

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

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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 for the Chinese iron and steel industry and use these data to estimate the short- and long-run effects of incentive use on labor productivity during the early period of SOE reform, 1976 to 1988. I find that incentive pay was associated with a small increase in labor productivity in the short-run, but a much larger decrease in the long-run.

**I. Introduction**

During the Cultural Revolution (1966-1976), compensation in Chinese state-owned enterprises (SOEs) was almost entirely independent of individual and firm performance. Firm

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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 E "gross industrial output in 1978, SOE  
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Any change that affected the productivity of these enterprises w

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 in technology, incentive schemes are likely to have negatively affected their long-run productivity.

My study tests for negative long-term consequences of incentive use. As in Groves et al. (1994), Yao (1997), and Li (1997), I study the correlation between productivity change in SOEs and the share of wages they distributed as performance bonuses. However, unlike all previous studies, I distinguish between the short- and long-run effects of the bonus share on productivity. I measure time-varying effects by allowing both the current bonus share and the bonus share issued over the past nine years to influence current labor productivity. Incorporating past bonus use as a productivity determinant requires information spanning a longer period than has been previously available. I collect a panel of compensation, employment, and output statistics for the population of SOEs in the Chinese iron and steel industry, covering 1949 to 1988, and use these data to evaluate the effect of bonuses on labor productivity from 1976 to 1988. Like other studies, I find that changes in the current bonus share were positively correlated with changes in labor

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 short-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.

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 to control for dependence of the bonus share on current profit and revenue to their average wage levels. However, some firms allocated more of

these wages to bonuses, while others relied more heavily on increases in time wages. I assume

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their wage quotas, but that the division of these quotas across time wages and bonuses varied exogenously across firms. This assumption implies that the effects of bonus use can be estimated in a panel regression that incorporates observed wage changes as a control variable.

The third method relies on instrumental variables to control for contract endogeneity and focuses exclusively on long-run effects. In this method, I compare firms' cumulative labor productivity growth between 1988 and 1977 as a function of the average bonus share firms issued between 1978 and 1983. Provinces set and enforced their own compensation regulations and this caused the timing of bonus adoption to vary across locations. I use province-level variation in the probability of bonus adoption between 1978 and 1983 to instrument for the bonus share. Since adoption became nearly universal by the mid-1980s, this instrument is not suitable for the analysis of bonuses effects during later periods. To test instrument exogeneity, I exploit firm-level incentive adoption in 1978 as an overidentifying restriction. Specifically, I check whether province-level probability of bonus adoption between 1978 and 1983 or firm-level adoption of incentives in 1978 are correlated with labor productivity growth after controlling for effects mediated through the bonus share.

Finally, I use the instruments developed in the third method to test for cross-firm heterogeneity in bonus effects. A Chinese language literature discussed in Section III argues that strong short-term incentives discouraged enterprises from adopting new technologies. Since advanced manufacturing technologies and worker skills are typically complementary, forgone technology adoption is likely to have larger negative effects on productivity among firms that



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 of incentive use in other settings.

## **II. Literature Review**

The finding that incentives negatively affected long-run performance is consistent with multitasking agency theory. Whether incentives are likely to improve productivity depends on the types of jobs agents perform and how effectively principals can measure their performance. Where performance can be measured across every dimension of a job, incentives should increase productivity. However, where jobs include some unobserved dimensions, agents can benefit by shifting effort from hidden to observed areas, potentially at some cost to efficiency. This problem was first formalized by Holmstrom and Milgrom (1991), who argued that the structure of conventional agency models, which assume that agents perform one task only rather than

" led economists to ignore the potential for incentives to distort behavior. In multitasking models, employees decide how much effort to supply and how to allocate it across multiple productive activities. Employers benefit from the full range of activities, but can only observe and reward performance in a subset of them. Rewarding a subset of tasks distorts effort

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, Akerberg 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 their compensation choices. Instead, firm " incentive choices are influenced by differences in their unobserved characteristics. For example, employers may adopt incentives because they hope to attract high quality workers or because they operate a technology that is well-suited to performance measurement. Employees may be more likely to accept an incentive package if they expect their performance to improve. Raw

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.<sup>3</sup> As a result, bonus use is unlikely to have affected the types 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.

