POLICY RESEARCH WORKING PAPER

WPS 1919

1919

The Empirical Effects of Performance Contracts

Evidence from China

*Mary Shirley**Lixin Colin Xu*

On average, performance contracts do not improve productivity in China's state enterprises and may even reduce it. But when they contain all the right features — managerial bonds, profit orientation, higher wage elasticity, and lower markup ratios — performance contracts can boost a firm's productivity growth rate by an estimated 10 percent.

Summary findings

Performance contracts are widely used to reform state-owned enterprises. By June 1994, there were 565 such contracts in 32 developing countries, used principally to reform large utilities and other monopolies, and roughly another 103,000 in China, where they are also used to reform state manufacturing enterprises.

A performance contract is a written agreement between the manager of a state enterprise (who promises to achieve specific targets in a certain time frame) and government (which — usually — promises to award achievement with a bonus or other incentive). Performance contracts are a variant of the pay-for-performance or incentive contracts often used to motivate managers in the private sector. In the public sector, they are viewed as a device to reveal information and motivate managers to exert effort.

Shirley and Xu analyze China's experience with performance contracts in more than 400 state enterprises. China is a good place for such a study because no country has ever used them on such a scale or with such a variety of enterprises (mostly in the competitive sector). China also uses many different kinds of contracts, with different targets (more profit-, tax-, or output-oriented).

Shirley and Xu find that performance contracts

- On average, do not improve productivity in China's state enterprises and may even reduce it.
- Are ineffective in competitive firms as well as monopolies.

- Do more harm when they provide only weak incentives and when they do not reduce information asymmetry.

They find no connection between variables for commitment and the effects of performance contracts.

Design matters. When performance contracts contain all the "good" features — profit orientation, higher wage elasticity, and lower markup ratios — the firm's productivity growth rate could increase as much as 10 percent.

The Chinese government was serious about implementing performance contracts, and used measures considerably more radical than other countries used, hailing the contract system as the official national mode for reforming state enterprises. But most of the contracts have had little or no effect on growth rates and the observed frequency of contracts with "good" provisions is exceedingly low.

Perhaps the political economy of incentive contracts in government settings merits further study. Political considerations may preclude the design of incentive contracts for government actors that could produce the sort of productivity gains some private firms have achieved. One observer (Byrd 1991) points out that the central government gave local governments a good deal of discretion in implementing performance contracts and local governments had a tendency to adopt the lowest common denominator, a "bare-bones" performance contract.

This paper — a product of the Development Research Group — is part of a larger effort in the group to understand state enterprises. Copies of the paper are available free from the World Bank, 1818 H Street NW, Washington, DC 20433. Please contact Paulina Sintim-Aboagye, room MC3-422, telephone 202-473-8526, fax 202-522-1155, Internet address psintimaboagye@worldbank.org. The authors may be contacted at mshirley@worldbank.org or lxu@worldbank.org. May 1998. (38 pages)

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Empirical Effects of Performance Contracts:

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Mary Shirley and Lixin Colin Xu¹

I. Introduction

Performance contracts (PCs) are widely used to reform state-owned enterprises (SOEs). The World Bank (1995) found that there were, as of June 1994, 565 such contracts in 32 developing countries, where they are principally used for large utilities and other monopolies, and another estimated 103,000 in China, where they are also used for manufacturing SOEs. PCs are written agreements between SOE managers, who promise to achieve specified targets in a given time frame, and government which (usually) promises to award achievement with a bonus or other incentive. PCs are a variant of pay-for-performance or incentive contracts, which have been often used to motivate private managers, and suggested as a way to improve central government agencies (Mookerjee 1997).²

The rationale for incentive contracts such as PCs is largely based on principal/agent theory (for reviews see Ross 1991; Stiglitz 1974; Sappington 1991). The

¹ The authors are Research Manager and Economist of the Development Research Group of the World Bank. We are grateful for the comments and suggestions of George Clarke, Robert Cull and Ross Levine. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the World Bank, its Executive Directors, or the countries they represent.

