should be less able to cooperate, all else equal. Similarly, if individuals have stronger social ties with workers of similar ability to

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<sup>19</sup>However this thought experiment is well outside the range of actual values for the national shares on any field-day we actually observe in the data.

themselves, then heterogeneous groups will be less cohesive and so may cooperate less.<sup>20</sup>

We measure the heterogeneity of the group on a field-day using the standard deviation of their estimated cost of effort,  $sd(\hat{\theta}_i)_{ft}$ . Throughout we also control for the mean cost of effort of workers on the field-day, so increases in  $sd(\hat{\theta}_i)_{ft}$  correspond to a mean preserving spread in the costs of effort (or ability) of the group.<sup>21</sup>

Column 1a shows that on field-days when worker  $i$  picks among a more heterogeneous group of workers, her productivity significantly increases under relative incentives. A one standard deviation increase in  $sd(\hat{\theta}_i)_{ft}$  increases individual productivity by 14%. There is no such effect of a mean preserving spread of ability on productivity under piece rates (column 1b). Taken literally, the estimates also imply the productivity of the average worker would fall by 56% moving from a group with the average distribution of ability, to a group of homogeneous ability.

Since the most able workers have the greatest incentives to break cooperative agreements other things equal, the next specification analyzes the behavior of the top 5% most able workers on a given field-day. Importantly, since group composition changes daily, this measure varies within the same individual over time. The result in column 2a shows that when worker  $i$  is in the top tail of the ability distribution, her productivity significantly increases above that when she is not in the top tail.

The sign of this effect is consistent with workers deviating from cooperative agreements when it is most profitable for them to do so. The magnitude of the effect corresponds to an extra \$15 in daily earnings, which, at the minimum wage, corresponds to two and a half hours of work. Under the assumption that deviators are punished, this amount provides an indication of the upper bound on the monetary value of the punishment.<sup>22</sup>

In column 3a we control for whether worker  $i$  is in the bottom 5% of the ability distribution on the field-day. We find that workers do not change their behavior relative to days when they are not in the bottom 5%. This is consistent with these workers have the weakest incentives to break cooperative agreements.

Columns 2b and 3b show that under piece rates, individual productivity does not depend on the position of worker  $i$  in the ability distribution on a given field-day. The findings confirm that in the absence of any reason to choose efforts cooperatively, there is no evidence that workers behave as if they have incentives to break cooperative agreements.<sup>23</sup>

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<sup>20</sup>The correlation coefficient for the estimated ability,  $\hat{\theta}_i$ , between workers and their self-reported friends is .211.

<sup>21</sup>In table 1 we report that the standard deviation and mean cost of effort by field-day, do not significantly differ across the relative incentive and piece rate schemes. Hence workers are not differentially sorted by ability across the two schemes.

<sup>22</sup>Note that the sign of this effect is however opposite to what would be expected if workers' behavior in the workplace is driven primarily by aversion to inequality (Fehr and Schmidt (1999)). In that case we would expect workers to work significantly *less* hard on field-days in which they are in the top 5% of the ability distribution.

<sup>23</sup>Moreover, consistent with their behavior under relative incentives, the same workers under piece rates do

Further analysis, not reported for reasons of space, indicates that the extent to which a given worker cooperates depends on how bad the worse pickers are. Namely, workers cooperate less when the worse pickers on the field are of lower ability. Intuitively, in this case individual incentives to deviate from cooperative behavior are stronger because the picking rate, (2), is raised to ensure the lowest ability workers still earn the minimum wage.<sup>24</sup>

#### 4.4 Heterogeneity: Stakes

Table 4 analyzes whether the extent to which workers cooperate depends on the share of co-workers present with exogenously higher stakes. To do so, we exploit the fact that some workers come to work on an internship programme as part of their university course. They face higher costs of being sent home due to their low productivity. Therefore interns should cooperate less compared to non-interns, other things equal. Moreover, the presence of interns on the field-day also lowers the returns to cooperation for workers who are not on the programme themselves.<sup>25</sup>

To begin with we estimate whether workers hired as part of an internship programme respond differently to the change in incentive scheme. We estimate the following specification, where all continuous variables are in logarithms;

$$y_{ift} = \alpha_i + \varphi_f + \gamma_0 P_t + \gamma_1 (P_t \times C_i) + \delta X_{ift} + \eta Z_{ft} + u_{ift} \quad (7)$$

$P_t$  is a dummy equal to one when piece rates are in place and zero when relative incentives are in place.  $C_i$  is a dummy equal to one if worker  $i$  is on an internship programme and zero otherwise. Worker and field fixed effects are still controlled for, as are the individual ( $X_{ift}$ ) and field level ( $Z_{ft}$ ) controls. We also continue to cluster standard errors at the field-day level.

Column 1 reports the specification in (7). For the average worker, productivity rises by 56% moving from relative incentives to piece rates. However, workers that are employed on an internship programme as part of their university course are significantly less responsive to the change in incentives ( $\hat{\gamma}_1 < 0$ ), namely the introduction of piece rates increases the productivity

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not behave as if they are averse to unequal payoffs across workers, or that they place particular attention to the payoffs of the least able workers (Charness and Rabin (2002)). As a further check, we explored whether workers change their behavior in response to the lowest payoff among workers of the same nationality. We found no evidence that individual productivity was significantly different depending on the ability of such individuals.

<sup>24</sup>In particular, we regress worker  $i$ 's productivity on the average cost of effort among the worse 10% of workers in the distribution of effort costs,  $E(\hat{\theta} | \hat{\theta} \geq \hat{\theta}^{90})_{ft}$ , where  $\hat{\theta}^{90}$  is the 90th percentile of the estimated cost of effort of all workers on the field-day. We find that one standard deviation increase in the cost of effort among the bottom 10% of workers leads others to increase productivity by 7%. There is no such effect under piece rates.

<sup>25</sup>The only significant difference in observables between workers on the internship programme and others, is that the former arrive earlier in the season and stay longer on the farm. The estimated worker ability (under piece rates) is not significantly different between interns and non-interns.

of interns by only 35% compared to 56% for non-interns.<sup>26</sup>

The remaining columns estimate specification (6) by incentive scheme, where we control for the share of co-workers that are interns on the field-day.

Column 2a shows that the productivity of all workers significantly increases, and thus cooperation decreases, when the share of interns on the field-day increases. Column 3a shows that the productivity of non-interns significantly increases as a greater share of workers present are on the internship programme. In short, the presence of those that have fewer incentives to cooperate affects the behavior of other workers who would normally cooperate – an increase in one standard deviation in the share of interns, increases the productivity of other workers by 24%. The estimates imply that moving from a group in which the share of interns is at its mean value, to one in which no interns are present, individual productivity would fall by 52%.<sup>27</sup>

Columns 2b and 3b repeat the analysis under piece rates and show that, in line with the interpretation that the share of interns determines productivity only through its effect on cooperation, the presence of interns has no effect on the productivity of others under piece rates.

The same result would however obtain if interns were to affect the productivity of non-interns only when the latter first arrive. Namely the results might be driven by individuals being affected differently over time by the presence of interns. To check this, we analyze the effect of the share of interns on the productivity of the cohort of non-intern workers that arrived later in the season and *only* worked under piece rates. This allows us to compare the effect of interns on workers at the same level of work experience under the two schemes. Reassuringly, in column 4 we find that the presence of interns on the field-day under piece rates does not significantly affect the productivity of the cohort of non-intern workers that only worked under piece rates.

## 5 Collusion or Altruism?

We now present evidence to identify the underlying motive for workers behaving cooperatively under relative incentives. In particular, we aim to distinguish between the hypotheses of pure altruism and collusion. ‘Altruistic’ workers, namely workers for whom the utility of co-workers enters directly into their utility function, would reduce their effort under relative incentives because they internalize the negative externality their effort imposes on others.

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<sup>26</sup>This is not just picking up some factor correlated to the course of study. We find that for those studying agriculture in particular, only those that are interns have significantly smaller changes in productivity moving to piece rates. Incidentally, the productivity of workers studying social sciences is no different from others under either incentive scheme.

<sup>27</sup>Some 68% of workers on the internship programme are Polish. As a robustness check we also controlled for the share of Poles in the field, both on its own and with the share of interns on the field day. In both cases the previous results were unchanged and the share of Poles on the field-day was insignificant.

Self interested workers, however, would also reduce effort under relative incentives, if they as a group, are able to enforce implicit collusive agreements through transfers and punishments. In our context such collusive behavior might occur despite the finite time horizon because workers are uncertain over when they, and their co-workers, will leave the farm. Workers are also uncertain whether they will keep on picking or will be allocated to different farm tasks in the future.