### **III. Strategic Responses to Bonus Schemes in China**

The Chinese government introduced performance incentives to curtail shirking among SOE managers and workers. However, the lack of a level playing field made the implementation of incentive schemes challenging. Some enterprises operated as profitable protected monopolies,

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<sup>3</sup>Workers could not switch between SOEs except through a very difficult bureaucratic process. Quitting to work in the private or cooperative sector was permissible, but occurred very rarely because SOE workers enjoyed higher wages and status than workers in these sectors. For information on employment institutions in Chinese SOEs during the early 1980s, see Walder (1988).

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 prevention of unemployment. Instead, the government negotiated performance targets on an enterprise-specific basis, setting lower profit targets when enterprises benefited from monopoly power. Typically, this was achieved by using changes in enterprise profitability as a performance measure.

The lack of clear property rights was an additional obstacle to the creation of incentive schemes. The central government, multiple local government agencies, enterprise managers, and workers all had some form of poorly-defined enterprise ownership. Central and local governments repeatedly negotiated over control of enterprise profits and negotiations affected the availability of residual profits for enterprise retention. Given multi-party negotiation over the distribution of enterprise profits and losses, local governments could not commit to stable, long-term profit-sharing arrangements with enterprises. For example, reform guidelines specified that enterprise profit targets and profit retention formulas should remain fixed for three-year intervals, but in practice these targets and formulas were renegotiated annually (Zhang 1992).

Short contracting periods and pervasive renegotiation of contract terms made enterprise claims on long-run profits insecure. Enterprises feared that benefits from long-run investments would accrue to central and local government principals rather than to enterprise managers and workers. Since enterprise claims on near-term profits were more secure, they focused their efforts on activities that boosted current profitability. Articles in Chinese-language management journals suggest that this type of strategic behavior was particularly pronounced in enterprises where managerial compensation was explicitly linked to enterprise profitability. Dong (to be

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-

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



historical use of incentives as a performance determinant greatly reduces the sample size.

Omitting historical data on incentive use, however, may yield misleading results. To resolve this issue, I collect a very long panel containing up to 40 years of information, which allows me to estimate the effects of historical compensation practices without significantly reducing the sample size.

## V. The Dataset

I collect data from sources that have not been used before in the English or Chinese language literatures. For ease of reading, I translate their titles into English in the text; Chinese titles are given in the bibliography. The principal source of data is a three-volume set of plant-level wage statistics for the iron and steel industry, *Chinese Smelting Industry Labor and Wage Statistics, 1949-1988, Vols. 1-3 (1990)*, hereafter CSILWS, which was compiled from firm records by the Ministry of Metallurgy for internal government studies of the effectiveness of performance incentives. This source provides long-run historical data on nearly every SOE which operated in the iron and steel industry in 1988. I restrict analysis to 788 mining and manufacturing firms which were founded before 1980 and which produced standardized commodities. I exclude 189 firms which began operation between 1980 and 1988, since these firms do not have well-defined long-run compensation histories. I also exclude 123 agencies engaged in planning, construction, research, and education. It is worth noting that private firms did not participate in this industry during the 1980s, that entry of government firms was limited, and that exit was nearly non-existent.<sup>4</sup> Accordingly, the exclusion of new entrants from my analysis does not substantially limit the scope of my study. The range of commodities produced

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<sup>4</sup> Circa 1988, the incumbent state-owned firms that I study produced 97.8 percent of gross output in the industry, with new state-owned entrants accounting for the residual 2.2 percent.

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.<sup>5</sup>

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 across firms through the use of fixed effects, product variety-specific time dummies, and firm-specific time trends. Product variety-specific time dummies also capture price changes and technological trends which may affect firms differently depending on what they produce. Wage data indicate total compensation, as well as the amounts paid on a time, piece, and bonus basis. I control for the incentive effects of both piece rates and bonuses, but focus the analysis on bonuses because they were the predominant form of performance-based pay. Employment data includes total employment, employee sex, occupation (supervisory, technical, production worker, trainee,

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<sup>5</sup> For many firms, administrative level is not clearly identified. Accordingly, I do not try to distinguish between the effects of incentives within firms under different forms of state administration; for a study which does this, see Liu and Otsuka (2004). I do control for firm size, which is closely correlated with administrative status.

service), and contractual status (permanent, contract, temporary). These are useful controls since technical and supervisory workers are more likely to receive bonus-based compensation and shop floor workers piece-based compensation, and differences in employee composition could affect firm performance independently of the compensation structure. In addition to the quantitative variables directly provided in the dataset, I code 16 dummies for product varieties in which at least ten enterprises specialize.

