SOCIAL PREFERENCES AND THE RESPONSE TO INCENTIVES: EVIDENCE FROM PERSONNEL DATA*

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We present evidence on whether workers have social preferences by comparing workers' productivity under relative incentives, where individual effort imposes a negative externality on others, with their productivity under piece rates, where it does not. We find that the productivity of the average worker is at least 50 percent higher under piece rates than under relative incentives. We show that this is due to workers partially internalizing the negative externality their effort imposes on others under relative incentives, especially when working alongside their friends. Under piece rates, the relationship among workers does not affect productivity. Further analysis reveals that workers internalize the externality only when they can monitor others and be monitored. This rules out pure altruism as the underlying motive of workers' behavior.

I. Introduction

This paper uses personnel data to present evidence on social preferences in the workplace, namely on whether workers internalize the effects of their behavior on their colleagues. While extensive evidence from experimental economics indicates that individuals take account of the effect of their actions on others in laboratory games, whether individuals exhibit social preferences in the workplace is largely unknown. The issue is of great practical relevance since workers' productivity under several incentive schemes, such as relative performance evaluation and team

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pay, depends crucially on whether and to what extent they internalize the effects of their actions on coworkers' payoffs.[1]

We use detailed personnel data from a leading farm in the United Kingdom that first paid its workforce according to a relative incentive scheme and then switched to piece rates. The workers' task is to pick fruit, and their individual productivity is recorded daily. To identify whether workers have social preferences, we compare their productivity under relative incentives to their productivity under piece rates. Under relative incentives, workers' daily pay depends on the ratio of individual productivity to average productivity among all coworkers on the same field and day. In contrast, under piece rates individual pay only depends on individual productivity.

The comparison is revealing because under relative incentives individual effort imposes a negative externality on coworkers' pay, whereas under piece rates individual effort has no effect on others' pay. The difference in workers' performance under the two schemes, if any, then provides evidence on whether and to what extent workers internalize the externality they impose on their colleagues.

It is important to stress that social preferences can be thought of as a reduced-form representation of either altruism or collusion. Namely, workers might internalize the externality either because they truly care about colleagues' payoffs, or because they fear punishment and retaliation. In this paper we first analyze whether workers internalize the externality their effort imposes on others, and then shed light on their underlying motives.

The empirical analysis proceeds in four stages. First, we identify the causal effect of the change in incentive schemes on workers' productivity. Second, we derive the empirical distribution of social preferences that is consistent with the observed change in productivity across incentive schemes. Third, we analyze whether the extent to which workers internalize the externality depends on the *relationship* between coworkers. To address this issue, we use data from a survey we administered to workers to collect precise information on their social network of friends on the farm. Fourth, we present evidence to distinguish between two

1. See Fehr and Gächter [2000a] for an overview of the experimental evidence on social preferences. That human relations matter in the workplace has long been noted in the sociology literature [Mayo 1933; Roy 1952], and the organizational behavior literature (see Williams and O'Reilly [1998] for a review).

hypotheses on why workers behave *as if* they have social preferences—pure altruism and collusion.

The data have four key features that help identify the causal effect of the change in incentive schemes on worker productivity, to attribute this change to workers' reaction to the externality created by the relative incentives scheme, and to investigate the motives behind the workers' behavior. First, we observe the daily productivity of the *same* workers before and after the introduction of piece rates. Time-invariant sources of unobservable individual heterogeneity are, therefore, controlled for.

Second, the same workers face an identical work environment throughout, except for the change in incentive schemes. In particular, there is no endogenous sorting of new workers into the sample and no endogenous attrition of workers out of the sample. Moreover, tasks, technology, management, and other farm practices were the same under both incentive schemes.

Third, the group of coworkers each individual works with, changes on a daily basis. This allows us to identify the effect of group composition on worker productivity from the comparison of the behavior of the same worker working alongside different coworkers.

Fourth, we observe a subsample of the same workers using an alternative technology that does not allow monitoring of coworkers. The ability to monitor coworkers creates differences in observed behavior depending on whether workers cooperate because of collusion or pure altruism. The comparison of workers' responses to the introduction of piece rates under the two monitoring scenarios then provides evidence to distinguish between collusion and pure altruism.

We find that the change in incentive scheme had a significant and permanent impact on productivity. For the average worker, productivity increased by at least 50 percent moving from relative incentives to piece rates. Calibration of the first-order conditions for worker's efforts reveals that the observed change in productivity is *too large* to be consistent with the assumption that workers ignore the negative externality they impose on others. At the same time, the observed change in productivity is also *too small* to be consistent with the assumption that workers maximize the welfare of the group and fully internalize the negative externality.

We, therefore, posit that workers place some weight on the payoffs accruing to their coworkers and derive the distribution of social preferences among workers that fits the observed change in productivity. We find that this is consistent with the average worker placing a weight of .65 on the benefits accruing to all other coworkers, assuming that they place a weight of one on their own benefits. Further analysis reveals that under relative incentives workers internalize the externality more when the share of their personal friends in the group is larger and this effect is stronger in smaller groups. In line with the interpretation that social preferences explain the difference in productivity across the two schemes, we find that the relationship among workers *does not* affect productivity under piece rates.

Finally, we find that productivity under relative incentives was significantly lower only when workers were able to monitor each other. Given that monitoring is necessary to enforce collusion while it does not affect altruism, we take this finding to support the hypothesis that workers are able to sustain implicit collusive agreements when relative incentives are in place.

Overall, our findings imply that worker productivity is significantly different under the two schemes because workers internalize the externality they impose on others under relative incentives, and they do so more when they work alongside friends. Importantly, workers internalize the externality only when they are able to monitor each other, which rules out pure altruism as the underlying cause of workers' behavior.

The results demonstrate the importance of understanding how workers behave in the presence of externalities on coworkers, when designing incentive schemes. For instance, the results 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 that they lower productivity when workers behave as if they have social preferences.

The paper contributes to two strands of the literature. First, we contribute to the literature on incentive schemes and workers' productivity [Prendergast 1999].[2] We complement existing re-

2. Knoeber and Thurman [1994] analyze the effects of two different relative incentive schemes on chicken ranchers. The predictions of rank order tournament theory, a type of relative incentive, have been examined in experimental data [Bull, Schotter, and Weigelt 1987] and sports tournaments [Ehrenberg and Bognanno 1990; Becker and Huselid 1992]. Lazear [2000], Paarsch and Shearer [1996], and Shearer [2004], find sizable productivity gains moving from fixed pay

sults by providing the first evidence on the comparison between relative incentives and piece rates. Our work is also closely related to Hamilton, Nickerson, and Owan [2003], who present evidence suggesting that productivity is higher under team pay, where workers' effort imposes a *positive* externality on others, than under piece rates. Consistent with their results, we find that productivity is lower under relative incentives where the externality is *negative*.

Second, our results complement the large experimental literature on social preferences, showing that these matter outside the laboratory. In particular, our findings are in line with the experimental literature on public good games where individual contributions are generally found to be halfway between the selfish Nash equilibrium (complete free riding) and the group optimum [Ledyard 1995; Fehr and Gächter 2000b].

The paper is organized as follows. Section II models the workers' effort choice under relative incentives and piece rates. Section III describes the data. Section IV presents reduced-form estimates of the effect of the change in incentives on productivity. Section V brings alternative structural models of workers' behavior to the data and derives the distribution of social preferences that are consistent with the observed change in productivity. Section VI analyzes the effect of the identity of coworkers on productivity under the two incentive schemes. Section VII provides evidence to distinguish between pure altruism and collusion. Section VIII concludes with a discussion on the design of incentive schemes when workers have social preferences.

II. Theoretical Framework

This section makes precise how social preferences affect workers' effort choice under relative incentives and under piece rates. Theory suggests that relative schemes have the advantage of differencing out common shocks to productivity and could, therefore, yield a higher payoff for the principal when such shocks are important [Lazear and Rosen 1981; Green and Stokey 1983;

to piece rate incentives. Similarly, Laffont and Matoussi [1995] find worker productivity to be 50 percent higher in farms operated under fixed rent contracts compared with those under sharecropping contracts. Foster and Rosenzweig [1994] show that effort, proxied by the depletion of body mass net of calorie intake, is 22 percent higher for rural laborers paid by piece rates compared with those paid hourly wages.

Nalebuff and Stiglitz 1983]. This literature, however, typically assumes that workers ignore the externality their effort imposes on others under relative incentives. The model that follows shows how, other things equal, a worker's response depends on whether she has social preferences. Therefore, the empirical comparison of productivity under the two schemes can provide evidence on whether social preferences are relevant in practice.

Consider a group of N workers. Each worker i exerts $e_i \geq 0$ units of effort which determines her productivity. Without loss of generality, we assume that effort equals productivity in what follows. Each worker's payoff is $\phi(\cdot) - \theta_i e_i^2/2$, where $\phi(\cdot)$ is the benefit derived from pay (which depends on effort), and $\theta_i e_i^2/2$ is the cost of effort. We assume that $\phi(\cdot)$ is a differentiable concave function, with $\lim_{x \to 0} \phi'(x) = \infty$. The parameter θ_i is interpreted as the inverse of the workers innate ability. We assume that workers are heterogeneous along this dimension, and can be ordered such that $\theta_1 < \theta_2 < \ldots < \theta_N$, where $\theta_i > 0$ for all i. The relationship between effort and pay depends on the incentive scheme as explained below.

II.A. Relative Incentives

Under relative incentives, a worker's pay depends on how she performs relative to her peers. More specifically, in our setting, workers' benefit from pay takes the form $\phi(e_i/\bar{e})$ for all i, where $\bar{e} = (1/N)\sum_i e_i$ is the average effort of all N workers.[3] The relative scheme has the key characteristics that other things equal, an increase in worker i's effort (i) increases her pay; and (ii) increases average effort and, hence, imposes a negative externality by reducing the pay of everybody else in the group.

