Short-Run Incentives and Myopic Behavior: Evidence from State-Owned Enterprises in China

Peter Zeitz[1]

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Abstract

How do performance incentives affect firm productivity? In 1978, Chinese industrial planners carried out reforms of the compensation system in state-owned enterprises (SOEs), introducing bonuses that linked pay to measures of worker and firm performance. Previous studies have argued that bonus use led to dramatic improvements in SOE productivity. However, since these incentives were based solely on short-run benchmarks, they could have encouraged enterprises to overemphasize current performance. Shifting effort toward short-run goals should increase productivity temporarily, but could have the opposite effect over a longer horizon. I collect a unique panel of compensation, employment, and output statistics .

profits and losses were absorbed by the state, so that firm performance had minimal impact on employee or managerial welfare. Within firms, employee compensation was strictly egalitarian; high-performing and ordinary workers received similar wages. In the late 1970s, reform-minded political leaders argued for the implementation of enterprise- and individual-level incentives. Over the ensuing decade, China moved from a centralized system in which the central government set the wages of individual factory workers, to a decentralized system where compensation was influenced by enterprise profitability, individual work performance, and local government policy.

First, in 1978, the central government ordered provincial governments to select firms for

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year, about half of SOEs began to use performance bonuses and piece rates. Then, in 1979, the Sichuan provincial government offered SOEs contracts that linked the availability of bonus and investment funds to firm profitability. A typical contract assigned a profit target equal to a three-year average of the past profit submissions and allowed firms to retain 20 percent of profits in excess of the target (Zhang 1992). Retained profits were distributed across funds for bonus payments, worker welfare facilities, and investment activities. Enterprises released money from bonus funds to managers and workers based on individual performance, but fund distributions were contingent on enterprises meeting performance benchmarks negotiated with provincial governments. Simultaneously, parallel reforms relaxed control over enterprise production and investment decisions, so that managers had latitude to adapt firm strategies to the

compensation, with the share of piece rates and bonuses in wage compensation growing from about 4 percent in 1978 to about 34 percent by 1988.

Since SOEs produced 76 percent of G gross industrial output in 1978

The multitasking hypothesis offers a potential explanation for these negative incentive effects (Holmstrom and Milgrom 1991). SOE incentives rewarded increases in current profit. However, besides production, workers and managers also make investments that affect future enterprise performance. Since SOE performance contracts were subject to frequent renegotiation with both central and local government principals, the bonus system did not provide enterprises with secure, long-term rights to profits (Cai et al. 2010). Case studies suggest that enterprises responded strategically to this setup by investing in short-term profit opportunities and avoiding technological projects with long incubation periods (Zhang 1992, Steinfeld 2000, Hassard et al. 2007). This pattern of investment allowed enterprises to maximize profit over the near-term, when they could be reasonably sure of earning performance-related rewards. If SOEs facing stronger incentives reduced investment

et al. 2010) periods 427TJ322.71 0 0. m[(pr)3(oje)-7(c)4(tsET7322.71 0 0unl.58 T7.09 1 464.02 571.18 Tm[( )]67322.71 0 0

productivity. However, I also find that past changes in the bonus share were negatively correlated with changes in labor productivity. The overall, long-run relationship between the bonus share and labor productivity is dominated by these negative correlations. Firms that issued more bonuses experienced less growth in labor productivity during the 1980s.

Interpretation of correlation between the bonus share and labor productivity requires an understanding of why the bonus share varied across firms. The bonus share varied according to regulations provincial governments used to control compensation practices, incentive contract choices made by firms, and -post success in fulfilling these contracts. Since firm choices and performance outcomes are endogenous, there is a legitimate concern that the negative relationship between labor productivity and the bonus share could be due to spurious correlation.

Correlation between the bonus share and labor productivity is comprised of a) an ex-ante causal effect of bonus contracts on employee and managerial decision-making, b) an ex-ante selection effect due to firms sorting into contract arrangements, and c) an ex-post effect due to random variables that i) are realized after the bonus contract is established, and ii) affect both labor productivity and success in meeting contract stipulations. The ex-post effect is the easiest to understand. Contracts between firms and provinces specified targets for profit, revenue, labor productivity, investment, and other enterprise-level outcomes. Firms that met their targets were allowed to increase their mean bonus share and wage. Accordingly, labor productivity growth and success in meeting performance targets should be positively correlated. That is, the ex-post effect should contribute a positive component to the overall correlation. The direction of effects of the two ex-ante components is difficult to predict a priori.