Using the notation from section 2, we denote worker  $i$ 's payoff as  $u_i = \phi(\text{compensation}_i) - \frac{\theta_i e_i^2}{2}$ . Both hypotheses of collusion and pure altruism then imply worker  $i$  behaves *as if* in reduced form she has social preferences of the form;

$$U_i = u_i + \sum_{j \neq i} \pi_{ij} u_j \quad (8)$$

where  $\pi_{ij}$  is the weight worker  $i$  places on the weight of worker  $j$ 's utility. Formally, workers are defined to be altruistic if the payoff of one or more co-workers enters their utility directly so that  $\pi_{ij} \neq 0$  for at least one  $j$  for all  $i$ . The structural and reduced form representation of preferences then coincide. The social weight,  $\pi_{ij}$ , may of course change over time depending on the number and nature of interactions between  $i$  and  $j$ .<sup>28</sup>

We define workers to be colluding if – (i) they use transfers and/or punishments to enforce an agreement designed to reduce productivity; and (ii) each worker's payoff only depends on her compensation, transfers and punishments she receives or gives, and her cost of effort so that in structural form,  $\pi_{ij} = 0$  for all  $(i, j)$ .

The economic environment we study has a number of features that facilitate both collusion and altruism. For example, workers live and work together, interacting repeatedly both inside and outside the work environment. This makes it relatively easy for them to build social ties, and provides a variety of mechanisms to provide transfers and enforce punishment.<sup>29</sup>

The difficulty in separating altruistic from collusive motives arises because factors that lead individuals to be more altruistic towards each other, typically also facilitate collusion, and *vice versa*. There are however a number of factors that create differences in observed behavior depending on whether workers cooperate because of collusion or altruism.

In section 5.1 we focus on the ability of workers to monitor each others' performance on the field. We present evidence on workers' productivity for another fruit type whose physical

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<sup>28</sup>A negative social weight can be interpreted as worker  $i$  being "spiteful" towards  $j$ . We assume altruistic feelings only extend towards co-workers performing similar tasks, but not towards the principal. In line with this, sociologists have long argued that individuals rarely have altruistic feelings for others that have direct authority over their actions (Homans (1950)).

<sup>29</sup>The ability to punish others – using either monetary payments, non-monetary transfers, or communicated threats – has been shown to be an important driving force behind cooperative behavior in the laboratory (Fehr and Gächter (2000), Andreoni *et al* (2003), Masclet *et al* (2003)), and in field studies of common resource management (Ostrom (1990)).

characteristics are such that, unlike for the fruit type studied so far, workers are unable to monitor the performance of co-workers. In sections 5.2 and 5.3 we present evidence on factors relating to differences across workers in their ability to communicate to each other, and differences in the social ties across workers.

These results provide a basis for understanding whether it is collusion or altruism that drives cooperative behavior in this setting. Discriminating between these hypotheses is important. The implications for designing a work environment that facilitates cooperation between co-workers – in terms of the ability of workers to monitor each other, the ability of workers to communicate to each other, and the mix of workers with different social ties – will be different depending on the underlying motive for cooperative behavior.

It is important to stress that we focus on the distinction between collusion and ‘pure’ altruism, namely that workers care about others regardless of the others’ behavior. This is in contrast to ‘reciprocal altruism’ (Axelrod (1984), Rabin (1993), Fehr and Fischbacher (2002)), whereby individuals are only altruistic towards those that act altruistically towards them, and thus cooperate as long as others cooperate. However, the only difference between collusion and reciprocal altruism is that under the latter, cooperation is sustained even in the last period of play.<sup>30</sup> We cannot however exploit this particular prediction in distinguishing between collusion and reciprocal altruism because we do not observe any worker leaving the farm while relative incentives are still in place.

## 5.1 Monitoring

To sustain a collusive agreement workers must necessarily be able to monitor each other’s behavior on the field-day. In contrast, the ability to monitor co-workers is irrelevant if workers’ behavior is driven by pure altruism. Under altruism, workers take into account the effect their effort has on others because it affects their own utility directly. Hence they cooperate regardless of whether they are monitored by others, and regardless of whether they can monitor co-workers’ performance.<sup>31</sup>

In our context monitoring is costless since workers work alongside one another. Workers are not physically separated and so can easily form accurate beliefs on the performance of co-workers. To establish whether the ability to monitor co-workers affects behavior, we analyze the effect on individual productivity of the change in incentive schemes for another fruit type, which we label

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<sup>30</sup>Hence distinguishing between these hypotheses has only minimal implications for the design of incentive schemes and organization of the workplace.

<sup>31</sup>In Kandell and Lazear’s (1992) terminology, altruism is a form of internal peer pressure or guilt, so individuals suffer disutility from hurting others even if others cannot observe their actions. In contrast collusive behavior is a form of external pressure or shame, where the observability of actions is the key driving force behind behavior.

type 2, as opposed to type 1 fruit that we have considered so far.

Type 2 fruit and grows on dense shrubs that are 6 to 7 feet high on average. In contrast to type 1 fruit, when picking type 2 fruit workers are unable to observe the quantity of fruit picked by workers in neighboring rows on the field-day. Hence the physical characteristics of type 2 fruit ensure workers *cannot* monitor each other on the same field-day.

Over 80% of our sample workers pick type 2 fruit at some point in the season. Of these workers, 54 pick under both relative incentives and piece rates. Only these workers are used in the analysis below. The two samples of workers – those that pick type 1 fruit at least 10 field-days under both incentive schemes and those that pick type 2 fruit under both incentive schemes – do not differ on observables, nor on their productivity when picking type 1 fruit. Pickers of type 2 fruit are however more likely to be female.

The number of workers that pick on the same field-day is much smaller for type 2 fruit compared to type 1 fruit – the average group size is 9 under relative incentives and 13 under piece rates. Type 2 fruit is picked across twelve fields over 269 field-days. Of these, 112 occur under relative incentives and 157 under piece rates. Worker productivity is 2.10 kg/hr under relative incentives and 1.62 kg/hr under piece rates.<sup>32</sup>

To estimate the effect on individual productivity for type 2 fruit,  $z_{ift}$ , of the change in incentive schemes we estimate the following specification;

$$z_{ift} = \alpha_i + \varphi_f + \gamma P_t + \delta X_{ift} + \eta Z_{ft} + u_{ift} \quad (9)$$

where  $P_t$  is a dummy equal to one when piece rates are in place and zero otherwise. The other controls are as defined before and we continue to cluster the disturbance terms by field-day.<sup>33</sup>

The results are presented in table 5. Column 1 reports that in the baseline specification (9), there is no significant effect on worker productivity moving from relative incentives to piece rates. The pattern of the other coefficients is however similar to the type 1 fruit (not reported).

The result suggests that when the production technology is such that co-workers performance cannot be monitored, workers do not cooperate under relative incentives, namely they are equally productive under relative incentives and under piece rates. The next column shows that this result is robust to restricting the sample to only the last ten days under relative incentives and the first ten days under piece rates. This minimizes the variation in productivity due to natural

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<sup>32</sup>The standard deviation of individual productivity under relative incentives and piece rates are 1.07 and .95 respectively.

<sup>33</sup>These controls are a linear time trend, worker’s picking experience and the field life cycle. The latter two are of course defined for type 2 fruit.

trends in productivity over the two halves of the season (column 2).<sup>34 35</sup>

One possibility is that, in contrast to the average worker, this particular subsample of workers simply do not cooperate, regardless of the monitoring technology. To check this, in column 3 we re-estimate the baseline specification for this subsample when they pick type 1 fruit, where the behavior of co-workers *can* be monitored. The results show that these workers cooperate as much as workers in the larger sample when monitoring is feasible. The effect of the introduction of piece rates is significant and of similar magnitude as in the larger sample.<sup>36</sup>

A remaining concern is that this may be due to different returns to other factors of production. To deal with this, in column 4 we combine the observations across fruit types for workers that pick both fruit types under both incentive schemes. The result shows that there is a significant difference-in-difference in the response of individual worker productivity to the introduction of piece rates between fruit type 1 and fruit type 2.