The principal defect of the CSILWS dataset is that it does not contain data on the capital stock. These data are difficult to obtain and I was only able to gather information for very large firms or firms located in the provinces of Sichuan, Jilin, Henan, Jiangxi, and Shandong. Since reliable data on investment good prices are unavailable, I rely on book values of the capital stock net of depreciation to measure differences in capital intensity across firms. Like the CSILWS dataset, my data on the capital stock come from a variety of internal government sources published between 1962 and 2003.<sup>6</sup>

The CSWILS data were compiled for internal government use, not for public release. The use of internal government data has two significant advantages. Firstly, the CSWILS dataset includes the entire population of iron and steel firms, rather than a nonrandom sample. Secondly, longitudinal information in the CSWILS data can be crosschecked with other internal government data. I find that these sources are consistent with one another, regardless of when they were compiled. On the other hand, publically released plant-level data for large firms, such

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<sup>6</sup> The sources for the capital data are as follows: *Local Medium and Small Iron and Steel Enterprises Historical Financial Statistics* (1983), *Smelting Industry Enterprise Financial Statistical Materials, Vols. 1 and 2 1985-1990* (1992), *Smelting Industry Enterprise Financial Statistical Materials, Vols. 1 and 2 1949-1978* (1979), *Reform of Small and Medium Iron and Steel Enterprises, 1979-1987* (1988), *Liaoning Smelting Industry Production and Labor Statistics* (1962), *Sichuan Smelting Industry Financial and Cost Statistics 1950-1985* (1987), *Hunan Smelting Industry Statistical Materials, 1949-1976* (1977), *Shaanxi Smelting Industry Statistical Materials, 1949-1977* (1978), *Guizhou Smelting Industry Statistical Materials, 1949-1976* (1977), *Jilin Smelting Industry Statistical Materials 1949-1980* (1982), *Henan Smelting Industry Statistical Materials 1979-1985* (1988), *Jiangxi Smelting Statistical Materials 1949-1990* (1991), *Shandong Smelting Industry Statistical Materials 1980-1985* (1986) and *1986-1990* (1991), and *Fifty Years of the Chinese Iron and Steel Industry Statistical Compendium, Vols. 1 and 2* (2003).

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.<sup>7</sup> Finally, the internal government statistical manuals are likely to prove valuable for future studies because they provide an abundance of plant-level panel data from a poorly documented period.

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 Compendium 1980), provinces were more likely to select firms with a history of superior performance.

" " " " " ensured that bonus-using firms would realize higher average productivity levels. Provincial leaders hoped this result would please the central government (Shirk 1993).

Selection for bonus use in 1978 was not related to contemporaneous performance. This is not true of bonus adoptions occurring in later years, and I focus analysis on adoptions occurring in 1978 for this reason. Previous analyses of incentives effects have only been able to access data from 1980 and onwards, when adoption of incentives was clearly endogenous. In contrast, in 1978 incentive adoption had an intentionally experimental component. Though the 1978 adoption process overrepresented firms with superior past performance, this is a comparatively minor problem. Since I am able to observe firms' performance histories, I can control for oversampling by conditioning the comparison between early and late adopters on their historical performance.

I estimate how incentive adoption " ; 9: " " " - and long-run labor productivity using the bias-corrected matching estimator developed by Imbens and Adabie (2006), which was coded into a Stata algorithm by Adabie et al. (2004). The matching estimator

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,  $\delta_{it}$ . The  $\delta_{it}$ s are then averaged to estimate the mean treatment effect in the population,  $\tau$ . The estimator is summarized below in Equation 1:

$$\hat{\delta}_t = \frac{1}{n} \sum \delta_{it} \quad \text{where } (\hat{\delta}_{it}(Z_i) = Y_{it}(1) - Y_{j(i)t}(0) | Z_i = Z_{j(i)}) \quad 1$$

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 " " " " " " " " ; 2 ¶ If selection was arbitrary, one would not expect to find a " " " " " " " " " " bonuses.