² New Zealand, for example, has used incentive contracts for ministries and other government bodies.

principal (in the case of state enterprises, the government officials) can only observe outcomes and cannot measure accurately the effort expended by the agent (the SOE manager) or distinguish the effects of effort from other factors affecting performance (Laffont and Tirole 1986, 1993). A negotiated incentive contract is viewed as a device to reveal information and motivate managers to exert effort. In the case of SOEs, performance contracts are also touted as a way to clarify the objectives of the multiple principals who govern state owned firms (different ministries, the president, the legislature), and hence make it easier to set goals and evaluate achievements. Proponents argue that the contract can translate multiple objectives into targets measured by specified criteria and given weights to reflect priorities (see Jones, "Performance Evaluation for State-Owned Enterprises" in Ramamurti and Vernon 1991). Moreover, targets can be set to take into account circumstances where SOE managers have less control over their firms than comparable managers in the private sector. For example, performance might be judged against the firm's past trends, rather than against an industry standard, to take account of situations where the firm's performance is sub-standard because of government imposed constraints (such as prohibitions on layoffs, price controls, etc.) By specifying targets and evaluating results ex post, the PC is seen by its advocates as a way to encourage governments to reduce ex ante controls, giving managers more freedom and motivation to improve operating efficiency.

Even though performance contracts are widespread there have been few empirical assessments of their effectiveness; and those that exist (Song 1988; Nellis 1989; Trevedi 1990; World Bank 1995; Shirley and Xu 1997) reach different conclusions. Song (1988) and Trevedi (1990) suggest positive outcomes based on country case studies in Korea and

India respectively. Trevedi finds that India's memorandum of understanding improves the dialogue between SOE management and government, but does not rigorously analyze the impact of the PCs on firm performance. Song finds improvement in performance in Korea, but the counterfactual is poorly specified. Nellis (1989) finds the effects of PCs in Africa were ambiguous, in part because at the time of the study the experience was still recent. Shirley and Xu (1997) found that PCs did not improve total factor or labor productivity or profitability because they failed to reduce information asymmetry, provide sufficiently high-powered incentives and credibly commit both parties to the goals of the contract. Their sample, however, was small (12 company cases in six developing countries), limited to natural monopolies, and consisted entirely of seriously flawed contracts. A larger, more diverse sample might find that, properly designed, PCs can improve performance, and shed light on how contracts should be designed in order to improve efficiency.

This paper analyzes the experience with PCs in over 400 SOEs in China. As a natural experiment, China's experience offers many advantages. First, there are a large number of contracts; no other country has used PCs on the scale of China. Second, Chinese PCs exhibit rich variations: contracts differed in length, type of targets (more profit-, tax- or output-oriented), and whether the manager changed and/or posted a bond as a pledge to improve performance. Third, the enterprises which signed PCs also varied: they were in many different industries in manufacturing with large variations in firm size, capital-labor ratio, markup ratio, pre-contract performance and the level of jurisdiction of the government which owned them. Most of the firms are in competitive sectors, which

allows us to test whether the pessimism generated by our earlier analysis of PCs for monopolies applies to PCs with competitive firms as well.

The disadvantage of using Chinese PCs for our sample is that it raises questions about the applicability of our results to PCs in other countries. However, since we are interested in contracts between government and state enterprises, the differences between China and other countries are less important than they would be if we were drawing conclusions for private firms. Studies of SOEs (such as World Bank 1995) suggest that the situation of SOEs in developing market economies closely resembles that of state enterprises in China (although not township and village enterprises) and other transitional economies. In both cases, governments intervene widely in operations, extend protection from competition and bankruptcy, and provide subsidies and debt relief. Since our study of PCs in China extends an earlier analysis of PCs in six market economies (Ghana, India, Korea, Mexico, the Philippines, and Senegal), radically different findings could suggest that China's special circumstances might be the cause. This was not the case, however. Finally, we postulate that the productivity effects of PCs are a function of how well the contracts address problems of information asymmetry, incentives, and commitment, contractual features which, judging from the literature on information economics, are the most important generic elements in characterizing contracts and country circumstances. At the same time we have attempted to control for as many aspects of the unobservable as we can, such as technology differences, competitive environment, etc., which should reduce the influence of factors which might be special to China in our results.