Overall, the results indicate the effect of the change in incentives on worker productivity depends fundamentally on the ability of workers to monitor their co-workers. When workers are able to monitor each other (type 1 fruit) productivity is significantly lower under relative incentives. In contrast productivity is identical under both schemes when workers cannot monitor each other (type 2 fruit). Given that monitoring is necessary to enforce collusion while it does not affect altruism, the comparison of productivity by fruit type and incentive scheme supports the hypothesis that workers behave cooperatively because of collusion.<sup>37</sup>

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<sup>34</sup>Further analysis not reported for reasons of space indicate that results are robust to restricting the sample to field-days where workers only picked type 2 fruit and performed no other tasks. This minimizes variation in productivity arising from a different composition of tasks over the two halves of the season. To analyze the heterogeneity in responses to the change in incentives, we also estimated (9) including a full set of interactions between worker fixed effects and the piece rate dummy. For workers that have picked more than 5 field-days under both incentive schemes, there is no evidence of any of them having significant increases (or decreases) in productivity moving from relative incentives to piece rates when picking type 2 fruit.

<sup>35</sup>An identifying assumption in (9) is that workers do not anticipate the change in incentives. To check for this we include a dummy for the week before piece rates were introduced. Neither this nor the piece rate dummy affect productivity. We also show that the effect of piece rates does not depend on the amount of time the scheme is in place for, namely piece rates have no effect on productivity at any point of the season.

<sup>36</sup>When this sample of workers picks fruit type 1, average productivity under relative incentives is 4.80 kg/hr, and 8.01 kg/hr under piece rates. These are not significantly different to the main sample used before as reported in table 1.

<sup>37</sup>These results are in the same spirit as Ostrom *et al* (1994). They find that in the context of common resource management, outcomes are closer to the social optimum if there are institutions that render the excess extraction of common resources visible or easy to detect precisely because it allows those who violate cooperative agreements to be punished.

## 5.2 Communication

Recent experimental evidence shows that communication, even if non-binding, helps sustain cooperative outcomes in a variety of public goods and common resource management games.<sup>38</sup> Communication might be important for collusive behavior if such agreements entail transfers that have to be agreed upon daily.<sup>39</sup> Communication might also proxy for social ties and hence for the degree of altruism if, as is reasonable, individuals only form ties with those they can communicate with and individuals are altruistic only towards those they have ties with.

However, the effect of communication on cooperation, if any, differs under altruism compared to collusion. In particular, only the ability of workers to communicate directly with each other proxies for their social ties and hence altruism whereas communication via a third party might help enforce collusive agreements.

This section exploits the difference between direct and indirect communication to shed light on the distinction between collusion and altruism. Workers in our sample come from eight Eastern European countries and hence speak different first languages. From the workers' questionnaire we can identify which, if any, second language each worker can speak. Seventy percent of workers speak a second language, the most common being Russian.<sup>40</sup>

To see how the ability of workers to communicate via third parties affects productivity, we regress the individual productivity of worker  $i$  on the share of Russian speakers among workers of the same nationality as  $i$  ( $R_{ift}$ ),

$$y_{ift} = \alpha_i + \varphi_f + \gamma_0 R_{ift} + \delta X_{ift} + \eta Z_{ft} + u_{ift} \quad (10)$$

In this specification we continue to control for worker and field fixed effects, so identification of  $\gamma_0$  arises from the variation in productivity within a worker on the same field over time, as the ability of co-workers to speak Russian varies.<sup>41</sup>

Under altruism,  $\gamma_0 = 0$  because workers can communicate to others of the same nationality

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<sup>38</sup>See Cardenas (2003) and Ostrom *et al* (1994) for evidence on this. It is generally found that if players are allowed to communicate in each round of play (as in this setting), levels of cooperation are higher than if communication only occurs in the first round. One reason why communication fosters cooperation is that it may reinforce group identity (Dawes and Thaler (1988)).

<sup>39</sup>In particular, workers may need to communicate in order to agree on the form of transfers, punishments, and the identity of those that punish deviators. They may also need to make clear their intentions, an aspect of cooperative behavior highlighted and formalized by Rabin (1993).

<sup>40</sup>23% percent of workers speak Russian as a second language, 10% as mother tongue. The next most common second language is German, spoken by 18% of workers. The most common first language is Polish (46% of workers) but no workers from other countries report Polish as their second language. From our time in the field, it is clear that many workers speak little or no English. Indeed we translated the worker questionnaire into a number of Eastern European languages.

<sup>41</sup> $R_{ift} = 1$  for native Russians who comprise 10% of the sample. The  $\gamma_0$  coefficient is therefore identified from workers of all other nationalities.

through their first language. In contrast, under collusion, a worker may be more likely to cooperate in this circumstance (and hence  $\gamma_0 < 0$ ) because the ability to speak a second language *within* the national group affects their ability to communicate with others *outside* the national group.

Finding that the productivity of a worker is affected by the share of workers within and across national groups to speak a common second language may however just pick up some common unobservable among those that speak the second language. To check for this we ask, is the behavior of a worker *who does not speak the second language* affected by the share of co-nationals on the field who can speak the second language? We therefore estimate (??) for the sample of workers who cannot speak Russian as a second language.

Since the ability to communicate with other ethnic groups depend on *their* ability to speak Russian as well, we estimate productivity as a function of the share of Russian speakers among workers of the same nationality as  $i$  ( $R_{ift}$ ), the share of Russian speakers among other nationalities ( $R_{-ift}$ ), and the interaction between the two to take into account their complementarity;

$$y_{ift} = \alpha_i + \varphi_f + \gamma_0 R_{ift} + \gamma_1 R_{-ift} + \gamma_2 (R_{ift} \times R_{-ift}) + \delta X_{ift} + \eta Z_{ft} + u_{ift} \quad (11)$$

If collusion drives cooperation, then  $\gamma_0 \leq 0$ ,  $\gamma_1 \leq 0$ ,  $\gamma_2 \leq 0$  *irrespective* of whether worker  $i$  can herself speak Russian.

If altruism drives cooperation, however, the sign of  $(\gamma_0, \gamma_1, \gamma_2)$  depend on whether worker  $i$  is a Russian speaker. If she is, she can communicate with workers of her own nationality in her first language, regardless of whether they can speak Russian. The share of Russian speakers in her national group does not influence how altruistic she feels towards them so  $\gamma_0 = 0$ . However she is likely to be more altruistic towards those in other nationalities, the greater the share of workers in other nationalities that she can speak to in Russian, so  $\gamma_1 \leq 0$ . Finally there is no interaction effect between the shares of Russian speakers across national groups so  $\gamma_2 = 0$ .

If worker  $i$  does not speak Russian, she is still able to communicate with those in her national group in their first language, so  $\gamma_0 = 0$ , she will not be altruistic towards others in other nationalities that she cannot communicate with in any language, so  $\gamma_1 = 0$ , and there is no interaction between the shares of Russian groups across national groups so  $\gamma_2 = 0$ .

To summarize, the signs of the predicted  $(\gamma_0, \gamma_1, \gamma_2)$  coefficients under the hypotheses that workers collude or are altruistic, by the ability of worker  $i$  to herself speak Russian, are as follows;

	<b>Worker's Ability to Speak Russian</b>	$\gamma_0$	$\gamma_1$	$\gamma_2$
<b>Collusion</b>	can or cannot speak Russian	$\leq 0$	$\leq 0$	$\leq 0$
<b>Altruism</b>	can speak Russian	0	$\leq 0$	0
	cannot speak Russian	0	0	0

The results are reported in table 6. In column 1a we estimate (10) and find that productivity is significantly lower when the share of Russian speakers in  $i$ 's national group is higher ( $\hat{\gamma}_0 < 0$ ). Column 2a show this holds also if worker  $i$  does not herself speak Russian.

Column 3a then estimates (11) and shows that productivity is significantly lower when the share of Russian speakers in either group is higher and the two are complements ( $\hat{\gamma}_0 < 0, \hat{\gamma}_1 < 0, \hat{\gamma}_2 < 0$ ) although the interaction term is not significant at conventional levels.

While these results confirm that workers are not altruistic towards *all* co-workers, they may just pick up the fact that Russian speakers are more altruistic towards other Russian speakers.

In column 4a we restrict the sample to workers who *cannot* speak Russian. The ( $\hat{\gamma}_0, \hat{\gamma}_1, \hat{\gamma}_2$ ) coefficients are not significantly different between columns 3a and 4a. The results imply that both Russian and non-Russian speakers are equally affected by the ability of other co-workers to communicate amongst themselves.

Columns 1b to 4b repeat the analysis for the same workers but under piece rates, when there are no incentives to cooperate. There is no evidence that worker productivity alters with the ability of others on the field-day to speak Russian.