The choice of effort under relative incentives then depends on whether and to what extent workers internalize this externality, namely on whether they have social preferences. Denote by π_i the weight that worker i places on her coworker's payoffs. The equilibrium effort for worker i solves

$$\max_{e_i} \phi\left(\frac{e_i}{\bar{e}}\right) + \pi_i \sum_{j \neq i} \left(\phi\left(\frac{e_j}{\bar{e}}\right) - \frac{\theta_j e_j^2}{2}\right) - \frac{\theta_i e_i^2}{2}. \quad (1)$$

3. This relative incentive scheme is not a rank order tournament. Worker benefits are based on their cardinal and not their ordinal ranking. It is, however, similar to a "linear relative performance evaluation" (LRPE) scheme as studied in Knoeber and Thurman [1994]. Under a LRPE worker's compensation is, $\alpha + \mu(e_i - \bar{e})$, where α and μ are parameters taken as given by workers.

Such social preferences can be thought of as a reduced-form representation of a number of models. They depict behavior consistent with reciprocity or altruism [Fehr and Schmidt 1999], or the evolutionary equilibrium of a repeated Prisoner's Dilemma game in which workers learn which strategies to play [Levine and Pesendorfer 2000; Sethi and Somanathan 1999]. In Section VII we present some evidence to distinguish between models in which workers' preferences display altruism toward others, and models in which workers behave *as if* they are altruistic because, for instance, they play trigger strategies to enforce implicit collusive agreements.

Assuming that worker i chooses her effort taking the effort of others as given, the Nash equilibrium effort of worker i solves

$$\phi'\left(\frac{e_i}{\bar{e}}\right)\frac{1}{\bar{e}}\left(\frac{\sum_{j\neq i} e_j}{(N\bar{e})}\right) - \frac{\pi_i}{\bar{e}}\sum_{j\neq i}\phi'\left(\frac{e_j}{\bar{e}}\right)\frac{e_j}{(N\bar{e})} = \theta_i e_i. \tag{2}$$

II.B. Piece Rates

Under piece rates, individual effort is paid at a fixed rate β per unit. With social preferences, worker i chooses her effort under piece rates as follows:

$$\max_{e_i} \phi(\beta e_i) + \pi_i \sum_{j\neq i}\left(\phi(\beta e_j) - \frac{\theta_j e_j^2}{2}\right) - \frac{\theta_i e_i^2}{2}. \tag{3}$$

The equilibrium effort level solves the first-order condition,

$$\phi'(\beta e_i)\beta = \theta_i e_i. \tag{4}$$

Naturally, as worker i's effort does not affect the pay of her coworkers, her optimal choice of effort is independent of the social weight she places on others.

II.C. Comparing Relative Incentives and Piece Rates

To compare effort choices under the relative scheme and under piece rates, we evaluate the first-order condition (4) at $\beta = 1/\bar{e}$ so that for a given average effort level, the pay per unit of effort is the same under both incentive schemes. The first-order condition under piece rates then is

$$\phi'\left(\frac{e_i}{\bar{e}}\right)\frac{1}{\bar{e}} = \theta_i e_i. \tag{5}$$

The difference between the first-order conditions (2) and (5) can then be ascribed to two sources. The first and most important difference is the externality worker i imposes on others under relative incentives. By increasing her effort, she also increases the average effort, and this reduces the pay of coworkers other things equal. The magnitude of this effect depends on the extent to which workers internalize the externality, that is, on the social weight π_i. This is captured in the second term on the left-hand side of (2). When $\pi_i > 0$, worker i's productivity is *lower* under relative incentives compared with that under piece rates. Second, by exerting more effort, each worker lowers the pay she receives for each unit of effort under relative incentives. This effect, captured by the $\sum_{j \neq i} e_j/(N\bar{e})$ term, also reduces productivity under relative incentives but is negligible in large groups.[4]

This highlights that the difference between productivity under relative incentives and piece rates depends on the extent to which workers have social preferences and so internalize the externality they impose on others. This difference is minimized when $\pi_i = 0$ for all i, namely when workers do not take into account the effect of their actions on others. In this case, effort levels under the two schemes are, thus, almost identical for large N.

In contrast, when $\pi_i = 1$ for all i, the first-order condition under relative incentives (2) coincides with the first-order condition of the social optimum among workers; that is, when effort levels are chosen to maximize the welfare of the group as a whole. In the remainder of the paper we establish whether productivity is indeed different under the two schemes and then derive implications for the workers' underlying preferences.

III. Context and Data Description

III.A. Context

We analyze personnel data from a leading United Kingdom-based fruit farm for the 2002 season. Workers in the sample are hired seasonally to pick fruit across a number of fields on the farm. They are paid according to a relative incentive scheme for

4. This is seen most clearly in the case of homogeneous workers. Then the Nash equilibrium effort level under relative incentives is $e_i^* = e^R = ((1 - 1/N)\phi'(1))^{1/2}$ and $e_i^* = e^P = (\phi'(1))^{1/2}$ under piece rates. The ratio of effort under the two systems is, thus, $(1 - 1/N)^{1/2}$. If workers are heterogeneous, the ratio depends on group size and workers' ability, although it can be shown that $\lim_{N\to\infty} e^R = e^P$.

the first half of the season and according to piece rates for the second half. In both cases workers face a compensation schedule of the form,

$$\text{compensation} = \beta K_i,$$

where K_i is the total kilograms of fruit picked by worker i on the field-day. Throughout we define individual productivity y_i as the number of kilograms of fruit picked per hour.[5]

Under the relative scheme, the unit wage β is *endogenously* determined by the average productivity of all workers in the same field on the same day. In particular, β is set according to

$$\beta = \bar{w}/\bar{y}, \tag{6}$$

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 on the field-day. At the start of each field-day, the field manager announces an ex ante unit wage based on her expectation of worker productivity. This unit wage is revised at the end of each field-day to ensure that a worker with productivity $\bar{y}$ earns the preestablished hourly wage $\bar{w}$.

In line with the relative scheme analyzed in Section II, worker i's compensation depends on her productivity relative to the average productivity of her coworkers. In particular, given $K_i = y_i h$, where h is the number of hours worked in a field, worker i's pay is $(y_i/\bar{y})h\bar{w}$. Note that an increase in worker i's effort increases the average productivity on the field-day and, thus, imposes a negative externality on her coworkers by reducing the unit wage β in (6).

Under piece rates, the unit wage is set ex ante, based on the manager's assessment of productivity that field-day, and is not revised. The manager aims to set the unit wage at the level such that the worker with average productivity receives the minimum wage plus a fixed positive constant. The data under piece rates indicate that the rate was set correctly in the sense that the unit

5. To comply with minimum wage laws, workers' compensation is supplemented whenever βK_i falls below the pro-rata minimum wage. In practice, the farm management makes clear that any worker who needs to have her compensation increased to the minimum wage level repeatedly would be fired. Indeed, we observe less than 1 percent of all worker-field-day observations involving pay increases to meet the minimum wage requirements. Of these, 46 percent occurred under relative incentives, 54 percent occurred under piece rates.

wage set ex ante is very close to the unit wage that would obtain ex post if the relative formula in (6) were used instead.[6]

The key difference between the two systems is that under the relative incentives, workers' effort on the day determines the unit wage for each kilogram picked on that field-day. Under piece rates, workers' effort does not affect the unit wage.[7]

Workers in the sample are hired on a casual basis, namely work is offered daily with no guarantee of further employment. The majority of workers hired are from Eastern and Central Europe and live together on the farm for the duration of their stay.[8] Workers are issued with a farm-specific work permit for a maximum of six months, implying that they cannot be legally employed elsewhere in the United Kingdom. Their outside option is, therefore, to return to their home countries. The vast majority of workers in the sample report their main reason to seek temporary employment in the United Kingdom to be financial, which is hardly surprising in light of the fact that wages are much higher in the United Kingdom than in their home countries.[9]

We analyze productivity data on one type of fruit only and focus on the season's peak time—between mid-May and the end of August. Fruit plants are lined up in rows, and each worker is assigned one or more adjacent rows to pick. The productivity of each worker depends exclusively on her effort and on the amount of fruit available on her rows, namely workers' efforts are not complements in production.

Data on workers' productivity are recorded electronically.

6. Hence, under both incentive schemes the unit wage in part depends on the field life cycle, field conditions, and other determinants of expected productivity. The unit wage is, therefore, mechanically negatively related to productivity. We do not observe exogenous changes in the unit wage that allow us to estimate the wage elasticity of labor supply for example.

7. Workers face more uncertainty over the unit wage under relative incentives because although a rate is announced ex ante, this can be revised ex post to reflect the productivity of the average worker. Under piece rates, the ex ante unit wage cannot be revised. However, uncertainty is unlikely to have a large impact on effort choices because workers play the same game daily and have many opportunities to learn the ex post adjustment of the unit wage under relative incentives. In Section IV we further discuss whether uncertainty can explain the change in productivity we observe in the data.

8. In order to qualify, individuals must be full-time students, and have at least one year before graduation. Workers must (i) return to the same university in the autumn, (ii) be able to speak English, (iii) have not worked in the United Kingdom before, and (iv) be aged between 19 and 25.

9. As of January 2003, gross monthly earnings at the United Kingdom minimum wage (Euro 1105) are five times as high as at the minimum wage in Poland (Euro 201), where the majority of workers come from, and almost twenty times higher than in Bulgaria (Euro 56), the poorest country in our sample.

Each worker is assigned a unique bar code, which is used to track the quantity of fruit they pick on each field and day in which they work. This ensures little or no measurement error in recorded productivity. The sample is restricted to those workers who worked at least ten field-days under each incentive scheme. Our working sample contains 10,215 worker-field-day level observations, covering 142 workers, 22 fields, and 108 days in total.

The compensation scheme changed from relative incentives to piece rates for all workers midway through the season. 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 announced on the same day it was first implemented. No other organizational change took place during the season, as reported by farm management and as documented in the next subsection.

Finally, interviews with management revealed that the relative incentive scheme was adopted because it allowed them to difference out common productivity shocks, such as those derived from weather and field conditions, that are a key determinant of productivity in this setting. The management eventually decided to move to piece rates because productivity had been lower than they initially expected at the start of the season. Whether the move to piece rates had the desired effect is analyzed in Section IV.

III.B. Descriptive Analysis

Table I provides information on unconditional worker productivity by incentive scheme. Productivity rose significantly from an average of 5.01 kg/hr in the first half of the picking season under relative incentives to 7.98 kg/hr in the second half of the season under piece rates, an unconditional increase of 59 percent.

Figures I and II show disaggregated productivity data across time and across workers under the two schemes. Figure I 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.