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



expected  $\mu_{it}$ . Equation 3 shows the model of wage setting, where  $W_{it}$  is log nominal wages,  $\beta_{jt}$  is a province- and time-varying coefficient specifying the responsiveness of wage levels to the unobserved productivity determinant, and  $\epsilon_{it}$  is an error term assumed to be uncorrelated with the variables in Equation 2:  $db_{i(t-T)}$ ,  $dp_{it}$ ,  $dK_{it}$ , and  $dZ_{it}$ . The province- and time-specific coefficients allow for some additional flexibility, but this set-up still requires that firms in the same year and province share the same contract. The model allows wage changes to be autocorrelated for up to nine lags, so that wages in firms with different patterns of past performance changes could respond differently to the same performance shock.

$$dY_{it} = \beta_{jt} + \sum_{l=0}^9 \delta_{l,t} \epsilon_{i(t-l)} + \epsilon_{it} \quad (3)$$

Estimation of Equation 3 by OLS yields a set of residuals,  $\mu_{it}$ , which are shown in Equation 4, where  $\mu_{it}$  and  $\mu_{i(t-T)}$  are current and lagged estimation errors.

$$\mu_{it} = \beta_{jt} + \sum_{l=0}^9 \delta_{l,t} \mu_{i(t-l)} + \epsilon_{it} \quad (4)$$

The residuals in Equation 4 are firm-level wage changes that are orthogonal to observed workforce characteristics, province- and product type-level time effects, and firm-level time trends. Rearranging Equation 4  $\mu_{it} = \beta_{jt} + \sum_{l=0}^9 \delta_{l,t} \mu_{i(t-l)} + \epsilon_{it}$ , substituting the rearranged expression into Equation 2, and redefining the error term yields the estimating equation, Equation 5.

$$dY_{it} = \sum_{l=0}^9 \delta_{l,t} db_{i(t-l)} + dp_{it} + dK_{it} + dZ_{it} + \beta_{jt} + \sum_{l=0}^9 \delta_{l,t} \mu_{i(t-l)} + \epsilon_{it} \quad (5)$$

where  $\mu_{it} = \beta_{jt} + \sum_{l=0}^9 \delta_{l,t} \mu_{i(t-l)} + \epsilon_{it}$

I assume that the estimation errors,  $\{\epsilon_{i(t-l)}\}_{l=0}^9$ , are uncorrelated with other right-hand side variables, so that Equation 5 can be estimated using standard panel techniques. I estimate Equation 5 using a two-step procedure, including the residuals from Equation 3 on the right-hand side of Equation 5. Alterig set of residuals,

and past wage levels on the right-hand side directly. The results would be nearly identical.<sup>10</sup> 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  $\beta_{0,0}$ , and the estimated sum of the coefficients on the lagged bonus share  $\beta_{0,1} - \beta_{0,9}$ . For one specification, I also graph point estimates of the coefficient estimates on each lag of the bonus share as well as the coefficient estimates for each lag of the wage residuals. To provide a summary measure of the overall effects of bonuses, I compute an estimate of the mean effect of current and past bonus use on log labor productivity in 1988. This is a weighted sum of all the coefficients on the bonus share, where sample means of the current (1988) and lagged (1979-1987) bonus shares provide the weights.

Estimations of Equation 5, shown in Table 1, indicate that bonus use improved current period labor productivity, but had negative effects on future productivity. In Specification (1), I omit the controls for wage residuals. Since the bonus share should be positively correlated with the error term in the absence of wage controls, this specification should overestimate the positive effects of bonuses. Nevertheless, the results do not support a positive overall effect. Estimates of short-run effects are positive, but those of long-run effects are negative and slightly larger in magnitude. The estimated cumulative effect on labor productivity in 1988 is near zero (negative

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<sup>10</sup> There is a minor difference in the structure of autocorrelation in the error term between the one- and two-step procedures.

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 wage residuals that have constant effects over time and across provinces. The residuals control for unobserved determinants of firm performance which also influence the bonus share. Adding in the current period wage residuals reduces the estimated positive effects of bonus use by about 60 percent and increases the estimated negative effects by about 30 percent. The large reduction in the estimated positive effect suggests that positive correlation of the bonus share and the error term biases estimates in Specification (1). After controls for current period wage residuals, the estimated cumulative labor productivity effect in 1988 decreases to negative 16 log percentage points, indicating a substantial adverse impact.