A concern is that workers may learn to use other modes of communication over time. Hence the ability of the group of workers to speak Russian later in the season under piece rates, may be less informative of their true ability to communicate than early in the season, under relative incentives. To address this we examine how the productivity of the cohort of workers that arrive later in the season and hence only worked under piece rates, changes with the ability of co-workers to speak Russian. The result in column 5 shows that under piece rates, this cohort of workers do not respond to the ability of others on the field-day to speak Russian.<sup>42</sup>

These results are consistent with workers cooperating due to collusive agreements. Under collusion, irrespective of worker  $i$ 's own ability to communicate with other co-workers, she is still subject to the collusive agreement. The results can only be reconciled with the hypothesis that workers cooperate because of altruism if – (i) each worker  $i$  is only altruistic towards those that can speak Russian, and (ii) this is true irrespective of whether  $i$  can herself speak Russian.

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<sup>42</sup>We also addressed two further concerns with these results. First, it may be that the share of individuals of the same nationality that speaks Russian is picking up the effect of group size *per se*. This would be the case if the fraction of people that can speak Russian is larger in *smaller* groups. To check for this we additionally controlled for the number of workers of the same and the other nationality. The result is very similar to that in column 2a. A second concern is that those that speak Russian are somehow different than others. This may be of concern given that the sample comprises individuals from Eastern Europe in their early 20s, not all of whom would have had to study Russian in school. This type of selection of Russian speakers is hard to reconcile with the results that – (i) *irrespective* of whether worker  $i$  can herself speak Russian, her behavior changes with the share of Russian speakers across different nationalities on the field-day, and (ii) that the shares of workers of the same and other nationalities have no effect on productivity under piece rates.

### 5.3 Social Ties

A key difference between the hypotheses of altruism and collusion relates to the strength of social ties among workers. Given the evidence indicating that workers are altruistic only towards some of their colleagues, if at all, it is plausible to assume that workers are altruistic only towards those they have established social ties with. New arrivals on the farm take time to form social ties and hence become altruistic towards co-workers. Hence under relative incentives, new arrivals are likely to initially cooperate less, and hence have higher productivity, compared to workers that have been present on the farm for longer.

Under collusion, in contrast, new workers are likely to have low productivity from their first day of work if they are informed immediately of the agreement. This in turn might depend on the experience of the group as a whole, namely new workers that start out in a group of experienced workers are more likely to be informed and collude straight away.

The next set of results explores these intuitions using the variation that arises because some workers start picking fruit on the first day after arrival while others are initially assigned to other tasks and start picking after 10 days or more. We refer to these workers as ‘early’ and ‘late’ starters respectively.

We then make the following assumptions – (i) late starters can still form social ties with other workers regardless of whether they actually pick with them; (ii) if workers cooperate because of some collusive agreement, early and late starters are both equally subject to this norm. The former assumption is likely to be true because of the close proximity with which workers live and work together. The latter is likely to hold if there are no restrictions on the information that can be communicated to new arrivals.

A key implication is that if workers cooperate because of collusive agreements, there should be no productivity differential between early and late starters when they pick on the same field-day. In contrast if altruism drives cooperation and it takes time for workers to develop altruistic feelings towards others, early starters should be, at least initially, less cooperative, and hence more productive, than late starters.

Of the workers in our main sample – namely those workers that pick at least 10 field-days under both incentive schemes – 92 start picking as soon as they arrive on the farm while 33 are assigned to other tasks for at least 10 days.<sup>43</sup> The two groups are however identical on observable characteristics, average productivity, and their estimated cost of effort, although late starters stay longer on the farm overall.<sup>44</sup>

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<sup>43</sup>To make a cleaner comparison between early and late starters, we drop workers who start picking between 2 and 9 days after arrival. Late starters are initially assigned to planting and other farm operations.

<sup>44</sup>After their first day of picking, late starters stay on average for 103 days while early starters stay for 97 days. The difference is not statistically significant. On the whole, late starters spend 125 days on the farm which is significantly higher than the 97 days spent by early starters. Most of this difference is driven by late starters

We first estimate the following specification;

$$y_{ift} = \varphi_f + \gamma late_i + \zeta_w + \delta X_{ift} + \eta Z_{ft} + u_{ift} \quad (12)$$

where  $late_i$  is a dummy equal to one if worker  $i$  started picking ten days or more after arrival and zero if the worker started picking one or two days after arrival. As  $late_i$  is individual specific, worker fixed effects are not controlled for. Since most non-picking tasks take place early in the season we control for week of arrival  $\zeta_w$ , to difference out unobservables that are common to those that arrive earlier in the season. As before, the sample includes workers that have worked at least 10 field-days under both schemes, but is restricted to their first week of picking under relative incentives.

The result is reported in column 1a of table 7. Productivity during the first week of picking under relative incentives is not significantly different between early and late starters. In other words, those workers that pick as soon as they arrive on the farm do not behave differently to those that have been present longer on the farm but were not picking. This suggests that the extent of social ties between co-workers, which late starters are more likely to have developed relative to early starters, is not the basis of cooperative behavior in this setting.<sup>45</sup>

However, this result may be due to unobserved individual heterogeneity. For example if late starters are intrinsically more able than early starters, their higher ability increases productivity. This may cancel out the effect of developing social ties with co-workers which is to lower productivity.

To check for this, column 1b repeats the analysis for a sample of workers that arrived *after* the introduction of piece rates. Of these, 82 started picking as soon as they arrived on the farm while 10 were assigned to other tasks for 10 days or more.<sup>46</sup> Under piece rates the fact that late starters have developed more social ties with co-workers compared to early starters does not matter, as worker's productivity does not affect others' pay. Hence any difference in productivity between early and late starters suggests they differ on other characteristics. Reassuringly, we find the productivity of the two groups to be identical in their first week under piece rates.

An alternative method to deal with unobserved heterogeneity is to analyze whether accumulated altruism affects individual returns to experience. This effect is identified within a worker

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initially spending more days on non-picking tasks.

<sup>45</sup>Most workers that already know each other from before their time on the farm, say because they study in the same university, typically arrive on the farm together. It is therefore not possible to estimate if initial productivity is different within early starters on the basis of how many workers they already know on the farm.

<sup>46</sup>As in the previous sample, late starters stay longer but have otherwise similar characteristics. In contrast to the previous sample, the productivity of late starters who arrived under piece rates is on average 25% lower compared to other workers who also arrived under piece rates.

through time by estimating the following specification;

$$y_{ift} = \alpha_i + \varphi_f + \delta(\text{picking experience}_{ift}) + \tau(\text{late}_i \times \text{picking experience}_{ift}) + \eta Z_{ft} + u_{ift} \quad (13)$$

We estimate whether the returns to experience in the first week of picking differ between early and late starters, so  $\tau \neq 0$ . Under the altruism hypothesis, late starters, who have had time to form social ties, should have lower returns to experience in the first week of picking so  $\tau < 0$ . The result in column 2a shows that conditional on worker fixed effects, the returns to experience are identical across the two groups under relative incentives,  $\hat{\tau} = 0$ .<sup>47</sup>

In column 2b the same specification is estimated under piece rates, where we also find no differential returns to experience. This confirms the stock of social ties or past interactions does not affect behavior under either scheme.

While altruism depends on the past interactions between individual workers, collusion depends on the cumulative experience of the group as a whole. New arrivals are therefore likely to be less productive if they start picking among a group of workers that have been present on the farm longer, and are therefore more aware of the collusive norm and how to enforce it. To explore this idea, in column 3a we control for the mean number of days on the farm for the group of co-workers on the field-day.

Conditional on worker  $i$ 's individual picking experience, the mean days on the farm for the group of co-workers significantly reduces individual productivity in the first week  $i$  picks under relative incentives. Namely workers are initially less productive when their co-workers have been present for longer on the farm. An increase of one standard deviation in the mean days spent on the farm by co-workers, reduces individual productivity by 24%.<sup>48</sup>

Column 3b repeats the analysis for workers who arrived after the introduction of piece rates. The effect of group experience on the first week of picking under piece rates is actually positive, indicating that workers' initial productivity is higher when they work along side more experienced pickers. This may be because of learning spillovers that occur in the first week of picking for new arrivals when their co-workers are both more experienced and working optimally. In contrast under relative incentives, new arrivals appear to immediately become subject to collusive agreements.<sup>49</sup>

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<sup>47</sup>There are still positive returns to experience in the first week of picking for early and late starters. The point estimate of  $\delta$  is .171, and this is significantly different from zero.

<sup>48</sup>One possibility is that people who have been on the farm the season before are more informed and can teach others how to collude. There are 15 such workers, but they all arrive later in the season when the norm is likely to already be in place. Moreover, those that are re-hired from the previous season typically perform supervisory tasks rather than picking tasks. We find no evidence that the presence of re-hired workers has an affect on others.