Figure II shows kernel density estimates of individual productivity by each incentive scheme. The productivity of each of

TABLE I
UNCONDITIONAL DIFFERENCES IN PRODUCTIVITY AND OTHER VARIABLES
MEAN, STANDARD ERRORS IN PARENTHESES, AND CONFIDENCE INTERVAL IN BRACKETS

	Relative incentives	Piece rates	Difference
Worker productivity (kg/hr)	5.01	7.98	
	(.243)	(.208)	2.97***
	[4.53, 5.49]	[7.57, 8.39]	
Kilos picked per day	Confidential		23.2***
Hours worked per day	Confidential		−.475
Number of workers in same field	41.1	38.1	−3.11
	(2.38)	(1.29)	
Daily pay	Confidential		1.80
Unit wage per kilogram picked	Confidential		−.105***

*** denotes significance at 1 percent. Sample sizes are the same as those used for the productivity regressions. Standard errors and confidence intervals take account of the observations being clustered by field-day. Productivity is measured in kilograms per hour. Daily pay refers to pay from picking only. Both daily pay and the unit wage per kilogram picked are measured in UK Pounds Sterling. Some information in the table cannot be shown due to confidentiality requirements.

the 142 workers in the sample is averaged within each incentive scheme in this figure. The mean and variance of productivity both rise moving from relative incentives to piece rates.

The second and third rows of Table I reveal that the increase

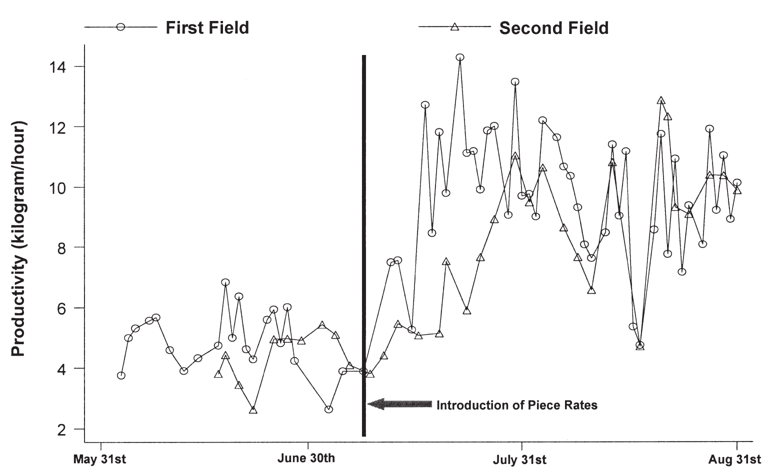

FIGURE I
Productivity (kilogram/hour) over the Season

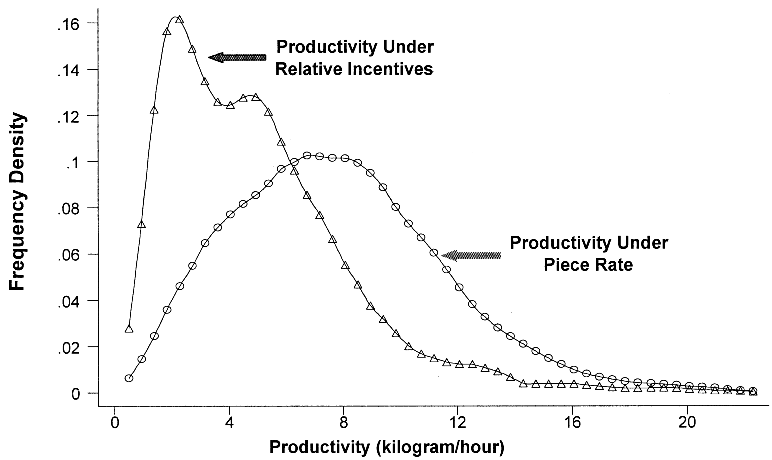

FIGURE II
Distribution of Productivity (kg/hr) by Incentive Scheme

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 percent level. Hours worked did not significantly change across incentive schemes.

The discussion in Section II makes clear that the size of the group over which relative pay is computed is key for understanding workers' behavior under relative incentives. The fourth row of Table I reports the average number of people each worker worked with on a given field-day. This remained constant throughout the season. The fact that under relative incentives, the unit wage depends on the average productivity of 40 workers has two implications. First, the effect each worker has on her own pay is negligible, and so if workers do have social preferences there should be no difference in their productivity under the two incentive schemes. Second, by exerting effort, the worker reduces the pay of many coworkers under relative incentives. Hence, if workers behave as if they have social preferences, there may be a large divergence in their productivity between when they are paid according to a relative incentive scheme and piece rates.

The analysis of aggregate farm level data reveals that the change in incentive scheme did not coincide with a wave of new arrivals, nor did it hasten the departure of workers. Indeed, very few workers left before or just after the change in incentive schemes.[10]

Second, we find that the total kilograms picked per day shows no discernible trend under either incentive scheme, a consequence of the deliberate timing of planting of fields to ensure a constant stream of fruit throughout the season. Third, the total man-hours spent picking are higher under relative incentives. This is due entirely to more workers picking, rather than each worker picking for longer hours. Under piece rates the total man-hours spent picking falls as fewer workers are required to pick each day.

Overall, while total kilograms picked and the time spent picking per field-day remained constant throughout the season, the total number of workers allocated to picking fell moving from relative incentives to piece rates. Under piece rates, the management had some workers pick less frequently and instead had them perform other tasks, mostly related to the transportation and packaging of fruit. These workers had the same productivity as workers who continued regularly on picking tasks. They also did not differ on characteristics such as gender and nationality.

Figure IIIa shows the wage paid per kilogram over time—the unit wage β, in percentage deviation from its mean.[11] Under relative incentives the unit wage rises gradually as productivity declines. This is as expected given that under the relative incentive scheme, the unit wage is set endogenously according to (6).

With the introduction of piece rates there is a one-off fall in the unit wage. Table I shows that the difference in average unit wages between the two halves of the season is significant at the 1 percent level. It is, therefore, unlikely that the observed rise in productivity is a consequence of higher returns to the marginal unit of effort under piece rates. To the contrary, the pay per unit of effort is lower under piece rates.

Figure IIIb then shows the daily pay from picking over the season, as a percentage deviation from its mean. Given that productivity and unit wages are inversely related to each other, average workers' pay remained relatively constant over time. Table I shows that the difference in average daily pay between

10. This and other results not reported here are available in a companion working paper [Bandiera, Barankay, and Rasul 2004].

11. Due to confidentiality requirements we cannot show this series in levels.

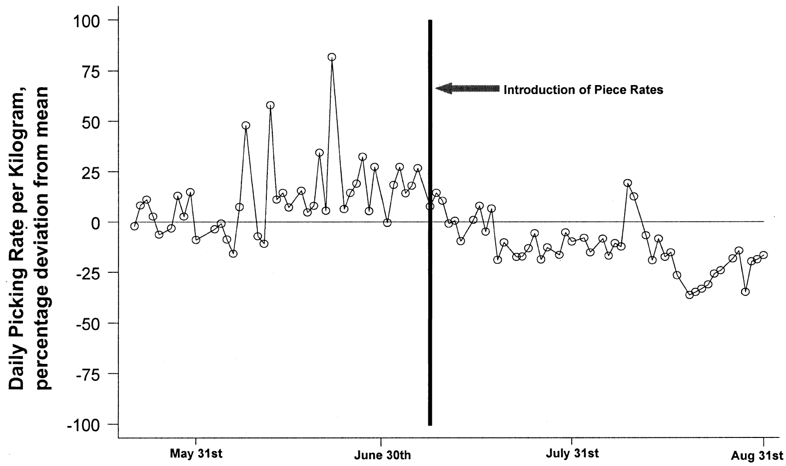

FIGURE IIIa
Unit Wage over the Season

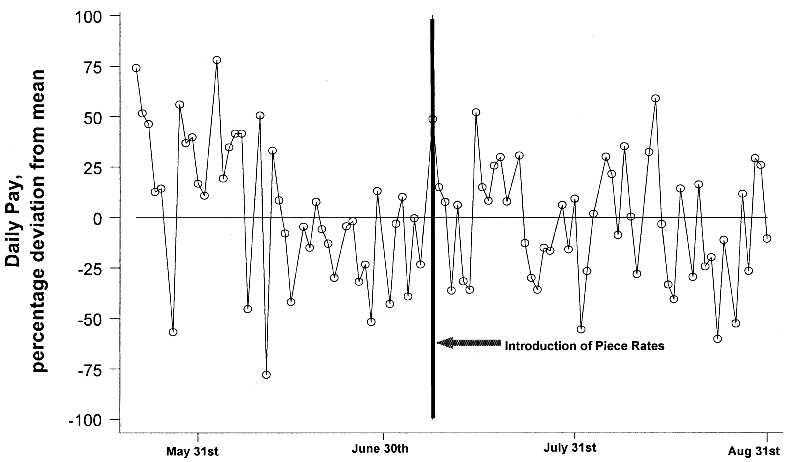

FIGURE IIIb
Daily Pay over the Season

Sample sizes are the same as those used for the productivity regressions. The series for the daily wage is an average over all fields operated on each day. This average is weighted by the number of man-hours on each field-day. The series for daily pay is averaged over all workers each day. This average is weighted by the hours worked per worker on each day.

relative incentives and piece rate is positive but not significantly different from zero. Overall, the average worker became worse off under piece rates: their productivity rose, while total compensation did not increase significantly. The regression estimates in the next section, therefore, most likely provide a lower bound of the effect of the change in incentives on productivity holding *utility* constant.[12] However, the top third of workers did have significant increases in pay moving to piece rates (not reported), which is as expected if workers are of heterogeneous ability.

The empirical analysis proceeds in four stages. Section IV presents reduced-form evidence on whether the change in incentive schemes causes the increase in productivity. Section V presents structural-form estimates on workers' social preferences that are consistent with the observed change in productivity. Section VI provides reduced-form evidence on how the relationship among coworkers affects individual productivity under the two incentive schemes. Section VII provides evidence to distinguish between pure altruism and collusion as motives behind workers' behavior.