The main benefit of the SOE setting is that much of the variation in incentive contracts is due to local government policy decisions that are plausibly exogenous to other determinants of firm-level productivity change. I focus on variation due to two types of plausibly exogenous government decisions: a) selection of firms for participation in pilot incentive schemes in 1978, and b) province-level variation in bonus adoption. I assume that these sources of variation are unrelated to pre-existing, unobserved factors affecting firm -run labor productivity growth. Admittedly, this assumption could be erroneous and the results might be biased by endogenous contract choices. To mitigate this concern, I use three different identification strategies and estimate effects using multiple sources of variation.

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of incentive pay in 1978 as a source of plausibly exogenous variation in the timing of incentive adoption. The first year of incentive reform is unique in that allocations to pilot incentive schemes were made by provincial regulators, rather than through a process of self-selection by firms. I compute a matching estimator that quantifies the effect of incentive introduction on short- and long-run cumulative growth in labor productivity. The estimator compares performance trends in firms that adopted incentives in 1978 to those that adopted incentives later. If incentives have negative long-run effects, late adoption should be associated with greater cumulative growth in labor productivity.

The second method, which I call the wage control approach, estimates the relationship between the bonus share and current and future labor productivity in a panel regression. This method controls for dependence of the bonus share on ex-post performance, but does not attempt

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current profit and revenue to their average wage levels. However, some firms allocated more of

employed more skilled labor. To test this hypothesis, I estimate interaction terms between long-

Results from all three methods support my claim that incentives had negative long-run effects. Matching estimates indicate that firms that adopted incentives early had more rapid labor productivity growth in the short-run, but slower labor productivity growth in the long-run. The wage control regression indicates that firms which paid a larger fraction of wages as bonuses had higher labor productivity growth in the short-run, but inferior labor productivity growth in the long-run. In both of the above cases, the estimated negative long-run effect on labor productivity is at least two-fold larger in magnitude than the positive short-run effect. The instrumental variables regression indicates that greater bonus use was associated with reduced labor productivity growth between 1977 and 1988. I use two instruments here: a) the province-level average probability of bonus use between 1978 and 1983 and b) early firm-level adoption of bonuses in 1978. Estimates based on either instrument yield significant, negative effects of similar magnitude. Moreover, neither instrument is correlated with labor productivity growth except through effects mediated by the bonus share. Finally, tests for heterogeneous bonus effects show that negative productivity effects were more severe in firms that employed more engineers and technicians. This supports the argument that negative long-run effects of incentives were related to technology adoption.

I divide the subsequent discussion into six sections. First, I discuss the relation of this paper to multitasking theory the study of how incentives affect the performance of agents responsible for multiple tasks. The finding that SOE incentives improved current firm productivity but detracted from future productivity provides a unique contribution to this literature. The second section discusses short-term behavior in the Chinese SOE system. Here, I

summarize Chinese language literature arguing that managerial performance contracts induced short-term behavior and describe an example of this phenomenon in the iron and steel industry. The third section discusses methodological problems in previous studies of bonuses in Chinese SOEs. One of the factors hindering previous studies has been the lack of a set of panel data with an adequate time dimension. In the fourth section, I describe the collection of a large-scale dataset which facilitates the study of incentives in a dynamic context. The fifth section describes the three statistical methods I use and presents results which show that bonus-based compensation negatively affected long-run productivity. Finally, I conclude by reviewing implications of my findings for the study of88.350(,)-9(I)13bves(t)-11(e uET(doiron a)5(nr she ltiron (re)7(s.B

allocation and can negatively affect productivity. If distortions are sufficiently severe, principals could be better off avoiding the use of incentives, even though this encourages agents to shirk.

Several empirical studies have demonstrated that incentives can raise effort levels, while others have shown that they can distort effort allocation. Lazear (2000) analyzes a windshield installation firm where labor productivity improved dramatically as a result of a shift from time to piece wages. Similarly, Shearer (2004) finds that tree planters are more productive under piece rate pay than time wages. Other authors have focused on more complex activities, where agents have more opportunities to strategically reallocate effort. In the Soviet Union, managers earned bonuses of 30 to 100 percent of their base salary for meeting monthly production quotas, and were frequently dismissed for failure. The 1980s Chinese SOE incentive system was modeled after the Soviet system, so Soviet experience with bonuses is particularly relevant. Berliner (1956) argues that bonus incentives encouraged Soviet SOEs to rush end-of-quota-period production, defer maintenance, and abuse production equipment. Oyer (1998) documents similar behaviors among US sales workers who aggressively discount products to meet revenue quotas, with negative consequences for future sales. Asch (1990) studies the behavior of navy recruiters subject to periodic performance assessments and finds that recruiting success peaks at assessment time, but is unusually poor during the post-assessment period.