<sup>49</sup>These results are not just picking up the effect of the linear time trend. The mean number of days on the farm for the group of co-workers on the field-day has correlation coefficient with the time trend of .309. Moreover, the coefficient of the time trend in columns 3a and 3b is the same as in the baseline specification, and the results

## 6 Conclusion

There are many economic situations in which individuals can be better off if they cooperate. It is therefore important to understand the forces shaping cooperation in the real world. This paper uses personnel data to present evidence on the determinants of cooperation in the workplace, and to distinguish between underlying motives for cooperation.

The data has four key features that helps us shed light on both issues – (i) we observe the same individual working in different groups on different days; (ii) the allocation of workers to groups is exogenous to characteristics of the group that affect their ability to cooperate; (iii) the composition of the group varies along dimensions that may affect their ability to cooperate; (iv) we observe the same workers under both a relative incentive scheme, where there are incentives for workers to cooperate, and under piece rates, where there are no incentives for them to cooperate.

The data allows us to estimate the effect of seven dimensions of the work environment on cooperative behavior. We find that workers cooperate more when their group is smaller, more homogeneous in terms of nationality and ability, when the share of workers with high stakes is lower, when communication across ethnic groups is possible and when they start working alongside more experienced workers. Importantly we show that the ability of workers to monitor each others' performance is a necessary prerequisite for cooperation.

The balance of evidence suggests individual behavior is not driven by pure altruism, but is more in line with the traditional wisdom of the folk theorem. In short, although individuals cooperate, this is not because they have social preferences in structural form.

Our results have some wider implications for the literature. First, we provide real world evidence to corroborate experimental results. We show that individuals cooperate outside of the laboratory. In contrast to some interpretations of experimental results, however, our results cast doubts on the assumption that the payoffs of others enter individual utility functions directly.

Second, our results indicate that the design of incentives and organization of the work environment need to be considered simultaneously in economic analysis, a fact long recognized in the organizational behavior literature, and more recently reiterated in the literature on human resource management (Ichniowski and Shaw (2003)).<sup>50</sup> For example the results here speak to Lazear's (1989) observation on how rarely workers are compensated according to rank-order tournaments, despite the fact that theory suggests they may have desirable incentive effects (Lazear and Rosen (1981)). One reason why such schemes may not be observed in practice is because

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remain unchanged if the time trend is dropped altogether.

<sup>50</sup>This type of analysis in the organizational behavior literature dates back to the 'Hawthorne experiments' of Roethlisberger and Dickson (1937). They find that moving from individual piece rates to a group based piece rate, there was a permanent increase in output of 30% after workers were allowed to socialize with each other. In Roy's (1952) study of bricklayers, he provides evidence that under individual piece rates, workers set informal quotas to reduce output due to ratchet concerns.

workers can collude against them.<sup>51</sup>

Finally, under the relative incentive scheme analyzed here, workers have incentives to cooperate because each worker's effort imposes a *negative* externality on co-workers, all else equal. In this setting cooperation between co-workers is to the detriment of the principal. Incentive schemes such as team based pay, in contrast, entail *positive* externalities. In those settings cooperation among workers benefits the principal. Establishing whether cooperation arises also when it entails higher, rather than lower, effort levels is key to assess the desirability of team incentives and, as such, is the subject of ongoing research.

## 7 Appendix: Calibration

We calibrate the first order conditions of the workers' maximization problem derived in section 2 to compute an estimate of each worker's cost parameter,  $\theta_i$ , under each incentive scheme and each behavioral assumption of self interest and full cooperation. Since the workers' cost (ability) parameters are innate, we should find the *same* implied distributions of costs across workers under both incentive schemes if the underlying behavioral assumption is correct.

Workers are paid on the basis of their observed productivity  $y_i$  which is a function of their unobserved effort  $e_i$ . Taking this into account, the first order conditions for the choice of effort under relative incentives assuming that workers are non-cooperative, assuming they are fully cooperative, and under piece rates are respectively;

$$\phi' \left( \frac{y_i}{\bar{y}} \right) \frac{\partial y_i}{\partial e_i} \left( \frac{\sum_{j \neq i} y_j}{(\sum_i y_i)^2} \right) = \frac{1}{N} \theta_i e_i \quad (14)$$

$$\frac{\partial y_i}{\partial e_i} \frac{1}{(\sum_i y_i)^2} \left[ \phi' \left( \frac{y_i}{\bar{y}} \right) \sum_{j \neq i} y_j - \sum_{j \neq i} \phi' \left( \frac{y_j}{\bar{y}} \right) y_j \right] = \frac{1}{N} \theta_i e_i \quad (15)$$

$$\phi' (\beta y_i) \beta \frac{\partial y_i}{\partial e_i} = \theta_i e_i \quad (16)$$

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<sup>51</sup>A growing theoretical and experimental literature has emphasized the role of peer pressure, altruism, conformist behavior, or social status concerns, in the workplace. The first papers to address this were Holmstrom and Milgrom (1990), who study the effects of workers colluding in organizations, Kandel and Lazear (1992), who examine the role of peer pressure, and Rotemberg (1994) who analyzes the implications of workers being able to choose to become altruistic towards each other when their actions are strategic complements. More recently, Huck *et al* (2003) and Kandori (2003) have emphasized the role of conformist behavior in the workplace, and Fershtman *et al* (2003) have focused on social status concerns to explain wage inequality within firms. Hart (2001) also models norms within firms to understand the relation between how much agents trust each other and the optimal ownership structure. Fehr and Falk (2002) overview the experimental literature highlighting the interaction between monetary incentive schemes and workers' underlying motivations.

We assume the benefit function is a CRRA type,  $\phi(y) = \rho y^{\frac{1}{\rho}}$  for  $\rho \geq 1$ . Throughout we set  $\rho = 2$ . All reported results are robust to alternative choices of  $\rho$ .

We therefore derive three estimates of  $\theta_i$  based on the calibration of the first order conditions (14), (15), and (16) respectively – (i) under relative incentives assuming workers behave non-cooperatively,  $\hat{\theta}_i^{RN}$ ; (ii) under relative incentives assuming workers choose efforts cooperatively,  $\hat{\theta}_i^{RC}$ ; and, (iii) under piece rates,  $\hat{\theta}_i^P$ . Finally, we compare the distribution of  $\hat{\theta}_i^P$  to the distributions of  $\hat{\theta}_i^{RN}$  and  $\hat{\theta}_i^{RC}$  to assess if either of these two assumptions on the underlying behavior of workers is consistent with the observed change in productivity.

While productivity ( $y_i$ ), picking rates ( $\beta$ ) and group size ( $N$ ), are all observed, we still need to derive an estimate of worker effort,  $e_i$ , to calibrate the first order conditions and recover the implied distribution of  $\theta$ .

### Workers’ Effort

To estimate workers’ effort, we assume a Cobb Douglas relationship between effort and productivity.<sup>52</sup> We estimate the productivity of worker  $i$  on field  $f$  on day  $t$ ,  $y_{ift}$ , using the following panel data regression, where all continuous variables are in logarithms;

$$y_{ift} = (\alpha_i \times P_t) + \varphi_f + \delta X_{ift} + \eta Z_{ft} + u_{ift} \quad (17)$$

The estimate of worker  $i$ ’s effort in field  $f$  on day  $t$  under incentive scheme  $s \in \{R, P\}$  is each worker’s estimated fixed effect added to the residual from the regression (17) when incentive scheme  $s$  is in place;

$$\hat{e}_{ift}^s = \hat{\alpha}_i^s + \hat{u}_{ift}^s \quad (18)$$

The first term captures the workers average effort over time under incentive scheme  $s$ . The second term captures how much of the worker’s productivity is not explained by observables. This provides an estimate of each workers effort (measured in kilograms per hour) on *every* field–day on which they pick.

Consistent with the actual distribution of productivity by incentive scheme in figure 2, we find the mean and variance of effort both rise significantly moving from relative incentives to piece rates (not shown). There is also little evidence of churning of workers – those who put in the most effort under relative incentives continue to exert the most effort under piece rates.