In Specification (3), I add in lagged wage residuals to control for possible dependence of current wage changes on past wage changes. If past wage changes affect current ones, but are omitted from the regression, then lags of the bonus share will be correlated with the error term. However, coefficient estimates in Specification (3) are almost identical to those in Specification (2), suggesting that dependence of current wage changes on past ones is not an important source of bias.

Specification (3) has another interpretation. The regression compares the persistence of productivity changes associated with bonuses to those associated with general wage increases. This comparison provides a test for mean reversion in labor productivity. If labor productivity reverts to a mean level after experiencing a shock, a jump in productivity could simultaneously cause an increase in the bonus share and a future fall in productivity. This would lead to negative coefficients on lags of the bonus share due to spurious correlation. However, this mechanism

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  $\epsilon_{it}$  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-issuing provinces had different characteristics, estimates of bonus effects should differ depending on which instrument is used for identification. On the other hand, if both instruments are exogenous, a) estimated effects should be similar regardless of which instrument is used, and b) the instruments should be uncorrelated with firm-level labor productivity growth after effects mediated through the bonus share are controlled for. The results confirm these conditions, suggesting that the instruments are exogenous.

Another possible source of endogeneity bias is dependence of bonus adoption on past labor productivity levels. If both bonus adoption and subsequent labor productivity growth " " past performance, estimates of bonus effects will be biased. To test for this, I estimate specifications that restrict the sample to firms established prior to 1964 and control for " past labor productivity levels in each year from 1964 to 1976. I find that the estimated bonus effects remain unchanged after past labor productivity levels are controlled for. This indicates that " " ; 9: " " " " " " " " " bias estimates of bonus effects.

The regression specification is shown in Equation 6, where  $Y_i$  is change in gross output per worker between 1988 and 1977 in firm  $i$ ,  $B_i$  is the average bonus share issued in firm  $i$



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 " "the reform period with technically skilled workforces likely had persistently superior access to skilled labor. I expect these firms to have had more opportunities to adopt technologies and thus to have experienced more labor productivity growth. If the incentive system discouraged

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 categories (supervisory, technical, service, and trainee) and two mutually exclusive labor contract categories (temporary and contract). Effects are measured relative to those associated with production and permanent workers, which accounted for 78 and 91 percent of mean employment in 1977, respectively. The regressions employ the same instruments for bonuses as in Equation 6, firm-level bonus adoption in 1978 and the provincial average probability of bonus adoption between 1978 and 1983. I use interactions between the instruments and workforce composition to instrument for bonus-workforce composition interactions. The regression is shown in Equation 7, where  $Y_i$  is change in gross output per worker between 1988 and 1977 in firm  $i$ ,  $b_0$  is the average bonus share issued between 1978 and 1983,  $Z_{0i}$  is a vector of workforce characteristics circa 1977,  $Z_{1i}$  is a vector of other firm characteristics circa 1977<sup>14</sup>, and  $\epsilon_i$  is an error term assumed to be correlated with

$$Y_i = b_0 + b_1 b_0^2 + b_2 Z_{0i} + b_3 Z_{0i} + b_4 Z_{1i} + \frac{1}{2} Z_{0i}' Z_{0i} \frac{1}{2} + \epsilon_i \quad 7$$

In Equation 7,  $b_0$  and  $b_1$  measure the average effect of the bonus share in the sample,  $b_2$  measures bonus effect heterogeneity due to workforce characteristics, the coefficients in  $b_3$  and  $b_4$  capture effects of workforce and firm characteristics, and  $b_5$  captures any nonlinearity in the effects of workforce characteristics.

<sup>14</sup> The 1977 firm attributes in  $Z_{1i}$  include employment level, bonus share (equal to zero for 92 percent of observations),  $Z_{1i}$  also includes either dummies for 16 product varieties or a constant.

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  
 " " " " " " " " " " " " 0The  
 interaction term between the share of trainees and the bonus share is negative and significant. One possible explanation for this is that bonuses discouraged firms from making long-run investments in training.

Specification (2) allows bonuses effects to have a quadratic term. Firms employing technical workers tended to issue larger bonuses. If bonuses have a nonlinear effect, the nonlinearity could be spuriously absorbed in the bonus share-technical worker share interaction term. Accordingly, it is important to evaluate robustness of coefficients on the worker category-

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.<sup>15</sup> Unlike the instruments based on adoption dummies, these instruments are related to the size of the bonuses offered and are thus more appropriate for identifying the quadratic effect. The estimated coefficients are 4.62638 and -0.048, respectively, and the t-statistics are 1.98 and -1.98.

similar to those in Specification (3). The technical worker share-bonus share interaction term increases in magnitude relative to the Specifications (1) and (2) and retains statistical significance. The trainee share – bonus share interaction term decreases in magnitude and is no longer statistically significant.