IV. Reduced-Form Evidence of the Response to Incentives

IV.A. Empirical Method

We first identify the effect of the change in incentives on individual worker productivity. 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:

$$y_{ift} = \alpha_i + \lambda_f + \gamma P_t + \delta X_{ift} + \eta Z_{ft} + \kappa t + u_{ift}. \tag{7}$$

Worker fixed effects α_i capture time-invariant, worker-level determinants of productivity such as innate ability and intrinsic motivation. Field fixed effects λ_f capture time-invariant, field-level determinants of productivity such as soil quality and plant spacing. P_t is a dummy equal to one when piece rates are in place and zero when relative incentives are in place. The parameter of interest throughout is γ, namely the effect of the move from relative incentives to piece rates on individual productivity.

As piece rates are introduced simultaneously across all fields, it is not possible to control for day fixed effects. Instead, we

12. We maintain the standard assumption in the incentives literature that the utility-maximizing level of effort is increasing in the piece rate. We discuss whether income effects or income targeting can explain the observed increase in productivity in Section IV.

control for time-varying factors at both the individual (X_{ift}) and field (Z_{ft}) level and for a farm level trend t.

The disturbance term u_{ift} captures unobservable 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. We account for this by clustering standard errors at the field-day level in all productivity regressions.[13]

IV.B. Baseline Results

Table II presents the baseline estimates of the effect of the change in incentive scheme on worker productivity. Column (1) regresses worker productivity on a dummy for the introduction of piece rates, clustering standard errors by field-day. Productivity rises significantly by 70 percent when moving from relative incentives to piece rates.

Column (2) controls for worker fixed effects, so that only variation within a worker over time is exploited, while column (3) additionally controls for field fixed effects, so only variation within a worker picking on the same field over time is exploited. Controlling for worker heterogeneity improves the fit of the model considerably—worker fixed effects almost double the explained variation in productivity. In contrast, field heterogeneity appears to be much less important. The estimated effect of the change in incentives on individual productivity remains significant and of similar magnitude as in column (1).

Column (4) controls for other time-varying determinants of productivity at the level of the farm, field, and individual. We include a linear time trend to capture farm level changes over time, a measure of each field's life cycle to capture field level changes over time and a measure of each worker's picking experience. We measure the field's life cycle as the number of calendar days the field has been picked at any moment in time, divided by the total number of days the field is picked over the season. Each field is picked for a predetermined number of days that depends on the number and the age of the plants, which were planted earlier in the season or in the previous years. The field's life cycle

13. We also allowed observations to be clustered at the work level and at the worker-incentive scheme level to account for idiosyncratic worker characteristics that lead to worker productivity over different field-days being correlated. Doing so caused the estimated standard errors to fall considerably.

TABLE II
THE EFFECT OF THE CHANGE IN INCENTIVES ON INDIVIDUAL PRODUCTIVITY
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

	(1) Unconditional	(2) Worker heterogeneity	(3) Field heterogeneity	(4) Controls
Piece rate dummy (P_t)	.530***	.515***	.460***	.577***
	(.059)	(.056)	(.070)	(.098)
Time trend				.004
				(.003)
Field life cycle				−1.16***
				(.362)
Worker experience				.077***
				(.031)
Worker fixed effects	No	Yes	Yes	Yes
Field fixed effects	No	No	Yes	Yes
Adjusted R^2	.1607	.2925	.3407	.3640
Number of observations (worker-field-day)	10215	10215	10215	10215

*** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Standard errors are clustered at the field-day level. All continuous variables are in logs. The piece rate dummy is set equal to zero when relative incentives are in place, and set equal to one when piece rates are in place. The sample is restricted to workers who have worked at least ten days under both incentive schemes. The field life cycle is defined as the number of days the field has been operated at any moment in time, divided by the total number of days the field is operated over the season. Worker experience is defined as the number of field-days the worker has picked for. There are 142 workers, 22 fields, and 108 days in the sample.

variable, thus, captures the natural trend in productivity that occurs within each field as it depletes over time.

Picking experience is defined as the number of field-days the worker has picked for. To identify the effect of the field's life cycle and the worker's picking experience from the general farm trend, we use the variation arising from the fact that different fields start being picked at different points in time, that fields are not picked every day, and that different workers arrive on different dates.

We find that there is no trend in productivity over time at the level of the farm. This is consistent with the fact that different fields are operated at different times to ensure a constant stream of output throughout the season. Within each field, however, productivity declines as the field is picked later in its cycle.

Moreover, there are positive returns to picking experience as expected.[14]

The results confirm the quantitative importance of the change in incentives on productivity. A one standard deviation increase in the field life cycle reduces productivity by 20 percent, while a one standard deviation increase in picking experience increases productivity by 7 percent. In comparison, the move from relative incentives to piece rates significantly increases productivity by 78 percent.

Further analysis, not reported for reasons of space, shows that this result is robust to controlling for other time-varying factors including contemporaneous and lagged weather conditions, field supervisor fixed effects, and the ratio of supervisors to workers.[15]

IV.C. Robustness Checks

As the change in incentives occurs at the same time in all fields, identification of the effect of this change on productivity arises from a comparison over time of the same worker. The estimated effect $\hat{\gamma}$ is then biased upward to the extent that it captures factors that cause productivity to rise through the season regardless of the change in incentive schemes and that are not captured by the farm level trend or the field-specific life cycle.

Table III presents a series of robustness checks to precisely address this concern. First, we augment the sample by adding worker-field-day observations from the same farm in 2004 when workers were paid piece rates throughout. We restrict the sample to workers who picked at least ten days before and after July 8. The 2004 sample, thus, consists of 55 workers, 18 fields, and 3664 worker-field-day observations. We estimate the productivity of worker i on field f on day t, y_{ift}, using the following panel data regression:

14. To check robustness to functional form specifications, we also ran the regression in log-linear form (i.e., log productivity on the levels of picking experience, field cycle, and trend) and in log-quadratic form (i.e., log productivity on the levels and the squares of picking experience, field cycle, and trend). Finally, we also allowed for field-specific trends. The estimated effect of the introduction of piece rates is robust to these alternative specifications. These results are not reported for reasons of space and are available from the authors on request.

15. Each supervisor is assigned a group of 15 to 30 workers. The supervisor is primarily responsible for ensuring that fruit is taken from the field for packaging and preventing bottlenecks in production. Supervisors are paid a fixed wage throughout the season.

TABLE III
THE EFFECT OF THE CHANGE IN INCENTIVES ON INDIVIDUAL PRODUCTIVITY-ROBUSTNESS CHECKS
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

	(1) Difference in difference with 2004 season	(2) Placebo piece rate: fields	(3) Placebo piece rate: workers	(4) Twenty days
Piece rate dummy	.577***			.387***
(2002)	(.098)			(.110)
Placebo piece rate	.096			
dummy (2004)	(.086)			
Placebo piece rate based		.156		
on field life cycle		(.196)		
Placebo piece rate based			−.009	
on number of days			(0.91)	
present on the farm				
Worker fixed effects	Yes	Yes	Yes	Yes
Field fixed effects	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	No
Adjusted R^2	.4149	.4927	.5921	.2922
Number of observations (worker-field-day)	13879	2863	879	2969

*** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Standard errors are clustered at the field-day level. All continuous variables are in logs. The piece rate dummy is set equal to zero when relative incentives are in place, and set equal to 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 (1) covers workers who have worked at least ten days under both incentive schemes in 2002 and workers who have worked at least ten days either side of July 8 in 2004. Control variables are year specific. The sample in column (2) covers workers who have worked at least ten days under both incentive schemes and fields that have been operated exclusively under one incentive scheme. The sample in column (3) covers workers who have arrived at the farm after the introduction of piece rates. The sample in column (4) is restricted to the last ten days under relative incentives and the first ten days under piece rates.

$$(8)\quad y_{ift} = \alpha_i + \lambda_f + \sum_{s=1}^{2} \gamma_s P_t d_t^s + \sum_{s=1}^{2} \delta_s X_{ift} d_t^s + \sum_{s=1}^{2} \eta_s Z_{ft} d_t^s + \sum_{s=1}^{2} \kappa_s t d_t^s + u_{ift},$$

where $s \in \{1,2\}$ identifies the season and $d_t^1 = 1$ for the 2002 season and 0 otherwise, while $d_t^2 = 1$ for the 2004 season and 0 otherwise. We define a placebo piece rate dummy P_t for the 2004 season to be equal to one after July 8; that is, when piece rates were introduced in 2002. Thus, γ_1 measures the effect of the introduction of piece rates in 2002 on individual productivity and γ_2 measures the effect of the placebo piece rate in 2004. As in the previous specification we include workers' fixed effects α_i, fields fixed effects λ_f, workers' experience X_{ift}, the field life cycle Z_{ft}, and a farm level trend, allowing these variables to be different in the two seasons.[16]

The difference-in-difference estimates in column (1) indicate that the previous result was not due to seasonality: the placebo dummy for piece rates for the 2004 season has no effect on productivity. Interestingly, we also find that the estimated coefficients of the farm level variables (trend and field life cycle) are the same in the two seasons. In contrast, returns to experience are twice as large in 2004, in line with workers holding back effort under relative incentives in 2002.

The second and third tests simulate the introduction of piece rates in fields and for workers that did not actually experience the change in incentive schemes in 2002.

For fields, we note that the two main fields operated most frequently under both incentive schemes, experienced the change in incentive scheme at one-quarter of their life cycle. We construct a placebo piece rate dummy for each field, set equal to one after a field has passed 25 percent of its life cycle and zero otherwise. We then examine whether this placebo dummy affects the productivity of the sample of fields that are only operated under *either* relative incentives or piece rates. The result in column (2) shows no evidence of a natural jump in productivity on fields after they pass 25 percent of their life cycle.

Column (3) exploits the same idea at the worker level. In the baseline sample, workers had been picking for an average of nineteen days before the change in incentives. For the placebo test we exploit information on workers who arrived *after* the introduction of piece rates. We create a placebo piece rate dummy for each such worker set equal to one after that worker has been picking for nineteen days. The result in column (3) shows no

16. While some of the fields observed in 2004 are the same as those in 2002, we allow the fixed effect to be different to capture the fact that the plants are at a different stage of their life in the two years. None of the workers is present in both seasons.

evidence of the natural jump in worker productivity at this level of picking experience.