### Workers’ Cost of Effort

To derive the implied distribution of workers’ cost of effort under each incentive scheme and

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<sup>52</sup>This ensures that the same effort on two different days can lead to two different levels of productivity depending on other inputs into production, such as field conditions. In the first order conditions (14) to (16),  $\frac{\partial y_i}{\partial e_i} \propto \frac{y_i}{e_i}$  with a Cobb Douglas specification, so that  $\theta_i$  is identified up to some scalar in each case. This does not affect the comparison of the estimated  $\theta_i$ ’s across the first order conditions.

behavioral assumption, we substitute data for estimated effort, observed productivity, observed picking rates, and group size into the first order conditions (14), (15), and (16). We then obtain an estimate of  $\theta_i$  on each field-day the worker picks,  $\hat{\theta}_{ift}$ . The model is overidentified as sample workers work at least 10 field-days under each incentive scheme. We use  $\hat{\theta}_i = \text{median}(\hat{\theta}_{ift})$  to combine the estimates within each worker and incentive scheme as this is less sensitive to outliers. The results are robust to taking the mean of the  $\hat{\theta}_{ift}$ 's or to estimating them for each worker using maximum likelihood.

Figure A1 shows the kernel density estimate of the implied distribution of workers' cost of effort  $\hat{\theta}_i^P$  and  $\hat{\theta}_i^{RN}$ , namely under the assumption that workers ignore the externality they impose on others under relative incentives. The figure shows that the distribution of cost parameters under relative incentives lies almost entirely to the *right* of the distribution under piece rates, indicating that the implied cost of effort is higher under relative incentives than under piece rates. Assuming cost of effort is an innate parameter, the fact that the same distribution of costs cannot be fitted to both incentive schemes indicates that effort choices are not consistent with workers behaving non-cooperatively under both incentive schemes.

Next, we estimate of the distribution of workers' cost of effort  $\hat{\theta}_i^P$  and  $\hat{\theta}_i^{RC}$ , namely under the assumption that workers choose their efforts cooperatively to maximize the sum of utilities of all workers under relative incentives. Figure A2 shows the implied distributions of the cost parameter by incentive scheme. The distribution of  $\hat{\theta}_i^P$  under piece rates is, by definition, unchanged to that in figure A1. However, the distribution of costs under relative incentives  $\hat{\theta}_i^{RC}$  now lies almost entirely to the *left* of the distribution under piece rates.

If workers chose their effort levels cooperatively, then the cost of effort under relative incentives would have to be significantly *lower* under relative incentives to fit the observed productivity data. In other words, productivity is actually too high under relative incentives to be explained by workers choosing their effort levels cooperatively.

Figures A1 and A2 together reveal an interesting pattern. The observed change in productivity is too large to be reconciled with the assumption of non-cooperative behavior, but too small to be reconciled with the assumption of fully cooperative behavior. This suggests workers are able to cooperate to some extent.

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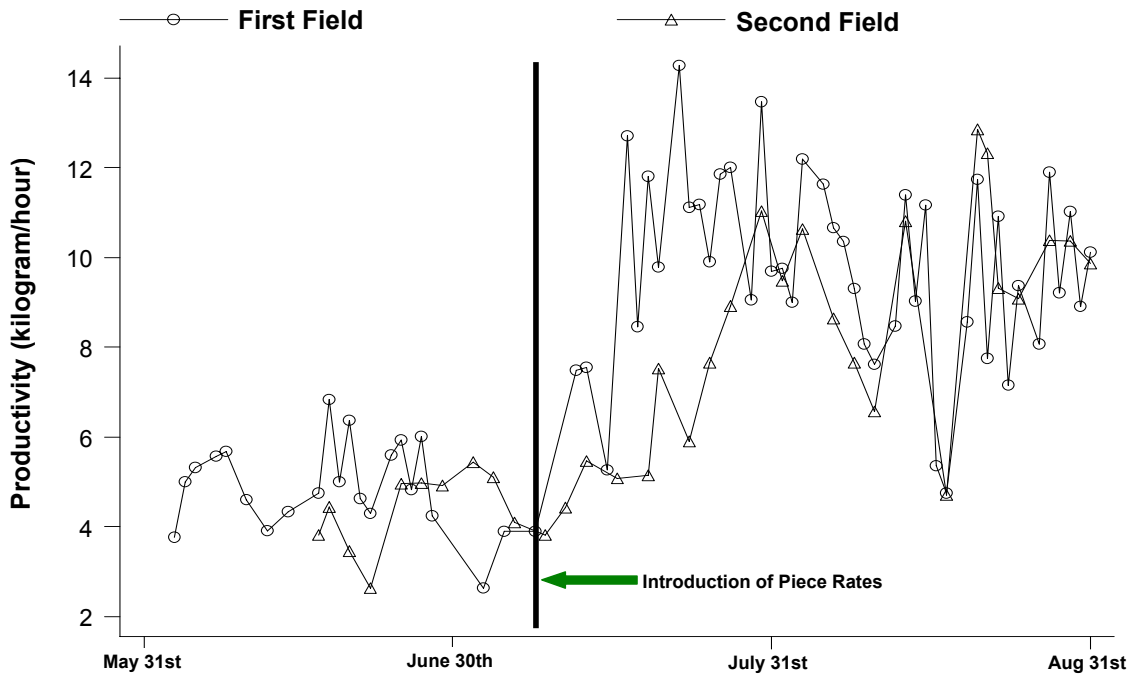
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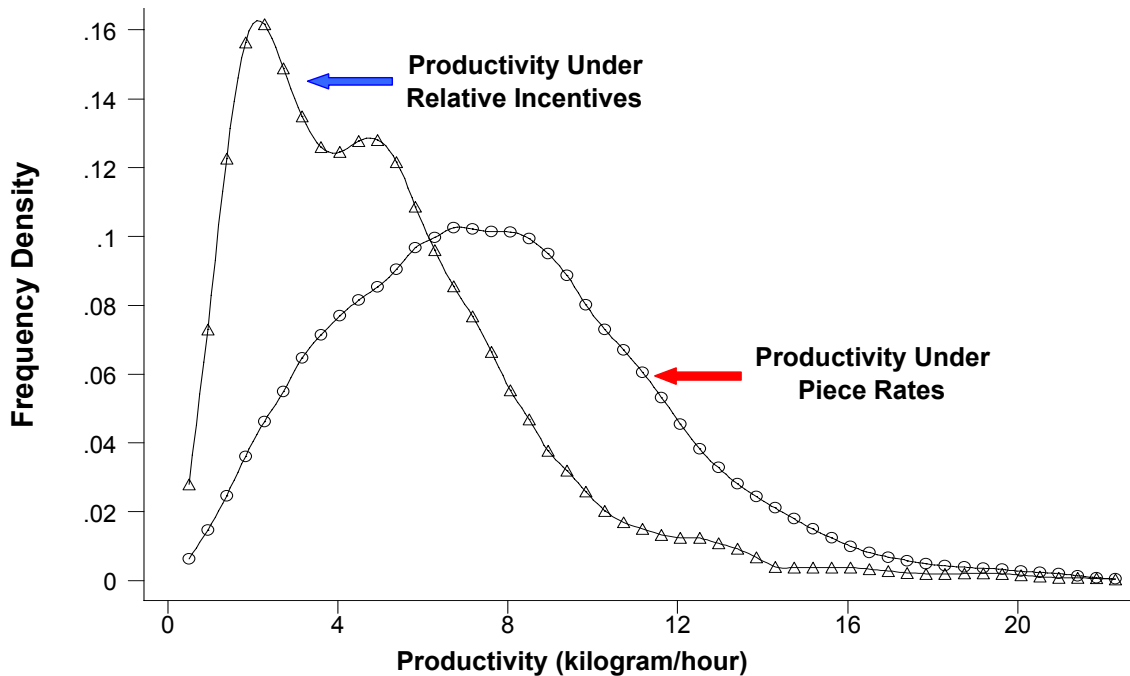
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**Figure 1: Productivity (kg/hr) Over the Season**