Our findings confirm Shirley and Xu (1997): PCs on average are not significantly correlated with improvements in productivity in a large sample of competitive SOEs in China. In fact, most types of PCs are found to have a large, significant and negative correlation with productivity. We also find that the contractual provisions mattered. In particular, wage incentives had a significant and positive correlation with productivity. PCs were more effective when they reduced the information advantage of managers by bidding or by setting targets focused on profits, which provide better information on performance than partial targets based on taxes paid or volume and value of output produced, or when they were in more competitive industries. Contrary to our expectation, PCs did not perform better when both parties were more committed, as manifested by the manager posting a bond or by a longer contract duration. Our findings suggest that PCs can improve productivity when they *simultaneously* improve information and offer incentives. Unfortunately, most of the PCs signed by the Chinese government did not include these features, and the overall effect on productivity was negligible.

Our analysis differs from other studies of Chinese contracts. Byrd (1991) describes in useful detail the principal types of performance contracts, and suggests that the main advantage of PCs over the traditional mode of government oversight was that they enhance and legitimize the positions of factory managers. He argues that the main problems with PCs are the strong bargaining power of managers, the tendency for local governments to opt for types of PCs that were easily implemented (rather than the most efficient or most suitable for local conditions), not enough risk-bearing on the part of firms, ambiguous ownership type, and non-credibility of contracts. Byrd (1991),

however, does not offer systematic evidence about the effectiveness of PCs. Groves et al. (1995) is more closely related to this paper. It examines the contractual provisions that affect SOE managers (such as length of the contract, management turnover, and the changing management pay sensitivity) and find that these provisions are consistent with a well-functioning managerial labor market. Groves et al. also analyzes the determinants of many other provisions, but does not systematically assess how they affected productivity, with the exception of the impact of management turnover. In contrast, our focus is on the impact of alternative specifications of performance contracts and their quantitative importance.

The next section briefly describes the implementation of PCs in China. Section III presents our hypotheses about the effects of PCs. Section IV then investigates the effects of PCs on performance of our sample, and compares the effects of alternative provisions. The final section draws policy implications from our findings.

II. Performance Contracts in China.

Apparently inspired by the success enjoyed by the Household Responsibility System, which gave more autonomy to farmers, the Chinese government began to experiment with PCs for SOEs in the mid 1980s³. Not until 1987, however, did the government implement PCs on a large scale.⁴ In our data set, the share of state enterprises

³ For more information on Chinese reforms, consult Groves et al. (1994, 1995), Jefferson (1994), Johnson (1990), Lin (1992), and McMillan et al. (1989).

⁴ See Byrd (1991) and Lin, Li, Cai (1997), for more details about the implementation of PCs in China.

under PCs grew from 8 percent in 1986 to 42 percent in 1987; it then skyrocketed to 88 percent by 1989.

The PCs in China included many variants⁵: (a) the traditional Manager Responsibility System, in which the manager was usually selected by the government and the performance targets tended to be vague; (b) the Contract Management Responsibility System, introduced in 1987, which usually consisted of multi-year targets and a reward system based on a link between employee wages and realized profits, but also included “a hodgepodge of different arrangements” (Byrd, 1991). (c) leasing, which is quite similar to leasing in the industrialized market economies (the lessor became the residual-claimant on any surplus after the leasing fee); and, (d) the Asset Management Responsibility System, which usually included selection of the CEO through open bidding and setting part of managerial compensation based on whether the SOE’s assets had appreciated by the end of the contract (as reflected by the next round of bidding). In this paper we treat any of these types of contracts as a PC. We capture the differences between the types of contracts by analyzing the impact of the different provisions -- such as their incentive component, primary target, and the method of selection of the CEO.

Some of the variations in Chinese PCs are summarized in Table 1. The duration of contracts ranged from one to eight years. Seventy one percent of the contracts specified an ex ante wage elasticity--the percentage by which total wages would increase when profits increased by 1 percent -- which varied from 0.1 for the 5th percentile to 0.8 for the 95th percentile, with a median of 0.6. Some PCs were coupled with a simultaneous

⁵ See Byrd (1991) for a discussion of the evolution and variants of PC systems. This paragraph borrowed heavily from Byrd (1991).

change of manager, with most (74 percent) managers appointed by the government, but some selected through bidding (17 percent),⁶ or elected by the employees (5 percent). The form of managerial selection varied according to the level of government overseeing the SOE. The more local the government, the more likely it was that the CEO was determined by bidding rather than being appointed.