The negative interactions between the bonus share and the technical labor share are robust across the four specifications. This indicates that firms issuing more incentive pay were less effective at utilizing technical labor. " " " " " " " ; 99" "

a robust positive relationship to subsequent productivity growth. These results are consistent with the argument that technology adoption was an important source of productivity growth and that incentive use discouraged technology adoption.

All of my results suggest that the 1980s bonus system negatively affected SOE productivity. Negative productivity effects raise the question of why the government used incentives in the SOE system. Divergence of short-run and long-run bonus effects is important here. The government may have expanded bonus use because it initially appeared to improve enterprise performance. The potential for negative long-run effects may have been difficult to detect early on because of the considerable information gap between enterprises and policymakers. A related question is why the government continued to use strong incentives even after it became aware of problems created by their use. The government likely had strong preferences for reform policies even when they failed to achieve their stated goals. Rollback of key reform policies could have jeopardized political momentum supporting China's transition to a market economy.

Since the government's inability to commit to permanent profit-sharing arrangements likely played a central role in incentives' negative long-run effects, the introduction of other

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 largely failed to establish fixed financial relationships between enterprises and the state.

Finally, in some cases, the Chinese government did curtail the use of incentive schemes. Managerial performance contracts offered beginning in the mid-1980s were heralded as a solution to the problem of managerial shirking. However, during the mid-1990s, as SOEs continued to perform poorly and policymakers became aware of problems related to short-term behavior, managerial performance contracts fell out of favor (Hassard et al. 2007).

## **VII. Conclusion**

This paper has shown that reform of the bonus system between 1978 and 1988 had a severely negative impact on SOE labor productivity in the Chinese iron and steel industry. Unlike most empirical studies of incentives, I explicitly contrast short- and long-run effects. The contrast between the short- and long-run is revealing, as the effects differ in sign and magni

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 property rights and incentives. The Chinese government was unable to commit to reward enterprises for long-term activities. In this environment, incentive payments appear to have severely distorted enterprise decision-making. A policy implication is that incentive intensity should be kept limited in institutional environments where long-term contracts cannot be enforced.

## Figures and Tables

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

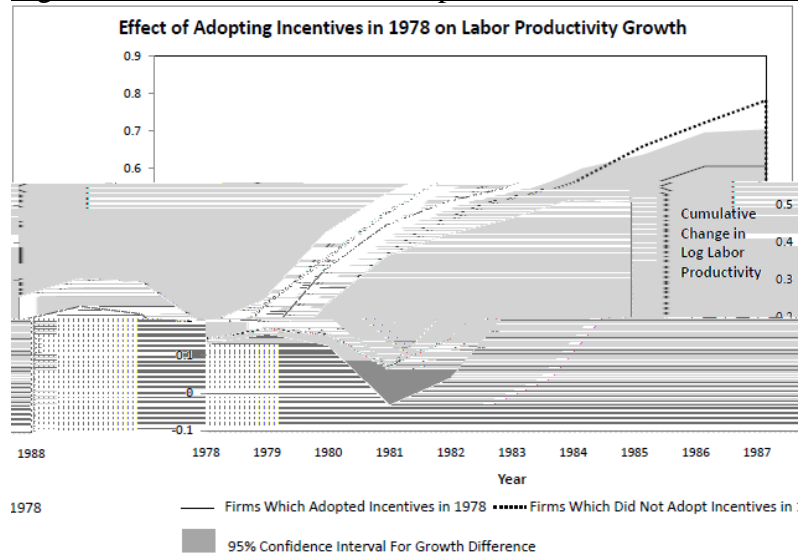


Figure 2 shows results from a matching estimator comparing labor productivity change in firms which participated in incentive adoption in 1978 to those which did not. The figure shows that treated firms had more rapid productivity growth during the initial three years of policy implementation (1978 to 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