Finally, in column (4) we restrict the time window to ten days before and after the introduction of piece rates to eliminate the effect of natural long-term changes in productivity. This specification yields our lowest estimate of $\hat{\gamma}$. The effect is nevertheless large: for the average worker, productivity was 47 percent higher during the ten days after the introduction of piece rates compared with that during the last ten days under relative incentives.[17]

Further analysis indicates that results are robust to the possible selection of different workers into picking over time, and to controlling for changes in task composition in the two halves of the season. We also find that the introduction of piece rates was not anticipated in the sense that productivity did not significantly change in the week prior to the introduction of piece rates, that workers reacted slowly to the change (possibly in the hope of reinstating the relative incentive scheme), and that the quantitative effect of the move to piece rates on productivity lasted throughout the season.[18]

A remaining issue is whether the increase in productivity came at the expense of the *quality* of fruit picked. Pickers are expected to classify fruit as either class 1—suitable as supermarket produce, or class 2—suitable as market produce. This is especially pertinent in this context because, due to technological restrictions, misclassifications of fruit *cannot* be traced back to individual workers. To check for this, we analyze whether the misclassification of fruit worsened after the introduction of piece rates. Results, reported in Appendix 2 show that this was not the case.

IV.D. Income Targeting and Other Hypotheses

Taken together, the results show that moving from a relative incentive scheme to piece rates significantly increased worker productivity by at least 50 percent. As workers' pay remained constant on average under both incentive schemes while productivity increased, this estimated increase in productivity is most likely a lower bound on the pure effect of the change in incentives, holding worker *utility* constant.

17. Given the shorter time frame, we do not include other time-varying controls in this specification.

18. These robustness checks are discussed in detail in Bandiera, Barankay, and Rasul [2004].

We now discuss alternative explanations for the change in workers' behavior from relative incentives to piece rates. While the negative externality is the most evident source of difference between the two schemes, workers' behavior might possibly reflect income targeting, their reaction to uncertainty, and ratchet concerns. We address these below.

First, as discussed in Section III, the unit wage per kilogram decreased by 12 percent moving from relative incentives to piece rates. If workers adjust their effort to reach a constant daily income target, the fall in the unit wage may cause the observed increase in productivity. To judge the empirical relevance of income targeting in our context, it is important to stress that workers cannot choose the number of hours they work, implying that the standard income-leisure trade-off does not arise. In other words, workers can adjust only on the intensive margin, for instance by choosing to work harder when the unit wage is low, to achieve their income target.[19]

Three pieces of evidence cast doubt on the empirical relevance of income targeting in this setting. First, we find that workers who face higher piece rates work harder. To establish this, we exploit the fact that the *real* value of piece rates varies exogenously among workers who come from countries with different levels of GDP per capita, and that workers save most of their earnings to bring back to their home countries. Although all workers come from Eastern Europe, there are large cross-country differences. For instance, gross monthly earnings at the minimum wage are four times higher in Poland (euro 210) compared with Bulgaria (euro 56). In line with the assumption that more high powered incentives result in higher effort, we find workers who come from poorer countries have higher productivity, all else equal. To the extent that the worker pools from different countries are not selected differently, the cross-sectional evidence does not lend support to the income targeting hypothesis.

Second, we find that workers' daily pay responds to exogenous variation in weather conditions. In particular, workers earn more when the temperature is milder. This is in contrast to the

19. Other analyses of income targeting in different settings reach mixed conclusions. Oettinger [1999] finds that exogenous wage increases have a strong and positive effect on the labor supply of stadium vendors, which is not consistent with daily income targeting. Camerer et al. [1997] find that New York cabdrivers work fewer hours when the observed daily wage is higher and interpret this as evidence in favor of income targeting. In contrast, Farber [2004] using similar data, presents evidence against such income targeting by cabdrivers.

hypothesis that workers adjust their effort levels to achieve the same absolute daily income target.

Finally, as we show in Section VI, productivity under relative incentives depends on the social relationships between workers in the field and on the number of workers in the field, while neither of these two factors affect productivity under piece rates. These findings cast doubt on the hypothesis that income targeting is fully responsible for the difference in productivity under the two incentive schemes, as income targeting does not predict that social connections and group size should have a different effect under the two schemes (if at all).[20]

Another difference between the relative scheme and piece rates is that under the latter, the unit wage is set ex ante at the beginning of the field-day, whereas under the former the unit wage is determined endogenously at the end of the field-day, based on workers' productivity. Workers may then work less hard under relative incentives because of uncertainty over the ex post unit wage.

Such uncertainty may play a role in the first days a worker picks, but is unlikely to be driving the difference in productivity given that workers form expectations on the unit wage based on repeated observations each field-day they pick. Simulation results further show that uncertainty can only explain the observed change in productivity if workers' expectations of the variance are orders of magnitude larger than is observed in the data.

Finally, relative incentives and piece rates also differ because under piece rates workers may underperform if they believe that working hard will result in management setting lower piece rates in the future. In such a dynamic framework, productivity under piece rates is lower than implied by (4). This ratchet effect does not occur under relative incentives because the unit wage is based exclusively on the average productivity on a given field-day. Hence, in the presence of such ratchet effects, the true effect of the change in incentives on productivity is *underestimated*.

There are a number of reasons why in this setting, there are unlikely to be such large ratchet effects. First, given the stochastic nature of agricultural production, it is difficult for workers to disentangle changes in the piece rate due to changing conditions and those due to management learning about workers' true abil-

20. We thank an anonymous referee for suggesting this point.

ity.[21] Second, the effect of a worker's current performance on the unit wage she faces in the future is weak as the unit wage is field-day specific and workers are reallocated to different fields on different days.[22] Finally, any ratchet effect should become weaker as the time horizon of the worker becomes shorter. We checked for this and did not find worker's productivity to change significantly in their last week of work.

V. Structural-Form Evidence on Workers Preferences

V.A. *Empirical Method*

We now use the data on worker productivity to draw implications for workers' behavior in light of the models discussed in Section II. Our first aim is to assess whether the observed change in productivity is consistent with the standard assumption that workers ignore the externality they impose on others under the relative scheme ($\pi_i = 0$), or whether they fully internalize it ($\pi_i = 1$).

To this purpose, we use the first-order conditions of the workers' maximization problem derived in Section II to compute an estimate of each worker's cost parameter, θ_i, under each incentive scheme and behavioral assumption. Since the workers' cost (ability) parameters are innate, we ought to 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 which is a function of their effort e. Taking this into account, the first-order conditions for the choice of effort under relative incentives assuming that workers do not internalize the externality ($\pi_i = 0$), assuming that they do fully ($\pi_i = 1$), and under piece rates are respectively,

$$\phi'\left(\frac{y_i}{\bar{y}}\right)\frac{\partial y_i}{\partial e_i}\left(\frac{\sum_{j\neq i} y_j}{(\sum_i y_i)^2}\right) = \frac{1}{N}\,\theta_i e_i, \tag{9}$$

21. Such ratchet concerns have been documented to exist in workplaces where productivity shocks are less common such as shoe making [Freeman and Kleiner 1998] and bricklaying [Roy 1952].

22. In particular, workers face uncertainty over which fields they will be assigned to in the future—the probability a worker works on the same field on two consecutive days is .25. Workers are also uncertain about the identity of their future coworkers.

$$(10)\qquad \frac{\partial y_i}{\partial e_i}\frac{1}{(\Sigma_i\, y_i)^2}\left[\phi'\left(\frac{y_i}{\bar{y}}\right)\sum_{j\neq i} y_j - \sum_{j\neq i}\phi'\left(\frac{y_j}{\bar{y}}\right)y_j\right] = \frac{1}{N}\,\theta_i e_i,$$

$$(11)\qquad \phi'(\beta y_i)\beta\,\frac{\beta y_i}{\partial e_i} = \theta_i e_i.$$

To derive estimates of θ_i in each case, we proceed as follows. First, we assume that the benefit function is of the following CRRA type,

$$(12)\qquad \phi(y) = \rho y^{1/\rho} \text{ for } \rho \geq 1.$$

Throughout we report results obtained with $\rho = 2$. The results are, however, robust to alternative choices of ρ. Second, we derive an estimate of worker effort, e_i, assuming a Cobb-Douglas relationship between effort and productivity, as explained below.[23]

Third, we substitute data (on each field-day) for estimated effort, observed productivity (y_i), unit wages (β) and group size (N), into the first-order conditions above. We then obtain an estimate of θ_i on each field-day the worker picks, $\hat{\theta}_{ift}$, and take the median of these to derive a unique estimate of θ_i, under each incentive scheme and behavioral assumption.[24]

We, therefore, derive three estimates of θ_i based on the calibration of the first-order conditions (9), (10), and (11), respectively, (i) under relative incentives assuming that workers do not internalize the externality, $\hat{\theta}_i^{RN}$; (ii) under relative incentives assuming workers fully internalize the externality, namely that they choose efforts cooperatively, $\hat{\theta}_i^{RC}$; and (iii) under piece rates, $\hat{\theta}_i^{P}$.

Finally, we compare the distribution of $\hat{\theta}_i^{P}$ with the distributions of $\hat{\theta}_i^{RN}$ and $\hat{\theta}_i^{RC}$ to assess whether either of these two assumptions on the underlying behavior of workers is consistent with the observed change in productivity.

We assume that workers' effort e translates into productivity y through a Cobb-Douglas production function. To estimate

23. This implies 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 (9) to (11), $\partial y_i/\partial e_i \propto y_i/e_i$ with a Cobb-Douglas specification, so that θ_i is identified up to some scalar in each case. This does not affect the comparison of the estimated θ_i's across the first-order conditions.

24. The model is overidentified as sample workers work at least ten field-days under each incentive scheme. We use $\hat{\theta}_i$ = median ($\hat{\theta}_{ift}$) 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.

worker effort, we first estimate the productivity regression as in (7) controlling for the same determinants of productivity as in the baseline specification of column (4) in Table II, and interacting each worker fixed effect with the piece rate dummy. 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 (7) when incentive scheme s is in place:

$$\hat{e}^s_{ift} = \hat{\alpha}^s_i + \hat{u}^s_{ift}. \tag{13}$$

The first term captures the worker's average effort over time under incentive scheme s. The second term captures how much of the worker's productivity cannot be explained by observables. This method provides an estimate of each worker's 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 II, the mean and variance of effort both rise significantly moving from relative incentives to piece rates.[25] Moreover, we find that nearly all workers put in more effort under piece rates than under relative incentives and that there is little churning among workers—those that exert the most effort under relative incentives continue to exert the most effort under piece rates and vice versa.