While these studies demonstrate that incentives have both positive and negative effects, they do not measure their relative magnitude. One way of comparing the positive and negative effects of incentives is by measuring productivity effects over different time horizons. Incentives are likely to disproportionately emphasize short-run activities because the provision of incentives for long-horizon tasks poses difficult measurement and commitment problems. When a considerable gap intervenes between when an activity occurs and when its effects become

evident, it can be difficult to identify the responsible individuals and offer them appropriate rewards. Moreover, in weak institutional environments, principals may not be able to credibly commit to maintain incentive schemes over a long time horizon. Multitasking theory proposes that agents will exploit these asymmetries by focusing on improving current performance, and neglecting activities which only yield results in the future. This could lead principals to prefer fixed wages when they employ agents to perform a mixture of short- and long-run activities.

Indirect support for the multitasking hypothesis is found in studies of contract choice which show that principals are less likely to use intense incentives when they hire workers to perform investment activities. For example, Ackerberg and Botticini (2000) find that land owners in Renaissance Tuscany used weaker incentives in employment contracts for perennial crops than for annual crops because investment activities were more important for perennials. Implicitly, studies of this type assume that incentives increase output in the short-run, and infer the importance of long-run concerns from employers choice not to use them. However, there are other potential explanations for the failure to use incentives. For example, employers might not use incentives because they subject employees to risk, generate socially unacceptable wage inequality, or fail to improve employee motivation.

Direct support of the multitasking hypothesis requires a demonstration that excessively intense incentives can actually reduce the value of the firm. However, this type of study is difficult to perform because firms do not randomize thei0 0 or1 300.43 52.44 72.0TBT1 0 0t.a5 Tm -0.048 Tc[(a

correlation between incentive use and productivity could reflect differences in these firm characteristics rather than the direct effects of incentives themselves.

Identifying the effects of incentives requires a source of exogenous variation, but this is typically unavailable in market settings. I analyze the effects of incentives in a population of government-run firms where much of the variation in incentive use comes from plausibly exogenous shifts in government policy. The availability of exogenous variation in compensation provides a strong justification for studying Chinese SOEs. A second advantage of the SOE setting relates to the bureaucratic arrangements governing the labor market. Even where bonus practices are externally imposed, the potential for performance-based pay to attract higher quality workers complicates productivity measurement. Bonus-using firms might perform better simply because they use higher quality inputs. However, during the 1980s SOE jobs were assigned by local labor bureaus and workers could not select their own employers.[3] As a result, bonus use is unlikely to hheir oIe affectedtypes of workers employed. The availability of exogenous sources of variation and the ability to rule out any role for workforce selection makes the Chinese SOE system an exceptionally good environment to test how incentives affected productivity.

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while others operated as subsidized make-work schemes for redundant employees. The imposition of consistent performance standards under these conditions conflicted with government objectives such as the retention of monopoly rents and hxsv[(edunda)-2(ti)-m[(ounm[(el-2(ti[()-12(r

added), a municipal government employee in Jiangsu province, describes -term

,*duanqi xingwei*) caused by performance contracts offered to enterprise managers in his jurisdiction. These are: falsification of profit reports and asset value reports, improper use and maintenance of machinery, misappropriation of investment funds; failure to upgrade production technology, develop new products, and offer workers training; the sale of substandard products damaging enterprise reputation, and the violation of safety regulations. Other articles describe the relationship between short-term behavior and managerial performance contracts in industry-specific contexts, including hospitals (Wang to be added), metals plants (Yu to be added), publishing houses (to be added), tea fields (Zhou to be added), fruit orchards (Chen and Wang to be added), coal mines (Zheng to be added), and export companies (Fuller to be added).

To provide a concrete example of how short-run behavior occurred in the industry I analyze, it is helpful to discuss the Handan Iron and Steel Company (The Reform of Small and Medium Iron and Steel Enterprises 1987). The incentive system at Handan was similar to that at many SOEs during the mid-1980s. Beginning in the early 1980s, Handan allocated a fixed fraction of its after-7.T1 01(ep with(indus)-2(t1 0ts)]2(on of)3(it)-] TJt(a)4(ds, H)-4(r)3(rnm6)-9cBT1 0 0 1 43

output using existing plant facilities and technologies. Revenue generated through the asset sale s allowed the firm to fulfill its performance targets and distribute bonuses.

IV. Problems in Past Studies

Existing studies have found that bonus use had a positive effect on SOE performance, but these studies suffer from some methodological problems. Econometric studies using the 1980-1989 Chinese Academy of Social Sciences (CASS) survey of 769 SOEs in four provinces, including those of Groves et al. (1994), Li (1997), and Yao (1997), have found that the introduction of bonuses improved enterprise productivity. The CASS data are based on a non-random sample of SOEs surveyed by the State Reform Commission. SOEs participating in the CASS survey were larger than average and had faster labor productivity growth than average. The reform effects of interest to researchers are those in the general population of SOEs, but these could differ from those found in the CASS data. Since productivity in the CASS firms improved unusually rapidly, it is possible that the reforms carried out in the CASS enterprises were unusually successful. If so, researchers using the CASS data may be likely to overestimate the benefits that reforms offered to the general population of firms (Woo 1999).