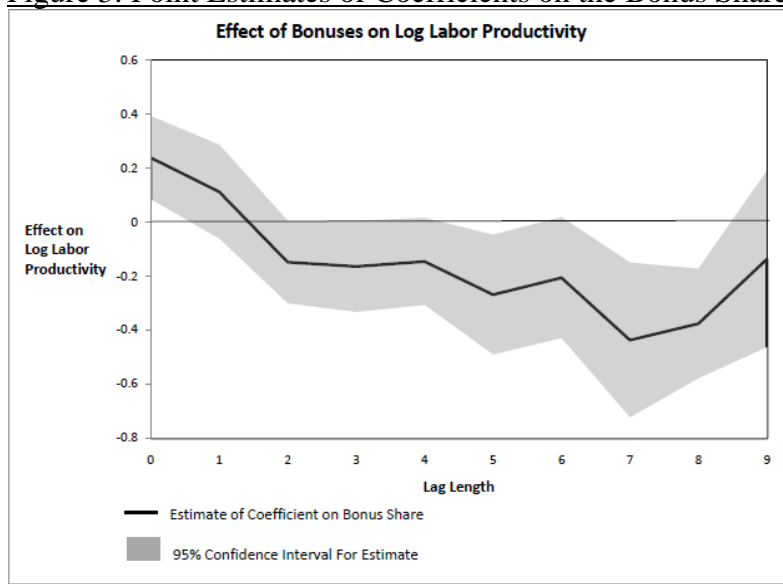


Figure 3 shows the coefficients on the current and lagged bonus share estimated in Table 1, Specification (3). The solid line shows the coefficient estimates and the shaded region bounds a 95 percent confidence interval around the point estimates. The positive coefficients on the current period coefficient 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 labor productivity.

Figure 4: Point Estimates of Coefficients on Wage Residuals

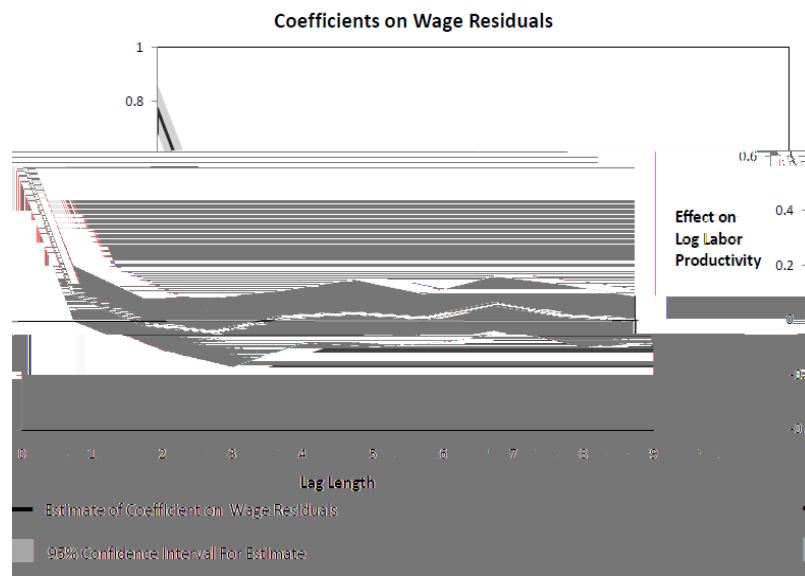


Figure 4 shows the coefficients on the current and lagged changes in wage residuals estimated in Table 1, Specification (3). The solid line shows the coefficient estimates and the shaded region bounds a 95 percent confidence interval around the point estimates. The positive coefficients on the current 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) <i>d</i>	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	Yes
Capital-Labor Ratio	No	No	No	No	Yes	No
Firm Specific Time Trend	Yes	Yes	Yes	Yes	No	No
Time Dummies	Variety and Province	Variety and Province	Variety and Province	Variety and Province	National	National
Observations	7603	7603	7603	7603	1494	1494

\*\*\*1% significance \*\*5% significance \*10% significance

Standard errors, reported in parentheses, are clustered at the level of 29 provinces.

Table 1 shows regression estimates of the short-run and long-run effects of bonus use on labor productivity. Results indicate that bonuses had large, negative long-run effects. Row A reports the coefficient on the current bonus share. This

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** (0.004)	0.04** (0.004)		0.03** (0.004)	0.03** (0.004)	0.03** (0.006)	0.03** (0.006)
Prov Adopt	0.10** (0.02)		0.12** (0.02)	0.10** (0.02)	0.10** (0.02)	0.16**	

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)	-

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