Table 1. Characteristics of Performance Contracts

	<i>Pooled Sample</i>	<i>Firms Governed by:</i>			
		<i>central gov't</i>	<i>provincial gov't</i>	<i>municipal gov't</i>	<i>county gov't</i>
Number of PC-participants	628	52	64	450	54
by how the CEO was generated:					
%: appointed by government	.740	.942	.859	.733	.500
%: generated by bidding procedure	.169	.000	.063	.173	.389
%: elected by all employees	.049	.038	.047	.047	.093
by the Primary PC target:					
%: Profits	.422	.385	.406	.433	.407
%: Tax	.279	.192	.219	.313	.148
%: Quantity	.260	.327	.312	.227	.370
The share of PC-participants posting bond	.282	.019	.172	.280	.648
The average bond for bond-posting firms (in 1,000 yuan)	32.4	13.0	34.7	36.1	22.2

Note. Based on the authors' calculation from *A Survey of Chinese SOEs: 1980-89*.

In a few cases (16 percent of the companies) the manager posted a bond that was forfeited if he failed to achieve the contract's goals. As with bidding, posting a bond was more likely, the more local the government authority. The amounts are non-trivial, averaging about 32,400 yuan, several times a CEO's annual wages. SOE managers under

⁶ This category also includes those firms whose managers were directly appointed by the contract signer.

the central government (some of the largest firms) posted the smallest average amount, followed by county firms, which are some of the smallest SOEs.

The targets specified by the PCs also varied in the weight given to output goals, tax receipts, or profits.⁷ Almost 42 percent of firms in our sample reported that their PC's primary target was total before-tax profits (profit target hereafter), another 28 percent reported profits plus taxes remitted to the government (tax target hereafter) as the primary PC goal, and 26 percent, output quantity and value and labor productivity (output target hereafter). The choice of primary target also varied by oversight authority. The PC targets for firms governed by municipal and county governments tilted more frequently towards profit than PCs for firms governed by the two upper levels of government.

III. PCs Effects, Information, Incentive, and Commitment

Consistent with the literature on incentive contracts and information (see, for example, Freixas et.al, 1985; Grossman and Hart, 1983; Harris and Raviv, 1979; Laffont and Tirole, 1986), we postulate a game between risk-adverse managers with disutility of effort, and a risk-neutral government with imperfect information about the managers' effort. We expect that PCs will improve performance when they reduce the information advantage enjoyed by managers, increase managers' incentives to overcome their disutility of effort, and strengthen the government's and the firms' commitment to honor the contracts' goals.

⁷ We know from the questionnaires whether the primary target of the contracts focused on profitability, taxes or outputs.

Should PCs not reduce information asymmetry, we expect that managers will exploit the opportunity to shirk, perhaps by negotiating lower targets than they could potentially achieve, and performance will not improve. In our empirical analysis, the reduction of information asymmetry is represented by bidding and targets which focused on profits. First, we expect that bidding, by providing government with more information about the firms and potential managers (as well as by adjusting the manager's incentive to the conditions of the firm, as shown by Nalebuff and Stiglitz 1983 and McAfee and McMillan 1987), will reduce shirking and hence should be associated with better performance. Second, we expect that profit targets, by providing a more comprehensive signal about a firm's performance than tax or output targets, will also be associated with better performance. When the primary target is output, the firm may over-produce low-quality or high-cost products without increasing profits or efficiency; when it is tax, the firm may sacrifice investment or other expenditures important to long-term growth to be able to pay the tax (leaving aside the broader issue of whether the allocation of income to taxes instead of retained earnings enhances welfare). Profitability targets, in contrast, measure whether firms maximize revenues and minimize costs; as long as the firm does not enjoy monopoly power (in which case it can meet the target by raising prices), higher profitability should be associated with higher effort. Because the majority of our firms were competitive, we expect that PCs with targets which focus on profitability are more likely to improve performance than those focused on tax or output goals.⁸ Finally, we

⁸ Many economists emphasize the importance of a profit orientation in reforming SOEs in China. Lin, Li and Cai (1997), for instance, suggest that profitability could be a sufficient statistic for performance in competitive industries without soft budget constraints. Implicit in the ideas of market socialism was also the belief that SOEs could perform well when they pursued profit goals.

expect that PCs signed with firms facing more competition benefit from less information asymmetry and hence should be associated with higher productivity. Performance measures will be more meaningful, and shirking will be more evident in competitive firms relative to firms with more market power, and it will be easier to discern the firms' true efficiency level by comparing them with other similar firms. In the empirical analysis we proxied for competition with markup ratios (based on Li 1997; see appendix).