Studies using the CASS data estimate production functions incorporating measures of bonus use and other institutional controls as productivity determinants. Since planners distribute bonuses across firms on the basis of performance, the potential for reverse causation in these regressions is serious; the failure to observe all factors affecting productivity could lead to a positive coefficient on bonuses even if they are irrelevant or harmful to performance. Solving this problem requires a valid instrument for bonus use, but the instruments proposed in most studies are not convincing. Groves et al. (1994) and Yao (1997), for example, use a one-period

lag of bonus payments as an instrument, but since a as reflected in recent bonus payouts is positively correlated with current performance, this strategy is likely to bias estimates of the effects of bonus use upwards. Li (1997) argues that changes in planner-determined output quotas are set through a political process and not on the basis of firm productivity, and uses these allocations as instruments. However, this argument is dubious because both managers and planners observe performance-related information and factor this in when negotiating output quotas.

Shirley and Xu (to be added) use CASS data from the same period, but they use a more robust methodology and focus on a particular incentive reform rather than the expanded use of incentives in general. Their study compares total factor productivity in enterprises as a function of whether they operated under a contract linking managerial compensation to measures o enterprise performance. Since they are concerned that adoption of these contracts could be endogenous to enterprise characteristics, they use the province-level average of firms adopting managerial performance contracts as an instrument. After instrumenting, they find that managerial performance contracts negatively affected enterprise total factor productivity and that short-duration performance contracts were particularly harmful. The authors interpret this finding as evidence that performance contracts did not have incentive effects. In my view, performance contracts that lack incentive effects should not affect total factor productivity. If contracts negatively affected productivity, then this indicates that they encouraged strategic behavior that negatively affected productivity.

A problem with all of these studies is that they only look at contemporaneous outcomes. The focus on contemporaneous outcomes could be due to the limited time coverage of the CASS

includes mineral ores such as iron, manganese, and fluorite, intermediate products such as coke, pig iron, and steel ingots, and final goods such as finished steel and smelting machinery. The sample firms employ from 5 to 231,575 workers with an average of 3,659 and a median of 882. Based on firm names, they include central government-level, provincial government-level, and township and village-level enterprises.[5]

The CSILWS data contain observations of gross output per worker, measured in constant 1980 prices, together with physical data on labor productivity measured in the tonnage of the

However, the data do not contain financial data, and accordingly I use gross value of output per worker as a measure of firm performance. Since the gross value of output is based on a constant price weighting of all the firm s products, it is a physical measure

In theory, value-added is a better measure of output than gross value. However, during the 1980s SOEs often underreported the cost of inputs purchased on credit in order to over-report value-added and profit. Since state marketing agencies purchased most iron and steel goods, the physical quantities produced by each firm were much more difficult to misreport. I control for variation in the ratio of value-added to gross value

service), and contractual status (permanent, contract, temporary). These are useful controls since technical and supervisory workers are more likely to receive

as statistics in *Fifty Years of the Chinese Iron and Steel Industry Statistical Compendium, Vols. 1 and 2 (2003)*, report some investment, capital, and employment figures which are inconsistent with internal government sources and seem implausible.[7] Finally, the internal government statistical manuals are likely to prove valuable

I use the first year of policy reform (1978) as the source of variation in adoption timing. In 1978, the Chinese central government ordered provinces to select firms for the experimental use of incentives. As a result, 40 percent of iron and steel firms shifted from no use of incentives to some use of either bonuses or piece rates. I compare these early adopters to firms which adopted incentives in later years. The interpretation of cross-group productivity differences depends on how firms selected into the early adopting group. Though the central government ordered provinces to apply incentives in a broad range of firms (Wage Policy Compe

specifies two potential outcomes for each firm depending on whether the firm adopted bonuses, piece rates, or both bonuses and piece rates in 1978, and is thus in the treatment group, or did not adopt incentives at this time, and is thus in the control group. The underlying assumptions of the model are: first, after controlling for observables, selection for treatment is random; and second, conditional on their observed characteristics, all firms have some positive probability of treatment. For each firm *i*, the estimator evaluates two potential outcomes at time *t*: $Y_{it}(1)$, the outcome conditional on treatment, and $Y_{j(i)t}(0)$, the outcome conditional on non-treatment. Firms in the treatment and control groups are matched based on a set of observed characteristics, Z_i, where similarity is calculated on the basis of the distance norm described in Adabie et al. (2004). The parameter of interest is the average treatment effect, or the average difference between $Y_{it}(1)$ and $Y_{j(i)t}(0)$ in the population. This is computed by matching each treated firm *i* with a similar control firm *j(i)*, and calculating the difference in their outcome variable, τ_{it}. The τ_{it}s are then averaged to estimate the mean treatment effect in the population, $_t$. The estimator is summarized below in Equation 1:

$$\hat{}_t \quad \frac{1}{n} \quad _{it} \quad Z_i \quad \text{where } (\hat{}_{it}(Z_i) \quad Y_{it}(1) \quad Y_{j(i)t}(0) \mid Z_i \quad Z_{j(i)}) \qquad 1$$