V.B. Individualistic versus Fully Cooperative Behavior

Figure IVa shows the kernel density estimate of the implied distribution of workers' cost of effort $\hat{\theta}^P_i$ and $\hat{\theta}^{RN}_i$, namely under the assumption that workers ignore the externality they impose on others under the relative scheme ($\pi_i = 0$). This 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 that 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

25. Splitting the estimated effort (13) into each of its components—the residual $\hat{u}^s_{ift}$, and the worker fixed effect $\hat{\alpha}^s_i$—we find that the exponent of the residuals is centered around zero under each scheme, but the variance of the residuals under relative incentives is significantly higher. Hence, it is the distribution of worker fixed effects, and not the residuals, that drives the differences in the distributions of effort. See Bandiera, Barankay, and Rasul [2004] for details.

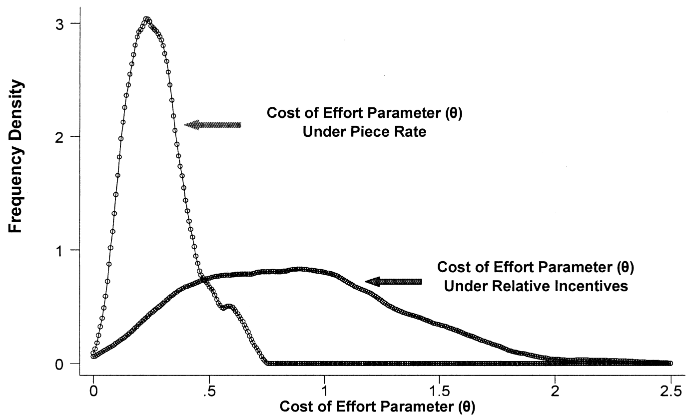

FIGURE IVa

Kernel Density Estimates of Cost of Effort Parameter, by Incentive Scheme Assuming Individualistic Behavior

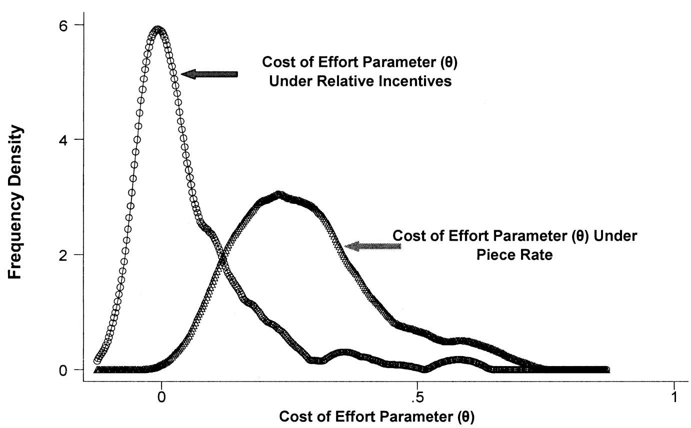

FIGURE IVb

Kernel Density Estimates of Cost of Effort Parameter, by Incentive Scheme Assuming Cooperative Behavior

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

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

The total cost of effort is assumed to be quadratic in effort. Under individualistic behavior we imply that the worker chooses her effort to maximize her own net benefits. Under cooperative behavior we imply the worker chooses her effort level to maximize the sum of all workers utilities.

workers ignoring the externality they impose on others under relative incentives.

Next, we estimate the distribution of workers' cost of effort $\hat{\theta}_i^P$ and $\hat{\theta}_i^{RC}$, namely under the assumptions that workers *fully* internalize the externality their effort imposes on their coworkers under the relative incentive scheme. Figure IVb shows the implied distributions of the cost parameter θ_i, by incentive scheme. The distribution of $\hat{\theta}_i^P$ under piece rates is, by definition, unchanged to that derived above. 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 IVa and IVb together reveal an interesting pattern. The observed change in productivity is too large to be reconciled with the assumption of individualistic behavior but too small to be reconciled with the assumption of fully cooperative behavior. This suggests that workers behave as if they internalize the negative externality to some extent. The next subsection explores this idea.

V.C. Social Preferences

We now posit that workers have social preferences, namely that they place some weight on the payoffs of their coworkers because of either pure altruism or collusion, and retrieve the reduced-form "social weights" (π_i) that fit the observed change in productivity.

To do so, we assume the true cost of effort of each worker is that derived under piece rates $\hat{\theta}_i^P$.[26] Given $\hat{\theta}_i^P$, we calibrate the first-order condition of the worker's maximization problem when they have social preferences, (2),

$$\frac{\partial y_i}{\partial e_i}\frac{1}{(\sum_i y_i)^2}\left[\phi'\left(\frac{y_i}{\bar{y}}\right)\sum_{j\neq i} y_j - \pi_i \sum_{j\neq i}\phi'\left(\frac{y_j}{\bar{y}}\right) y_j\right] = \frac{1}{N}\hat{\theta}_i^P e_i. \tag{14}$$

26. Using this measure of ability, we find that groups were equally heterogeneous, in terms of ability, before and after the change in incentives. Hence, there is no evidence of management sorting workers differently into fields by ability across the incentive schemes.

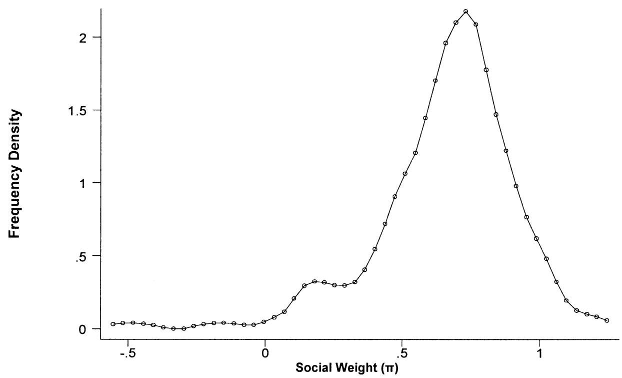

FIGURE V
Kernel Density Estimates of Social Weight (π)

This provides an estimate of each worker's social weight on every field-day they pick, $\hat{\pi}_{ift}$. As the model is overidentified, we take the median of these to derive $\hat{\pi}_i$.

The resulting distribution of social weights that explains the observed change in productivity is shown in Figure V. The average worker places a social weight of .65 on the benefits of all others in the same field-day. Less than 3 percent of workers have an implied social weight greater than one, and less than 2 percent of workers have an implied social weight of less than zero.

The next section pursues the idea that social preferences explain why productivity is so much lower under relative incentives. In particular, we explore whether the identity of coworkers on the field-day has differential effects on workers' behavior under relative incentives—when worker's effort imposes a negative externality on others, and under piece rates—when there are no externalities imposed on coworkers.

VI. INCENTIVES, SOCIAL NETWORKS, AND WORKERS PRODUCTIVITY

A natural candidate to explain the extent to which workers place weight on the payoffs of their coworkers is the *relationship* among workers on any given field-day. Naturally, the extent to which a given worker is altruistic or able to collude might depend on the social relations between her and her coworkers on a given day. To measure this social relation, we exploit data on each worker's self-reported friends on the farm. This information allows us to explore the hypothesis that workers internalize the externality *more* and, hence, are *less* productive when the externality hurts their friends rather than other workers.[27] To this purpose, we exploit the variation in the number of friends each worker works with on each field-day. We identify the effect of group composition on productivity by comparing the productivity of the *same* worker, on the same field, working alongside different coworkers on different days.

To obtain information on the workers' social networks on the farm, we administered a questionnaire about two weeks after the change in incentive schemes. Workers were asked to name up to five other workers they were friends with *before* coming to the farm and up to five other workers they became friends with during their stay. In the main analysis below we pool these two categories.

All but seventeen workers report having at least one friend on the farm. Conditional on having at least one friend on the field-day, the share of coworkers who are friends of worker i is on average 4 percent under both schemes. The dispersion is also very similar under the two schemes indicating that the results under piece rates are not due to lack of variation in the share of coworkers who are friends.[28]

To be clear, the *composition* of the group varies each field-day. However, workers themselves do not choose which field they work in and with whom they work. Rather, group composition is decided by management taking account of the demand for workers to perform nonpicking tasks, and how close workers live relative to the fields that need to be picked. The way in which workers are allocated to fields then leads to no systematic relation between individual shocks to productivity and group composition.[29]

27. Levine and Pesendorfer [2002] show that in an evolutionary equilibrium of a repeated Prisoner's Dilemma game in which workers learn which strategies to play, players behave as if they have social preferences. Moreover, the weight each player places on the benefits of another player depends on the relation between players. They argue that, "individuals will behave more altruistically when they can identify with the beneficiary of their altruism."

28. The mean share of workers who are friends of i is .043 under relative incentives and .037 under piece rates. Standard deviations are .042 and .034. The mean share is slightly lower under piece rates because new workers arrive at the farm. All but 36 workers report having at least one "old" friend on the farm. Conditional on having at least one old friend, the mean (standard deviation) share of workers who are old friends of i is .032 (.028) under relative incentives and .023 (.024) under piece rates.

29. Unsurprisingly, workers are more likely to work alongside those they live with than other randomly chosen individuals. The probability that a worker works

TABLE IV
THE EFFECT OF GROUP COMPOSITION ON PRODUCTIVITY BY INCENTIVE SCHEME
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

	(1a) Relative incentives	(1b) Relative incentives	(2a) Piece rates	(2b) Piece rates
Share of workers in the field who are friends	−1.68***	−5.52**	.072	1.17
	(.647)	(2.36)	(.493)	(1.60)
Share of workers in the field who are friends × number of workers in same field		1.60**		−.285
		(.684)		(.501)
Number of workers in same field		.182		.085
		(.117)		(.069)
Marginal effect of group size (at mean friends' share)		.236**		.076
		(.110)		(.065)
Worker fixed effects	Yes	Yes	Yes	Yes
Field fixed effects	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Adjusted R^2	.3470	.3620	.3065	.3081
Number of observations (worker-field-day)	2860	2860	4400	4400

*** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. 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 ten field-days under both incentive schemes. "Share of workers who are friends" is equal to the number of self-reported friends present on the field divided by the total number of workers on the field. Other controls include worker experience, field life cycle, and a linear time trend.

Table IV reports estimates of the productivity regression (7) under relative incentives, where we now additionally control for group composition at the field-day level as well as the baseline determinants of worker productivity in column (4) of Table II.