Not surprisingly, we expect incentives to raise productivity. We also expect that the strength of the incentive is likely to be constrained by information asymmetry. Since incentives to SOEs have a cost to society, governments will try to assure that the incentive payment does not exceed the social gain from increased firm efficiency. Hence, when there is information asymmetry, government will tend to set the incentive too low to motivate much improvement in performance. Alternatively, government may opt for a high powered incentive despite information asymmetry, in which case the firm may use its private information to extract a bonus without improving efficiency (for example, by convincing the government that the firm is less efficient than it really is, so that targets are set too low). Thus, without solving the information problem, introducing incentives can bring about only limited change. (Laffont and Tirole, chapter 1, 1994). In our analysis, the incentive we investigate is the elasticity of the total wage bill to profits. This incentive device has drawbacks. First, it may be distributed equally among employees in a context where there are limited ways to punish shirking (for example, there are often restrictions on firing SOE workers), which may reduce its incentive effects. Second, it reduces funds available for investment, which may reduce the long-term growth rate. Finally, since it is aimed only at workers, it will not have a sustained impact unless the

manager is also motivated to improve management practices and take other steps to enhance productivity.

Our third hypothesis is that PCs will improve performance if they elicit both the government's and the firms' commitment. When managers are not committed--perhaps because they expect to use their information advantage to bargain down the targets ex post --then ex ante they will exert only enough effort to fulfill the anticipated bargained-down targets. In this case a PC will fail to improve the manager's incentives, and fail to turn around the firm's performance. Alternatively, when government is not committed, it will fail to enforce the contract and/or will renege on paying the promised incentives. Since government is both a signatory and the enforcer of the contract, it is especially important that its commitment be credible. If the manager and employees do not believe that the government will honor the contract and pay the incentive if they meet their targets, they will hesitate to put in more effort for fear of ex post expropriation. Unfortunately our data do not permit us to test this hypothesis by assessing measures of commitment such as contract enforcement, reputation, or the like. Instead we proxy commitment by the length of the contract (longer contracts signal greater commitment by government) and by bonding (committed managers will agree to post bond). We expect that longer contracts and the use of bonds will be associated with greater improvements in performance.

IV. Effects of PC Participation and Provisions

We examined how PC participation and provisions affected the productivity of SOEs using a panel data set consisting of 769 firms from 1980 to 1989 located in four

provinces of China from *A Survey of Chinese SOEs: 1980-1989*. (See the data appendix for more details.) To take into account the potential differences in technology for firms in different industries, we decomposed the firms into 12 industry-size categories. The four industries are chemical, light, machine, and material industries.⁹ For each of these four industries three different technologies are allowed depending on size classification (large, medium, and small).¹⁰

The firms in the sample can be classified into two groups, those that eventually adopted PCs (the participation group), and those that did not (the non-participation group). Let d_i represents these two groups: $d_i=1$ for the participation group, and $d_i=0$, the other, where the subscript i denotes an individual firm. We assume the following Cobb-Douglas production function¹¹:

$$\begin{aligned} \text{non - participants: } Y_{it}^j &= \beta_0 + \beta_L^j \ln L_{it}^j + \beta_K^j \ln k_{it}^j + \beta_R R_{it} + \tau_t + u_{it} \\ \text{participants: } Y_{it}^j &= \beta_0 + \beta_L^j \ln L_{it}^j + \beta_K^j \ln k_{it}^j + \beta_R R_{it} + D_{it} \alpha_1 + D_{it} T_{it} \alpha_2 + \tau_t + u_{it} \end{aligned} \quad (1)$$

where i indexes firm, j indexes industry-size category, and t indicates year. The variables are defined as follows:

Y_{it} : log(value added per worker) (see the data appendix for the construction of variables and the associated deflators.)