Bonus and piece-rate adoption in 1978 could be correlated with unobserved firm characteristics and this correlation could bias the estimates obtained from Equation 1. To address this, I test for an intuitive type of self-selection into the early adoption of incentives. During the 1980s, firms were gradually granted autonomy to select their own compensation practices. If the 1978 experiment involved self-selection, one would expect early adoption of bonuses to be

5 4 If selection was arbitrary, one would not expect to find a

bonuses.

To check for self-selection, I use the matching estimator shown in Equation 1 to test if treated firms had persistently higher levels of bonus use. I specify the outcome variable as the share of bonus payments in wages in year *t*, where *t* {*1978, …, 1988*}. In the set of variables used to define matches, Z_i, I include categorical variables which are constant over time, including

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of dynamic variables related to firm labor productivity, size, compensation practices, and labor force composition.[8] The estimated average bonus shares for the treatment and control groups are shown in Figure 1. Treated firms had significantly higher levels of bonus use in 1978 and also in the two subsequent years, 1979 and 1980. Afterwards, as firms were granted more autonomy to control their incentive policies, the average bonus share in the treatment and control groups converged to similar levels. This convergence suggests that self-selection did not play an important role and supports the interpretation of incentive adoption in 1978 as a quasi-experimental process.

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that incentive use had negative long-run effects on labor productivity. By 1986, cumulative labor productivity growth in treated firms had fallen behind that in control firms, and the estimated difference is significant at the 5 percent level. By 1988, cumulative labor productivity growth in treated firms was 17 log percentage points lower than in control firms. The estimates also suggest that incentive adoption had positive effects on labor productivity growth in the short-run. From 1978 to 1980 the only years when the treatment group had significantly higher levels of bonus use cumulative labor productivity growth in treated firms exceeded that in control firms by between four and six log percentage points, though the differences are not statistically significant.

In the second method, I measure the intensity of incentives as the share of bonus compensation in total wages. There are two endogeneity issues that complicate interpretation of labor productivity. The first is that contracts which link labor productivity to the bonus share could be endogenous. The second is that, conditional on a contract, any unobserved variable which increases labor productivity will also increase the bonus share. For now, I treat contract choices as exogenous and address only the latter type of endogeneity. In an IV approach I introduce later, I allow for endogenous contracts.

I use wage changes to control for unobserved variables that affect labor productivity. In SOEs, wage levels can be interpreted as a measure of firm performance. During the 1980s, SOEs racts that linked changes in their average wages to their success in meeting performance targets. Both wage levels and the bonus share should be positively correlated with firm performance. However, contracts regulating wages were more homogenous than those governing bonuses and thus wage changes should provide more precise information about firm performance. I assume that the information in wage changes is

sufficiently complete that movements in the bonus share provide no additional information about firm performance after wage changes are controlled for. Under this assumption, inclusion of wage changes in regressions allows recovery of unbiased estimates of bonus effects.

This method requires a homogenous relationship between firm wages and ex-post performance. To investigate whether this assumption is reasonable, I estimate specifications which allow the wage-performance relationship to vary across years and provinces and also to depend on past wage changes.

expected $_{it}$. Equation 3 shows the model of wage setting, where W_{it} is log nominal wages, γ_{jt} is a province- and time-varying coefficient specifying the responsiveness of wage levels to the unobserved productivity determinant, and ε_{it} is

and past wage levels on the right-hand side directly. The results would be nearly identical.[10] I use the two-step procedure because it offers a clearer interpretation of how wages function in the regression.

I estimate several specifications of Equation 5 which show how different controls for wage residuals affect estimates of the coefficients on the bonus share. Since Equation 5 contains a large number of variables, I report only the following coefficients which summarize short- and long-run bonus effects. These are $\theta_{0,0}$, and the estimated sum of the coefficients on the lagged bonus share $\theta_{0,1} / \quad / \quad \theta_{0,9}$. For one specification, I also graph point estimates of the coefficient estimates on each lag of the bonus share as well as

one log percentage point), indicating that negative long-run effects approximately balance positive short-run effects in this year.