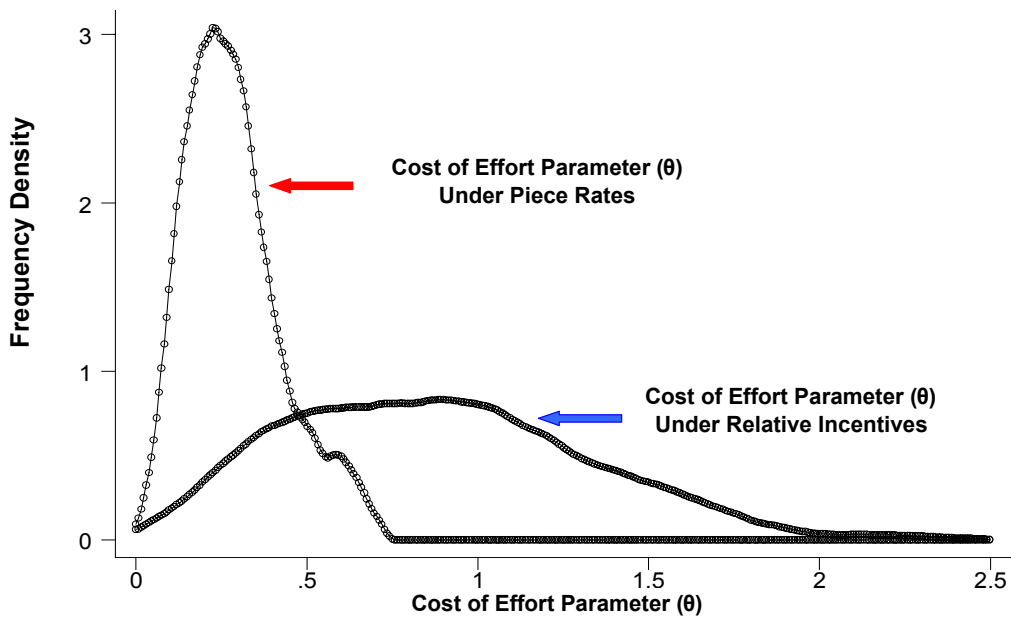


**Figure 2: Distribution of Productivity (kg/hr) by Incentive Scheme**

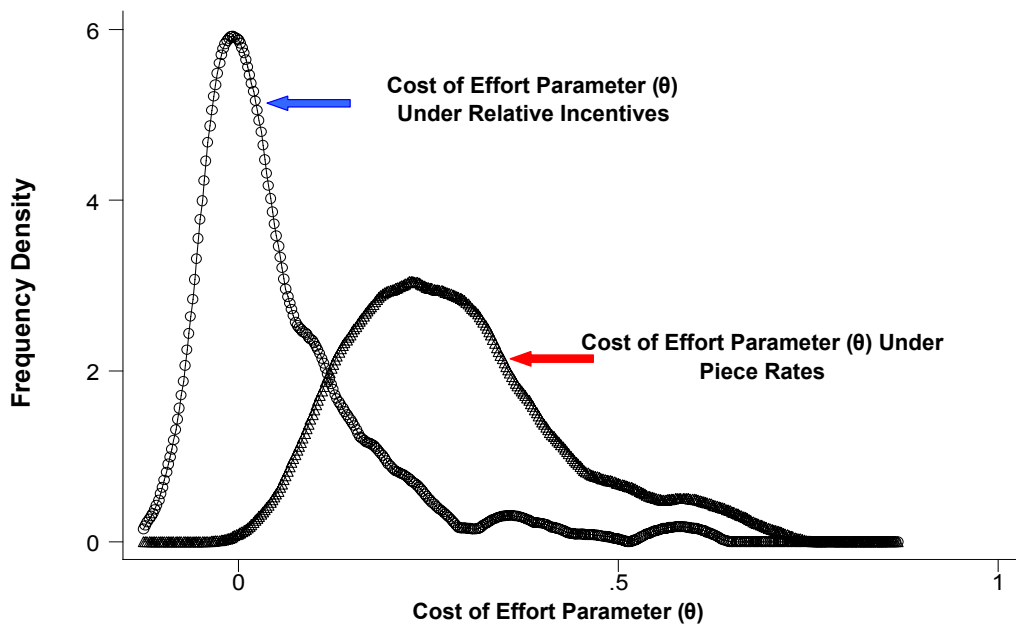


**Notes:** In figure 1, average productivity on the two main fields is shown for those workers that work at least 10 field-days under each incentive scheme. These fields are operated for the greatest number of days under each incentive scheme. Together they contribute one third of the total worker-field-day observations. The kernel density estimates in figure 2, and all those that follow, are calculated using an Epanechnikov kernel, based on 50 grid points and the bandwidth suggested by Silverman (1986, pp38-40).

**Figure A1: Kernel Density Estimates of Cost of Effort Parameter, by Incentive Scheme Assuming Non-Cooperative Behavior**



**Figure A2: Kernel Density Estimates of Cost of Effort Parameter, by Incentive Scheme Assuming Fully Cooperative Behavior**



**Notes:** Kernel density estimates are calculated using an Epanechnikov kernel. The underlying benefit function is assumed to be;

$$\varphi(x) = 2x^{\frac{1}{2}}$$

The total cost of effort is assumed to be quadratic in effort. Under non-cooperative behavior we assume each worker chooses her effort level to maximize her own payoff. Under fully cooperative behavior we assume workers choose their effort levels to maximize the sum of all workers payoffs.

**Table 1: Descriptive Statistics by Incentive Scheme**

Mean, standard deviations in parentheses

	Relative Incentives	Piece Rates
<b>Worker productivity (kg/hr)</b>	5.01 (3.21)	7.98 (3.81)
<b>Group size</b>	41.1 (28.8)	38.1 (18.2)
<b>Share of total workers of same nationality as worker <i>i</i></b>	.175 (.192)	.156 (.150)
<b>Number of workers of same nationality as worker <i>i</i></b>	7.80 (9.93)	6.20 (5.97)
<b>Standard deviation of cost of effort (sd(<math>\theta</math>))</b>	.142 (.035)	.116 (.041)
<b>Mean cost of effort (mean(<math>\theta</math>))</b>	.297 (.068)	.291 (.067)
<b>Share of workers on the internship programme</b>	.390 (.184)	.340 (.162)
<b>Share of workers of same nationality who can speak Russian</b>	.292 (.388)	.282 (.404)
<b>Share of workers of other nationality who can speak Russian</b>	.201 (.114)	.176 (.099)
<b>Mean days spent on the farm by the group of co-workers</b>	28.9 (10.81)	53.79 (14.49)

**Notes:** All variables apart from worker productivity are measured at the field-day level. Group size refers to the total number of pickers on the field-day. The cost of effort parameter,  $\theta$ , is calibrated for each worker. See the appendix for details of how this is calibrated. "Bottom tail" is the mean cost of effort among the worse 10% of workers (in terms of their cost of effort) on the field-day. The only significant differences in these characteristics across incentive schemes are for individual worker productivity, the share of workers on the field-day that are on the internship programme, and the mean days spent on the farm by the group of co-workers.

**Table 2: Group Characteristics - Group Size and Nationality**

**Dependent Variable = Log of worker's productivity (kilogram picked per hour per field-day)**

**Robust standard errors reported in parentheses, allowing for clustering at field-day level**

	<u>Relative Incentives</u>			<u>Piece Rates</u>		
	(1a)	(2a)	(3a)	(1b)	(2b)	(3b)
<b>Group size</b>	.233** (.109)			.064 (.067)		
<b>Number of workers of different nationality as worker <i>i</i></b>		.098 (.099)			.047 (.052)	
<b>Number of workers of same nationality as worker <i>i</i></b>		.136*** (.050)	.277** (.123)		.043 (.034)	.087 (.082)
<b>Share of total workers of same nationality as worker <i>i</i></b>			-.175* (.102)			-.038 (.065)
<b>Worker fixed effects</b>	Yes	Yes	Yes	Yes	Yes	Yes
<b>Field fixed effects</b>	Yes	Yes	Yes	Yes	Yes	Yes
<b>Other controls</b>	Yes	Yes	Yes	Yes	Yes	Yes
<b>Adjusted R-squared</b>	.4077	.4057	.4082	.3163	.3352	.3165
<b>Number of observations (worker-field-day)</b>	4063	4063	4063	6152	6152	6152

**Notes:** \*\*\* denotes significance at 1%, \*\* at 5%, and \* at 10%. Robust standard errors are calculated throughout, allowing for clustering at the field-day level. All continuous variables are in logs. The sample is restricted to workers who have worked at least 10 field-days under both incentive schemes. Other controls include worker experience, field life cycle, and a linear time trend.