Column (1a) controls for the share of coworkers in the same field who are friends of worker i. Having more friends present significantly reduces productivity under relative incentives. The estimated coefficient implies that if worker i moved from a group

with someone she lives with is .605, with a self-reported friend, .549, and with neither someone she lives with nor a friend, .344. These probabilities do not, however, change significantly over the two incentive schemes.

with none of her friends to a group where five of her friends are present, her productivity would fall by 21 percent.

In column (1b) we interact the share of workers in the same field who are friends of worker i, with the total number of workers on the field. We find that (i) having more friends present significantly reduces productivity under relative incentives, and (ii) this effect is smaller the greater the number of workers in the same field. The latter effect is as expected given that the externality imposed by i on her friends is smaller when the overall group size is larger.

Column (1b) also shows that the marginal effect of group size is positive and significant, indicating that workers internalize the externality less when they work in larger groups, all else equal. The result is consistent with the intuition that larger groups may find it harder to coordinate on the low effort equilibrium.[30]

The results in columns (1a) and (1b) have some obvious alternative interpretations: when workers work alongside their friends, they might exert less effort and become less productive because they talk and socialize with their friends. Or, alternatively, they might choose to work with their friends when they feel less prone to work hard.

To shed light on these hypotheses, we analyze whether having friends around affects productivity under piece rates. Intuitively, any relationship between the identity of coworkers and productivity that is *unrelated* to the incentive scheme in place, such as socializing with friends, will be present under both incentive schemes. However, if the relationship between the identity of coworkers and productivity is related to the externality, it should affect productivity only under relative incentives. The results in columns (2a) and (2b) lend support to the latter interpretation since the share of coworkers who are friends of i has *no* effect on productivity under piece rates.

In short, the evidence indicates that under relative incentives workers internalize the externality more when they work alongside their friends. The fact that workers' productivity is not affected by the presence of friends under piece rates, indicates

30. Section II does not model coordination costs and, therefore, does not capture this aspect. In the model, the effect of an increase in N on workers' behavior is negligible. Indeed, on the one hand, each worker's effect on the mean is smaller when the group is larger. On the other hand, each worker affects more people when the group is larger.

that group composition affects productivity only when workers' effort imposes a negative externality on coworkers.[31]

One remaining concern is that friendships may be endogenous outcomes of workers behaving cooperatively with each other. To check for this, we use an alternative measure of social networks based only on the friendships that were formed *before* workers came to the farm and that are, therefore, uncorrelated with any events that took place on the farm itself. Reassuringly, the results in Table IV are unchanged if we use this alternative definition of friends.[32]

VII. Explaining Worker Behavior

The evidence presented so far is consistent with both a model of altruism where workers care directly about the utility of others or with a model of collusion where workers agree to cooperate for mutual benefit. Discriminating between these hypotheses is important. The implications for designing a work environment that facilitates cooperation between coworkers will be different depending on the underlying motive for cooperative behavior. We now present evidence to distinguish between the hypotheses of pure altruism and collusion.

Both hypotheses imply that worker i behaves *as if* in reduced form she has social preferences as specified in (1). However, workers are altruistic if the payoff of one or more coworkers enters their utility directly so that the structural- and reduced-form representation of preferences coincide. In contrast, self-interested workers would reduce effort under relative incentives if, as a group, they 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 cowork-

31. Any factor unrelated to incentives but causing individuals to treat friends differently over time will be spuriously attributed to the change in incentive scheme. To check this, we examine whether under piece rates, the effect of having more friends on the field is different for those who arrived later and so *only* worked under piece rates, compared with those who were also present under the relative incentive scheme. The results, not reported for reasons of space, show that workers do not react differently to friends when they first arrive.

32. As an additional check we also exploit the fact that workers who arrive on the same date from the same country are very likely to become friends. The identifying assumption is that date of arrival is orthogonal to any event that took place on the farm and could have affected the formation of friendship links. Results are unchanged if we define the group of friends to include only the people who arrived within the same three-day window.

ers, 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.

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.

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; Fehr and Fischbacher 2002; Rabin 1993], whereby individuals are only altruistic toward those who act altruistically toward them, and, thus, cooperate as long as others cooperate. The key difference between collusion and reciprocal altruism is that under the latter, cooperation is sustained even in the last period of play. However, we cannot 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.

The difficulty in separately purely altruistic from collusive motives arises because factors that lead individuals to be more altruistic toward each other, typically also facilitate collusion, and vice versa. Here we exploit the fact that the ability to monitor coworkers creates differences in observed behavior depending on whether workers cooperate because of collusion or altruism. We present evidence on workers' productivity for another fruit type whose physical characteristics are such that, unlike for the fruit type studied so far, workers are unable to monitor the performance of coworkers.

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 coworkers 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 coworkers' performance.

For the fruit type analyzed so far, monitoring others is costless since workers work alongside one another. Workers are not physically separated and so can easily form accurate beliefs on the performance of coworkers. To establish whether the ability to monitor coworkers affects behavior, we analyze the effect on individual productivity of the change in incentive schemes for another fruit type, which we label type 2, as opposed to type 1 fruit that is the focus of the previous analysis.

Type 2 fruit 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 that workers *cannot* monitor each other on the same field-day.

Over 80 percent 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 who pick type 1 fruit at least ten field-days under both incentive schemes and those who 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 who pick on the same field-day is much smaller for type 2 fruit compared with type 1 fruit—the average group size is nine under relative incentives and thirteen 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.[33]

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

$$z_{ift} = \alpha_i + \varphi_f + \gamma P_t + \delta X_{ift} + \eta Z_{ft} + \kappa t + u_{ift}, \tag{15}$$

where P_t is a dummy equal to one when piece rates are in place and zero otherwise. As in previous specifications we cluster the disturbance terms by field-day, and we controls for time trend, worker's picking experience and the field life cycle. The latter two are of course defined for type 2 fruit.

The results are presented in Table V. Column (1) reports that

33. The standard deviation of productivity under relative incentives and piece rates is 1.07 and .95, respectively.

TABLE V
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

	(1) Fruit type 2	(2) Fruit type 1	(3) Fruit types 1 and 2 combined
Piece rate dummy (P_t)	−.063	.483***	
	(.129)	(.094)	
Piece rate × fruit type 2			−.100
			(.095)
Piece rate × fruit type 1			.490***
			(.092)
Worker fixed effects	Yes	Yes	Yes
Field fixed effects	Yes	Yes	Yes
Other controls	Yes	Yes	Yes
Adjusted R^2	.3015	.3777	.6098
Number of observations (worker-field-day)	934	4224	5150

*** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Robust standard errors are calculated throughout, allowing for clustering at the field-day level in all columns. All continuous variables are in logs. When picking fruit type 1 workers can monitor one another. When picking fruit type 2 workers *cannot* monitor one another. 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 all columns is restricted to workers who have picked fruit type 2 at least once under both incentive schemes.

in the baseline specification (15), 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.

The result suggests that when the production technology is such that coworkers' performance cannot be monitored, workers do not internalize the externality they impose on others under relative incentives; namely they are equally productive under relative incentives and under piece rates.[34]

One possibility is that, in contrast to the average worker, this

34. Further analysis, not reported for reasons of space, shows that the result is robust to restricting the sample to ten days either side of the change in incentives. Moreover, results are robust to restricting the sample to field-days where workers only picked type 2 fruit and performed no other tasks. We also find that workers do not anticipate the change in incentives, and that piece rates have no effect on productivity at any point of the season.

particular subsample of workers simply do not cooperate, regardless of the monitoring technology. To check this, in column (2) we reestimate the baseline specification for this subsample when they pick type 1 fruit, where the behavior of coworkers *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.[35]

In column (3) we combine the observations across fruit types for workers who 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 coworkers. 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 rules out the hypothesis that internalize the externality because they are purely altruistic.

VIII. Conclusion

This paper provides evidence on social preferences by comparing workers' productivity under a relative incentive scheme with the productivity of the same workers under piece rates. Our estimates indicate that moving from relative incentives to piece rates causes productivity to rise by at least 50 percent for the average worker. We show that the observed change in productivity is consistent with workers internalizing the externality their effort imposes on coworkers to some extent. We also find that workers internalize the externality to a greater extent when a larger share of their coworkers are their close friends. Finally, we find that workers internalize the externality only when they can

35. 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 from the sample used for Table I.

monitor others and be monitored. These results are among the first to precisely identify an economic environment outside of the laboratory, where behavior can be explained by individuals' having social preferences in reduced form.[36]

Throughout, we have taken the incentive schemes as given. Our focus has been the response of workers to an exogenous change in incentives and the implications for their underlying preferences. A separate issue is whether the observed incentive schemes are optimally designed by the principal. Two questions arise. First, if the relative incentive scheme was so detrimental to productivity, why was it ever adopted? Second, are piece rates optimal in this context?

Regarding the use of relative schemes, the farm management suggested that the relative scheme was mainly adopted to difference out common shocks that are a key determinant of workers productivity in this setting.[37]

While this is in line with the predictions of incentive theory, the superiority of relative incentives relies on the assumption that workers ignore the externality their effort imposes on others. Under these conditions the equilibrium effort choices under relative incentives and piece rates are approximately equal for large group sizes. This assumption on worker behavior is not supported in our data. Relative incentives led to lower productivity because, perhaps surprisingly, workers internalized the negative externality to some extent.

The finding that workers place a positive weight on their coworkers' payoffs also indicates that piece rates might not be optimal in this context. To the extent that workers internalize negative and positive externalities in a similar way, group incentives, namely schemes where the worker's pay and her coworkers' performance are *positively* related, might elicit more effort at the same cost to the principal. To explore this issue further, we use our estimates of worker ability and social weights to simulate effort levels under group incentives.

36. Relatedly, List [2004] compares the behavior of the sellers of baseball cards in the laboratory and in the marketplace. He presents evidence that "local" sellers (namely sellers who regularly operate in the market) display social preferences both in the laboratory and in the market, especially when they interact with buyers with whom they have a long-term relationship.

37. See Lazear and Rosen [1981], Green and Stokey [1983], and Nalebuff and Stiglitz [1983]. Relative performance evaluation may also be preferred to piece rates as it lowers informational rents to high types [Bhaskar 2002] and reduces incentives of workers to exert effort in influence activity [Milgrom 1988].