$\ln L_{it}$: the number of employees, excluding those absent for more than half a year.

⁹ We follow Li (1995), which uses the same data set. The data contains firms in 36 two-digit industries, but many industries contain too few observations.

¹⁰ Large, medium, and small firms had, on average, 4083, 1486, and 615 employees respectively.

¹¹ The use of a translog production function produced results similar empirical results for PC effects. To simply presentation, especially of elasticity of capital and labor, only the results using the Cobb-Douglas function are reported.

$\ln k_{it}$: the capital-labor ratio. Capital stock is constructed from the data on the basis of a perpetual inventory method.

α_1 : captures the change in productivity *level* after PC adoption. (Recall that $D_{it} = 1$ for PC-participants after the PC was adopted.)

T_{it} : the number of years at year t since the PC took effect for firm i . T_{it} is 0 for pre-PC years.

α_2 : then captures the changes of the firm's productivity *growth rate* after PC adoption.

τ_t : year dummies (to control for economy-wide shocks such as overall credit cycle effects and other business cycle-related effects). In case of OLS, we also control for industry-size dummies; provincial dummies (to capture regional effects), and government oversight dummies (to capture differences in subsidies.)

R_{it} : other reforms and changes in the market environment that affected productivity (considered in Xu, 1997) and were applied to both PC and non-PC participants, including marginal profit retention rates, managerial wage discretion, production autonomy, mandatory plan shares, the presence of new managers, and the markup ratio.

u_{it} : the unobservable for firm i at year t

This “institutionalized” production function assumes that, besides physical inputs, a firm's output also depends on how production is organized, its internal

incentives, and the economic environment (degree of competition, etc.).¹² Note that we include “other reforms” in the production function; these occurred at different times than the PCs and display large cross-sectional variations. These variations allow us to distinguish the PC effects from the rest of reforms. The parameters of interest are (α_1, α_2) , the effects of the PC on productivity levels and growth, respectively.

Empirical Issues and Strategies

If PCs were randomly assigned across firms, then the identification of PC effects would be easy. However, PC participation and provisions might be non-random and the PC effects could be endogenous. We use several methods to deal with this endogeneity problem.

It is possible that some observables determine PC participation or provisions and are correlated with the error term of the productivity equation.¹³ To address this problem, we control for potential determinants of PC variables by including time-invariant dummies for government oversight, year, and firm. To address the possibility that firm-specific unobservables determines both PC variables and productivity, we include firm dummies. Another problem is that the PC-participants might be systematically different from the non-participants, or that firms with PC feature j might be systematically different from those without that feature. If we don’t control for these differences, the estimation may wrongly attribute the differences in firm-specific or sample-specific

¹² For the justification of “institutionalized production function,” see Jensen and Meckling (1979), Coase (1991), and McMillan et al. (1989).

productivity--which persists both before and after the PC adoption--to the causal effects of PC participation or PC features. To entangle the causal effects from those of firm or sample heterogeneity, we allow PC-participants to have different productivity levels and growth rates throughout the sample period; similarly, we experimented with allowing firms with PC feature j to have different productivity levels and growth rates relative to those without PC feature j . To avoid the loss of efficiency and multicollinearity associated with the creation of the sample control variables, insignificant sample controls were dropped.

Still another possibility is reverse causality, i.e., productivity affects PC participation rather than the reverse. To capture this possibility, we construct a dummy variable D_{t-1} , whose value is 1 only for the year preceding the adoption of the contract. In effect, this dummy will capture the systematic pattern in the error term of the unobservable in the year before the adoption. If the inclusion of this term does not change the PC effects much and the term is insignificant, then reverse causality need not be taken too seriously.