Specification (2) is identical to Specification (1) except that it controls for current period

should also apply to lagged wage changes. Since wage changes are much more tightly correlated with labor productivity changes than the bonus share, they should be superior predictors of mean reversion. However, the data do not show any relationship between past wage changes and current labor productivity changes. This indicates that mean reversion cannot explain the observed pattern of bonus effects.

To illustrate the effects of changes in the bonus share and the average wage on labor productivity, I graph the point estimates in Specification (3) in Figures 3 and 4. Figure 3 shows the coefficients on current and lagged changes in the bonus share, together with their 95 percent confidence intervals. The estimates show that bonus use in the current year and in the year previous had positive effects on current labor productivity, but that bonus use in prior years had negative effects. The negative effects are larger for longer lags, suggesting that the most severe distortions affected activities which took many years (five or more) to affect productivity. Figure 4 shows point estimates of the coefficients on current and lagged wage residuals. The positive sign and narrow confidence interval on the current period wage coefficient indicates a very tight relationship between changes in wages and labor productivity. Past wage changes, however, do not show any relationship with current changes in labor productivity; improvements in labor productivity associated with wage increases were completely persistent over time.

In Specification (4), I repeat Specification (3), but allow the responsiveness of wage changes to v_{it} to vary arbitrarily over time and across provinces. This is a test of whether heterogeneity in contracts linking labor productivity and wages biases the results. Results in Specifications (3) and (4) are extremely similar, indicating that contract heterogeneity at the province-year level does not bias the bonus share coefficients. While I cannot allow for more arbitrary forms of heterogeneity (for example, at the firm level), the stability of the coefficient

estimates across Specifications (2), (3) and (4) suggests that

primarily during the early years of reform. In later years, almost all firms use bonuses. Secondly, provincial planning bureaus had more authority over compensation practices during the early period of reform, so plausibly exogenous policy changes had stronger effects on bonus use during this period.

Provinces that permitted more bonus adoptions may have had unobserved characteristics that affected labor productivity growth rates. I test for this by exploiting firm-level incentive adoption in 1978 as an additional instrument. If bonus-

between 1978 and 1983, Z_i is a vector of firm characteristics circa 1977[12], P_i is a vector of the labor productivity levels for each year from 1964 to 1976, and ε_i is an error term assumed to be correlated with

$$Y_i \quad b_i \quad {}'Z_i \quad {}'P_i \quad {}_i \qquad 6$$

Results from estimations of Equation 6 are shown in Table 2. Specifications (1) through (5) use the full sample of firms and exclude historical data on labor productivity. In Specification (1), I use both instruments to identify effects. The estimates predict that a firm which issued no bonuses between 1978 and 1983 would have experienced 47 log percentage points more cumulative labor productivity growth than the median firm that issued ten percent of wages as bonuses between 1978 and 1983. On average, firms experienced 70 log percentage points of labor productivity growth between 1977 and 1988, so the effect corresponds to a 67 percent increase in labor productivity growth rates. It is useful to compare the results from Specification (1) to the matching estimates which relied on similar sources of variation. Based on the first stage IV estimates, firms that adopted incentives in 1978 paid an additional 3.2 percent of wages as bonuses on average between 1978 and 1983. Given a difference of 0.032 in between early and late adopters, the IV regressions predict that cumulative labor productivity growth in late adopters should have exceeded that in early adopters by 15 log percentage points. This estimate is similar in magnitude to the 17 log percentage point difference predicted by the matching results.

Specifications (2) and (3) test whether effect magnitudes are sensitive to the choice of instrument. In Specification (2), I use only 1978 firm-level bonus adoption as an instrument,

[12]The 1977 firm characteristics in Z_i 0

whereas in Specification (3), I use only the province-level probability of bonus adoption as an

the estimated impact of bonuses, indicating that pre-existing firm-level labor productivity trends and levels cannot explain bonus effects.

What mechanism could explain the large productivity losses associated with bonuses? The most common criticism of the bonus system in the case study literature is that it discouraged firms from adopting new technologies. According to this argument, firms chose to expand production of low-quality products using outmoded technologies because this strategy was more profitable in the short-run than process or product upgrading. Ideally, one would test this hypothesis using a firm-level measure of potential productivity gains from technology adoption. If incentives discouraged technology adoption, bonuses should have a stronger negative impact on firms that had larger potential productivity gains from technology adoption. Unfortunately, opportunities are not observable, so performing a direct test of this mechanism is difficult.

As an alternative, I consider the share of engineers and technicians in the capacity to absorb and benefit from new technologies. A large literature argues that advanced technologies complement skilled labor and that firms employing skilled labor adopt technologies more rapidly (for a review, see Violante 2008). In China, especially during the 1960s and 1970s, technically-skilled labor was assigned to SOEs through an inflexible bureaucratic process, so that the stock of technically-skilled labor within firms was only weakly related to firm-level demand. Due to these rigidities, f

technology adoption, negative bonus effects should be larger among firms employing relatively skilled workers.