**Table 3: Group Characteristics - Heterogeneity**

**Dependent Variable = Log of worker's productivity (kilogram picked per hour per field-day)**

**Robust standard errors reported in parentheses, allowing for clustering at field-day level**

	<u>Relative Incentives</u>			<u>Piece Rates</u>		
	(1a)	(2a)	(3a)	(1b)	(2a)	(3a)
<b>SD of cost of effort (sd(<math>\theta</math>))</b>	.561** (.252)			-.202 (.177)		
<b>In top 5% of ability distribution on field-day</b>		.226*** (.091)			-.035 (.056)	
<b>In bottom 5% of ability distribution on field-day</b>			-.009 (.088)			.019 (.058)
<b>Mean cost of effort (mean(<math>\theta</math>))</b>	.014 (.452)	.382 (.490)	.412 (.492)	-.328 (.347)	-.616*** (.201)	-.621*** (.199)
<b>Worker fixed effects</b>	Yes	Yes	Yes	Yes	Yes	Yes
<b>Field fixed effects</b>	Yes	Yes	Yes	Yes	Yes	Yes
<b>Other controls</b>	Yes	Yes	Yes	Yes	Yes	Yes
<b>Adjusted R-squared</b>	.4056	.3981	.3966	.3289	.3282	.3282
<b>Number of observations (worker-field-day)</b>	4062	4063	4063	6150	6152	6152

**Notes:** \*\*\* denotes significance at 1%, \*\* at 5%, and \* at 10%. Robust standard errors are calculated throughout, allowing for clustering at the field-day level. All continuous variables are in logs. The sample is restricted to workers who have worked at least 10 field-days under both incentive schemes. Other controls include worker experience, field life cycle, and a linear time trend.

**Table 4: Group Characteristics - The Internship Programme**

**Dependent Variable = Log of worker's productivity (kilogram picked per hour per field-day)**

**Robust standard errors reported in parentheses, allowing for clustering at field-day level**

	<u>Relative Incentives</u>			<u>Piece Rates</u>		
	(1)	(2a)	(3a) Not on internship	(2b)	(3b) Not on internship	(4) Arrived under piece rates, and not on internship
<b>Piece rate dummy (<math>P_t</math>)</b>	.564*** (.097)					
<b>Piece rate dummy x on internship programme</b>	-.212*** (.036)					
<b>Share of workers on the field-day that are on the internship programme</b>		.377*** (.106)	.521*** (.108)	-.021 (.118)	-.125 (.111)	.011 (.154)
<b>Worker fixed effects</b>	Yes	Yes	Yes	Yes	Yes	Yes
<b>Field fixed effects</b>	Yes	Yes	Yes	Yes	Yes	Yes
<b>Other controls</b>	Yes	Yes	Yes	Yes	Yes	Yes
<b>Adjusted R-squared</b>	.3974	.4278	.3858	.3035	.3423	.5794
<b>Number of observations (worker-field-day)</b>	4518	3984	1042	5913	1894	702

**Notes:** \*\*\* denotes significance at 1%, \*\* at 5%, and \* at 10%. Robust standard errors are calculated throughout, allowing for clustering at the field-day level. All continuous variables are in logs. The piece rate dummy is defined to be equal to zero when relative incentives are in place, and one when piece rates are in place. Other controls include worker experience, field life cycle, and a linear time trend. The samples in columns 1, 2a and 2b are restricted to workers who have worked at least 10 field-days under both incentive schemes. The samples in columns 3a, 3b and 4 only include workers that are not on the internship programme. The sample in column 4 is restricted to workers that arrive late in the season and only work under piece rates.

**Table 5: Monitoring**

**Dependent Variable = Log of worker's productivity (kilogram picked per hour per field-day)**  
**Robust standard errors reported in parentheses, allowing for clustering at field-day level**

**In columns 1 and 2, fruit type is NOT the same as in other tables - has physical characteristics that prevent monitoring of the performance of co-workers on the field-day.**

	<u>Fruit Type 2</u>		<u>Fruit Type 1</u>	<u>Fruit Types 1 and 2 Combined</u>
	(1) Baseline	(2) Twenty Days	(3) Baseline	(4) Diff-in-diff
<b>Piece rate dummy (<math>P_t</math>)</b>	-.063 (.129)	.098 (.196)	.483*** (.094)	-.106 (.095)
<b>Fruit type 1</b>				.060 (.273)
<b>Piece rate x fruit type 1</b>				.597*** (.112)
<b>Worker fixed effects</b>	Yes	Yes	Yes	Yes
<b>Field fixed effects</b>	Yes	Yes	Yes	Yes
<b>Other controls</b>	Yes	Yes	Yes	Yes
<b>Adjusted R-squared</b>	.3015	.4391	.3777	.6104
<b>Number of observations (worker-field-day)</b>	934	532	4224	5158

**Notes:** \*\*\* denotes significance at 1%, \*\* at 5%, and \* at 10%. Robust standard errors are calculated throughout, allowing for clustering at the field-day level in all columns. All continuous variables are in logs. The piece rate dummy is defined to be equal to zero when relative incentives are in place, and one when piece rates are in place. Other controls include worker picking experience, field life cycle, and a linear time trend. The sample in column 2 is restricted to ten days either side of the change in incentive schemes. The sample in column 3 is for these same workers, but when they pick fruit type 1. The sample in column 4 is for these workers combining the observations under fruit type 1 and fruit type 2.

**Table 6: Communication**

Dependent Variable = Log of worker's productivity (kilogram picked per hour per field-day)

Robust standard errors reported in parentheses, allowing for clustering at field-day level

	<u>Relative Incentives</u>				<u>Piece Rates</u>				
	(1a)	(2a) Non Russian speakers	(3a)	(4a) Non Russian speakers	(1b)	(2b) Non Russian speakers	(3b)	(4b) Non Russian speakers	(5) Arrived under piece rates
Share of workers of same nationality who can speak Russian	-.112* (.066)	-.133* (.070)	-.260** (.130)	-.273** (.135)	.007 (.070)	-.004 (.107)	-.019 (.130)	-.068 (.150)	-.066 (1.12)
Share of workers of other nationality who can speak Russian			-.206** (.087)	-.273*** (.093)			.117 (.084)	.062 (.081)	-.610 (1.52)
Share of same nationality who can speak Russian x share of other nationality who can speak Russian			-.104 (.072)	-.106 (.075)			-.013 (.047)	-.035 (.048)	-.270 (.670)
<b>Worker fixed effects</b>	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<b>Field fixed effects</b>	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<b>Other controls</b>	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<b>Adjusted R-squared</b>	.4233	.3669	.4312	.3774	.3027	.3067	.3030	.3057	.6625
<b>Number of observations (worker-field-day)</b>	2824	1446	2824	1446	3345	1775	3345	1775	447

**Notes:** \*\*\* denotes significance at 1%, \*\* at 5%, and \* at 10%. Robust standard errors are calculated throughout, allowing for clustering at the field-day level. All continuous variables are in logs. Other controls include worker experience, field life cycle, and a linear time trend. The sample is restricted to workers who have worked at least 10 field-days under both incentive schemes. The samples in columns 1a, 1b, 3a and 3b include all workers. The samples in columns 2a, 2b, 4a and 4b only include workers that cannot speak Russian. The sample in column 5 is restricted to workers that arrive late in the season and only work under piece rates.

**Table 7: Social Ties**

**Dependent Variable = Log of worker's productivity (kilogram picked per hour per field-day)**

**Robust standard errors reported in parentheses, allowing for clustering at field-day level**

	<u>Relative Incentives, First Week Picking</u>			<u>Piece Rates, First Week Picking</u>		
	(1a)	(2a)	(3a)	(1b)	(2b)	(3b)
<b>Late starter</b>	.122 (.128)			-.144 (.229)		
<b>Late starter x picking experience</b>		-.073 (.086)			-.062 (.163)	
<b>Mean days spent on the farm by the group of co-workers</b>			-.644** (.274)			.635** (.225)
<b>Worker fixed effects</b>	No	Yes	Yes	No	Yes	Yes
<b>Field fixed effects</b>	Yes	Yes	Yes	Yes	Yes	Yes
<b>Other controls</b>	Yes	Yes	Yes	Yes	Yes	Yes
<b>Week of arrival fixed effects</b>	Yes	No	No	Yes	No	No
<b>Adjusted R-squared</b>	.4369	.5003	.5328	.5072	.5689	.6026
<b>Number of observations (worker-field-day)</b>	1295	1295	1441	518	518	520

**Notes:** \*\*\* denotes significance at 1%, \*\* at 5%, and \* at 10%. Robust standard errors are calculated throughout, allowing for clustering at the field-day level. All continuous variables are in logs. In columns 1a, 2a, and 3a the sample is restricted to workers who have worked at least 10 field-days under both incentive schemes. Other controls include worker experience, field life cycle, and a linear time trend. In columns 1b, 2b, and 3b the sample only contains workers that arrived after the introduction of piece rates. In all columns the sample is restricted to the first seven days of picking for each worker. Late starter is a dummy equal to one if the worker started picking ten days or more after arrival and zero if the worker started picking one or two days after arrival. Workers that started picking between 3 and 9 nine days after arrival are dropped from the samples in columns 1a, 1b, 2a, and 2b.