Under the two incentive schemes we observe in the data, worker i's compensation is $w\bar{e}^b e_i$, where w is some constant, $\bar{e}$ is the average effort of the group, and e_i is i's own effort. Under relative incentives $b = -1$, and under piece rates $b = 0$. We illustrate the effect of group incentives within this class of compensation schemes by setting $b > 0$. While this need not be the optimal group incentive scheme, it makes the comparison with the observed schemes more transparent.

Figure VI shows average effort under these three classes of incentive schemes where individual pay and group performance are negatively correlated (relative incentives), uncorrelated (piece rates), and positively correlated (group incentives). We derive effort levels under the three alternative assumptions that workers are self-interested ($\pi = 0$), fully internalize the externality ($\pi = 1$), or have the average social weight derived in Section V ($\pi = .65$). Throughout, we adjust the parameter w to hold the total wage bill constant, and, for simplicity, we assume that workers are of homogeneous ability.[38]

The figure shows that the three types of incentive scheme yield the same level of effort only in the case of pure self-interest ($\pi = 0$). In line with the previous findings, when workers have social preferences ($\pi > 0$), effort is higher under piece rates than under relative incentives. More interestingly, the figure also shows that group incentives would, in this context, lead to higher effort at the same total cost to the principal. The estimates indicate that if $\pi = .65$, average effort would increase by 30 percent moving from piece rates to a group scheme where individual pay increases linearly in the average productivity of the group ($b = 1$).

The intuition for this is that since workers place positive weight on other workers' pay, the marginal benefit of effort is higher when effort benefits their coworkers, other things equal. To the extent that workers internalize the *positive* externality

38. We maintain the assumptions that workers' benefit from pay x is $\phi(x) = \rho x^{1/\rho}$, and the disutility of effort is $\theta e_i^2/2$. The θ parameter is set equal to its average estimated value under piece rates, and N is set to 40. Worker's compensation is kept constant at $c = 4.5$, the average hourly pay in Pounds Sterling. The Nash equilibrium effort level as a function of b then is

$$e = \left(\frac{c^{1/\rho}(1 + b(1/N) + \pi b((N-1)/N))}{\theta}\right)^{1/2}.$$

Note the effect of b—the relationship between individual pay and group performance—on effort depends on the sign and the magnitude of the social weight π.

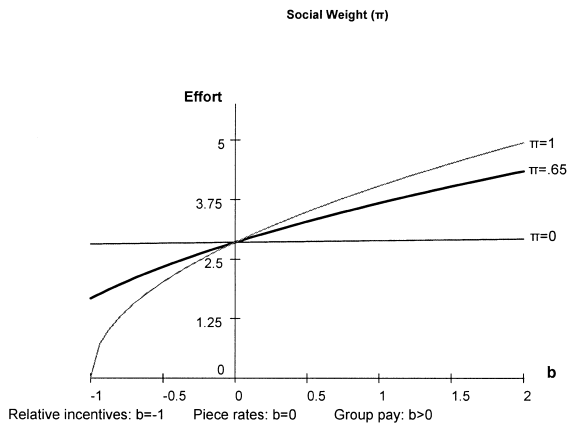

FIGURE VI
Incentive Schemes and Effort, Holding Total Wage Bill Constant

Estimated Effort under the Three Incentive Schemes when $\pi = .65$

	Relative incentives: $b = -1$	Piece rates: $b = 0$	Group incentives: $b = 1$
Effort, estimated from productivity data	1.66	2.82	
Effort, calibrated	1.67	2.86	3.68

Kernel density estimates in Figure V are calculated using an Epanechnikov kernel. The underlying benefit function is assumed to be

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

The total cost of effort is assumed to be quadratic in effort. Figure VI shows the Nash equilibrium efforts for $\pi = 0$, .65, and 1, as a function of the parameter b in the pay schedule:

$$pay = w\bar{e}^{b}e_i.$$

Throughout, the parameter w is adjusted to hold constant the total wage bill, and $N = 40$. We assume that workers have the same social weight (either 0, .65, or 1), and are of homogeneous ability. The θ parameter is set equal to its average estimated value under piece rates.

they impose on others, there is then a rationale for group incentives even in settings where the production technology does not exhibit complementarities.[39]

39. Rotemberg [1994] derives conditions under which group incentives are optimal if workers are altruistic. Sen [1966] analyzes the allocation rule that leads to Pareto efficiency in a cooperative whose workers place a positive weight on each other's material benefits. He shows that the optimal rule is a combination of individual and group rewards. Roethlisberger and Dickson's [1939] results from

In conclusion, our analysis emphasizes that understanding worker preferences is key for the optimal choice between alternative incentive schemes. Clearly, the magnitude of the effects might be particularly large in our context because workers live and work together and, thus, have both solid social ties and access to a variety of punishment mechanisms. The findings nevertheless show that to the extent that workers place *some* weight, either positive or negative, on the effect of their actions on the other workers' pay, group or relative incentive schemes can outperform piece rates in terms of productivity. The findings, thus, provide specific insights for further developments of incentive theory and shed new light on an old idea—the interplay between social effects and the provision of incentives within firms.[40]

Appendix 1: Quality and Quantity

We present evidence to see whether the change in incentives affected the *quality* of picking. To do so, we exploit the fact that pickers are expected to classify fruit as either class 1—suitable as supermarket produce, or class 2—suitable as market produce. While picking, each worker is expected to put class 1 and class 2 fruit into two separate containers. Class 1 fruit is the most common, accounting for 85 percent of the total weight of fruit picked on an average day under relative incentives and 87 percent under piece rates.

After fruit has been picked, it is transported to a cooled warehouse for packing. In the packhouse each container passes through a quality check. Whenever a class 2 fruit is detected in a class 1 container, it is removed—downgraded—and transferred to a class 2 container. By the time the fruit picked from a given field-day arrives in the farm packhouse for inspection, however, misclassification of fruit *cannot* be traced back to individual workers. Moreover, since the electronic system used to record

the Hawthorne experiments showed that productivity increased significantly for workers who were given group incentives and were allowed to socialize.

40. The idea that human relations affect workplace performance goes back to Mayo [1933], Barnard [1938], Roethlisberger and Dickson [1939], and Roy [1952]. More recently, Kandel and Lazear [1992], Lazear [1989], Rotemberg [1994], and Fershtman, Hvide, and Weiss [2003] have developed models incorporating social concerns into the analysis of behavior within firms. Fehr and Fischbacher [2002] discuss the experimental evidence on social preferences in firms.

individual productivity data is not the same used to record misclassification at the field-day level in the packhouse, it is not possible to match every field-day from the productivity and packhouse databases. We are, however, able to retrieve quality information for 67 field-day observations of which 29 are under relative incentives and 38 under piece rates.

In Appendix 2 we assess whether the trade-off between the quality and quantity of picking changed significantly with the change in incentives. We measure the quality of picking by the quantity of class 2 fruit that is wrongly classified as class 1, as a percentage of the total quantity of class 2 picked on a given field-day. We scale this to be measured in percentage points (0–100).

On average, 15 percent of class 2 fruit is misclassified as class 1 under relative incentives, and 12 percent under piece rates. The difference between the two schemes is not significant.

Since class 1 fruit on average constitutes 85 percent of all fruit picked, misclassifications are a negligible fraction of the total kilograms of class 1 picked on a given day. On average, 2.32 percent of the total class 1 fruit received by the packhouse is downgraded to class 2 under the relative scheme and 2.28 percent under piece rates.

In column (1) we regress this measure of the quality of picking on a dummy for the introduction of piece rates. In line with the unconditional results we find that the share of misclassified fruit falls by 3 percentage points, but the effect is not precisely estimated. Including field fixed effects, column (2), changes the sign of the piece rate coefficient, but the effect remains statistically insignificant. The magnitude of the effect is also quite small considering that on average there is five times as much fruit of class 1 as there is of class 2. The estimates then imply that the misclassified fruit as a share of the total of class 1 picked in a field-day increases by 0.4 percentage points.

In column (3) we additionally control for the quantity of class 1 fruit picked on the field-day and then also for a time trend and its square. Finally, column (4) adds controls for the field life cycle, and meteorological factors. We find that the level of misclassification of fruit picked increases over time, but at a decreasing rate. None of the other controls is significant. Our basic conclusion remain unchanged—the coefficient of the piece rate dummy is always small and not significantly different from zero.

The productivity gains achieved under piece rates were not at the expense of a lower quality of picking. Combined with the fact that worker pay remained constant over the season, the change in incentives unambiguously made the farm owners better off.

APPENDIX 2: THE EFFECT OF THE CHANGE IN INCENTIVES ON THE QUALITY OF PICKING

DEPENDENT VARIABLE = KILOGRAMS OF CLASS 2 FRUIT MISCLASSIFIED AS CLASS 1 AS A SHARE OF TOTAL KILOGRAMS OF CLASS 2 ON THE FIELD-DAY (PERCENTAGE POINTS 0–100)

ROBUST STANDARD ERRORS REPORTED IN PARENTHESES

	(1)	(2)	(3)	(4)	(5)
Piece rate dummy (P_t)	−3.39	2.08	2.19	2.02	2.71
	(3.78)	(2.90)	(3.00)	(4.20)	(3.86)
Tons of class 2 fruit picked × 10^{-3}			−.016	−.010	−.009
			(.010)	(.010)	(.011)
Time trend				1.02**	.846*
				(.438)	(.463)
Time trend squared × 10^{-3}				−.005**	−.005**
				(.002)	(.002)
Field life cycle				−5.61	−7.49
				(10.5)	(11.6)
Minimum temperature					.190
					(.584)
Maximum temperature					.831
					(.588)
Hours of sunshine					−.813
					(.834)
Field fixed effects	No	Yes	Yes	Yes	Yes
R^2	.0142	.0929	.1125	.1702	.2156
Number of observations (field-day)	67	67	67	67	67

*** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Robust standard errors are calculated throughout. The piece rate dummy is set equal to zero when relative incentives are in place, and set equal to one when piece rates are in place. Data are based on the packhouse software system. It is assumed that all fruit arrives in the packhouse two days after it is picked. The sample is restricted to those fields that operated under both incentive schemes. All right-hand-side variables are lagged by two days to allow for a time lag between picking and packing. Temperature variables correspond to a 0900–0900 time frame. Hours of sunshine are measured daily.

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