To test these hypotheses, I estimate regressions which allow bonuses to have heterogeneous effects across firms employing different worker mixes. The 1977 workforce characteristics I consider are the share of employees belonging to four mutually exclusive occupational f8v.vl (nt wie-4(ics()4(rsupc)46(vis[(3(o)-19(, m[(teci)4(l f8v.l, bo fouvi)40(e)-5, f8v.nd trhe sh)-8

Results from estimations of Equation 7 are shown in Table 3. For ease of interpretation, I report workforce characteristic effects in terms of the predicted effect associated with a one

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Specification (1), I assume that effects of workforce characteristics and the bonus share are linear. The results show that firms that started with more technical workers in 1977 experienced more labor productivity growth over the next eleven years. A one standard deviation increase in the technical worker share was associated with an increase in labor productivity growth of 24 log percentage points. The interaction term between the bonus share and the technical workers share is negative and significant. A one standard deviation increase in the share of technical workers was associated with a 40 percent increase in the negative effect of bonuses on labor productivity growth. These results are consistent with the hypothesis that access to technical workers increased technology adoption opportunities and that the bonus system discouraged firms from exploiting these opportunities.

A similar, but less robust, pattern is evident for firms starting with large stocks of trainees. Firms that started with large trainee shares experienced more productivity growth, perhaps

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interaction term between the share of trainees and the bonus share is negative and significant. One possible explanation for this is that bonuses

bonus interactions to control for quadratic bonus effects. To identify the quadratic term, I use two additional instruments in Specification (2), the average provincial bonus share issued between 1978 and 1983 and the square of the average provincial bonus share.[15] Unlike the instruments based on adoption dummies, these instruments are related to the size of the bonuses offered and are thus more appropriate for identify aaaan4598.78 T983 and the squar] TJ3(i8 Tm -0.048 Tc[(es)]

reforms that clarified the enterprise-state financial relationship had the potential to improve their effectiveness. During the mid-1980s, the government attempted to modify institutional arrangements to increase the security of SOE claims on future profits. When enterprise incentives were initially introduced in 1979, the above-quota profit retention scheme was the most commonly used system, but some enterprises experimented with an alternative called the tax-for-profit scheme. The tax-for-profit scheme attempted to establish fixed profit taxes, so that, in theory, the share of profits retained by enterprises would not be subject to renegotiation. Enterprises under the tax-for-profit scheme appeared to perform better than those under the above-quota profit retention scheme (Zheng 1992). In 1983, the Chinese government replaced the above-quota profit retention with the tax-for-profit scheme in the hope that this would clarify enterprises financial rights. However, even after this reform, local governments continued to negotiate tax rates and subsidies on an enterprise-by-enterprise basis, so that the tax-for-profit scheme

The findings highlight the importance of assessing long-run impacts in studies of incentives. They also provide direct support for the multitasking hypothesis: in this setting, negative long-run effects are sufficiently large to justify the use of fixed wages rather than incentives.

A key policy implication of these findings is that the decision to apply incentives, whether in the public sector or in private firms, should be informed by an estimate of their long-run effects. Proponents of incentive payments often argue that they align the interests of principals and agents and thus promote more efficient decision-making. In the Chinese iron and steel industry, incentives had the opposite effect, encouraging managers to pursue value-destroying strategies. In practice, this effect would have been difficult to anticipate *a priori*. Policy makers, observing initial performance gains, probably expected these effects to continue as incentives were strengthened, and this may have encouraged nationwide adoption of incentive use. In developed market economies, owners have superior means of monitoring managers, but they still face problems in distinguishing current financial performance from long-run changes in firm value. Incentives in the American financial sector have been identified as encouraging excessive risk taking that contributed to the current banking crisis (Cheng, Hong, and Scheinkman 2009). The difficulty in distinguishing performance from risk-taking probably played a role in the diffusion of these incentive practices.

Another key contribution of the paper is to provide evidence for a complementary relationship between reforms in

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Figures and Tables

Figure 2: Effect of Incentive Adoption in 1978 on Labor Productivity Growth

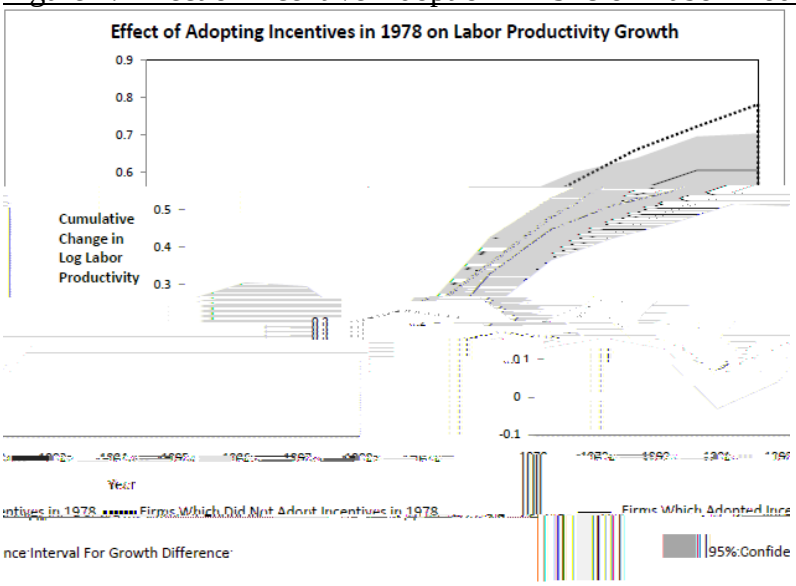

Figure 2 shows results from a matching estimator comparing labor productivity change in firms which participated in incentive adoption in 1978 to those which did not. The figure shows that treated firms had more rapid productivity growth during the initial three years of policy implementation (1978 to 1980), but inferior labor productivity growth in the long-run (1982 to 1988). The cross-group differences in the final three years (1986 to 1988) are statistically significant at the five percent level and large in magnitude. Average labor productivity growth for the control group is calculated as average labor productivity change in the entire sample after subtracting out the estimated treatment effect for the treated group. Average labor productivity change for the treated group is calculated as average labor productivity growth for the control group plus the estimated treatment effect.

Figure 3: Point Estimates of Coefficients on the Bonus Share

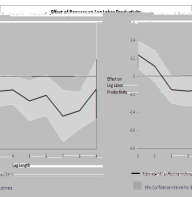

Figure 3 shows the coefficients on the current and lagged bonus share estimated in Table 1, Specification (3). The solid line shows the coefficient estimates and the shaded region bounds a 95 percent confidence interval around the point estimates. The positive coefficients on the current period coefficient and the first period lag indicate that bonus use had a positive effect on current labor productivity. The negative coefficients on the second through ninth period lags indicate that bonus use had a negative effect on future labore04rodur ctivity

Figure 4: Point Estimates of Coefficients on Wage Residuals

Figure 4 shows the coefficients on the current and lagged changes in wage residuals estimated in Table 1, Specification (3). The solid line shows the coefficient estimates and the shaded region bounds a 95 percent confidence interval around the point estimates. The positive coefficients on the current period and first period lag of the wage residuals indicate that wage increases were correlated with increases in current labor productivity. Coefficients on the second through ninth period lags are near zero, indicating that past wage increases were not correlated with changes in current labor productivity.

Table 1: First-Difference Estimates of the Short- and Long-Run Effects of Bonuses

	Dependent Variable: Change in Log Gross Output Per Worker for 1976 to 1988					
	(1)	(2)	(3)	(4)	(5)	(6)
(Row A) *d*	0.7*** (0.1)	0.3*** (0.1)	0.2*** (0.1)	0.2** (0.1)	0.3* (0.2)	0.3* (0.2)
(Row B)	-1.3** (0.7)	-1.7** (0.7)	-1.8*** (0.7)	-1.9** (0.8)	-2.2*** (0.8)	-2.3*** (0.8)
(Row C) Cumulative Effect	-0.01 (0.10)	-0.16* (0.10)	-0.17* (0.10)	-0.19* (0.11)	-0.21 (0.13)	-0.21 (0.13)
Current Wage Residual	No	Yes	Yes	Province-Year Specific	Yes	Yes
Lagged Wage Residuals	No	No	Yes	Yes	Yes	

Table 2: IV/2SLS Estimates of Long-Run Bonus Effects

	All Firms (1)	All Firms (2)	All Firms (3)	All Firms (4)	All Firms (5)	Old Firms (6)	Old Firms (7)
	First Stage Dependent Variable: Average Bonus Share from 1978 to 1982						
Firm Adopt	0.03**	0.04**		0.03**	0.03**	0.03**	0.03**
	(0.004)	(0.004)		(0.004)	(0.004)	(0.006)	(0.006)
Prov Adopt	0.10**		0.12**	0.10**	0.10**	0.16**	
	(0.02)		(0.02)	(0.02)	(0.02)		

Table 3: IV/2SLS Estimates of Interactions between Bonus Effects and Workforce Composition

	Dependent Variable: Change in Gross Output Per Worker Between 1988 and 1977			
	(1)	(2)	(3)	(4)
Bonus	-4.4** (1.6)	-8.4 (7.6)	-5.3** (1.6)	-4.71q469.18 (

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