Besides examining alternative statistical assumptions about the error terms, we also checked the robustness of the results with respect to the choice of sample. Our estimations can be based either on the pooled sample of both PC-participants and non-participants, or on the sample of participants only. The PC effects in the pooled sample suffer from two sources of bias: (a) a cross section selection bias, that is, when the selection of better firms to sign PCs results in higher productivity which is wrongly

¹³ See Heckman and Hotz (1989) and Heckman and Robb (1985) for the methods to deal with endogenous policy interventions.

attributed to the effects of the PCs; and, (b) a time series selection bias, that is, when the change in the unobservable of the participants leads to gains which are wrongly attributed to the PCs. The advantage of using the pooled sample is the increase in sample size; the disadvantage is the upward cross-section bias that results because good firms may select into PCs. On the other hand, the smaller sample of PC participants suffers only from the time-series selection bias, i.e., that PC participants may tend to show accelerated growth over time whether they sign a contract or not. This suggests that the estimates based on the sample of PC participants may be more accurate than the pooled sample. In later empirical application, we use both samples and check if it is true that the pooled sample overstates more the effects of PCs.

We also examine the robustness of the results when capital and labor allocation are treated as endogenous. Following Arellano and Bond (1991), which suggests that properly lagged variables can be used to deal with endogeneity problem in a panel data environment, we take a first difference on the production function to filter out the firm-specific fixed effects; then use the generalized method of moments to estimate the production function. The instruments are the lagged values of capital and labor of $t-2$, $t-3$, and $t-4$. Since now the error term is $u_{it} - u_{it-1}$, these instruments should be orthogonal to the error term.

Effects of PC participation: Benchmark Results

For the empirical application, we deleted any observations missing the dependent variable or covering too short a period (less than 8 years). This leaves us with a

unbalanced panel of 628 firms, of which 544 are PC participants, and 72 are non-participants.

Table 2 reports the benchmark results of how PCs affected the productivity level and growth rate, both for the pooled and the participants samples. Column (1) reports the OLS results for the pooled sample, controlling for productivity levels and growth rates specific to the sample of PC participants. The joint significance of these two variables has a p-value of 0.003, suggesting the existence of cross-section selection bias with the OLS specification. In column (2), the FE estimates for the pooled sample suggest that when we control for firm dummies, PC-participants no longer differ significantly from non-PC-participants: the participants-specific growth rate is not significant. Note also that the dummy D_{t-1} (which is one in the year before the PC adoption) is not significant, suggesting that the reverse causality problem for PC participation should not be taken too seriously.¹⁴ Column (3) reports that first difference GMM results.

[Table 2 inserted here]

The estimates suggest that PCs did not improve productivity levels. While the FE estimates for the pooled sample suggest that PC participation has a positive association with productivity growth rates (of 3.4%, significant at the 10% level), the estimates for the sample of participants imply no statistically significant effect. Since, following our

¹⁴ The estimates of coefficients of labor and capital coefficients indicate that the production function for each of the 12 categories are indeed quite different (see the second panel of table 2). For OLS, the elasticity of capital ranges from -0.04 (insignificant) for small enterprises in Chemical industries to 0.787 in large firms in meaterial industries. In addition, some large enterprises, for instance, those in light industries and Chemicial industries, exhibited increasing returns to scale, while other industries, machines industries in participar, seems to exhibit decreasing returns to scale. The industry-size-specific growth rate also varied greatly. While machine industries grew more than 10 per cent per annum, light industries displayed negative growth rate.

earlier discussion, the cross-section selection bias for the pooled sample could overestimate the PC effects, greater confidence should be placed on the finding from the sample of participants. When capital and labor are treated as endogenous in the GMM estimates, the results are similar to our earlier finding that PCs did not systematically affect productivity. Both the level and rate effects are insignificant and enter with opposite signs in the sample of participants.

If it takes time for PCs to have an effect on productivity, our estimates of PC level and growth rate effects may be misleading. To examine this possibility, we estimated the same FE specification but replaced the PC terms with PC_{t+j} , $j=1, 2, 3, 4$, which is one if a PC was observed to be in place j years ago (see table 3). In effect, in this estimate PCs are assumed to generate effects only after j years. As before, we also controlled for PC-participants-sample-specific growth rate and reverse causality (D_{t-1}). The results lend little support to the premise that PCs have a positive effect on productivity. The only statistically significant association for PCs and productivity growth are found for PC_{t+1} in the pooled sample, which, as we noted, may overstate the PC effects. For the participants sample, we found no significant association between PC_{t+j} and productivity.

In summary, this evidence suggests that on average PCs in China were not associated with an improvement in productivity. This finding supports Shirley and Xu (1997): PCs, on average, do not improve performance. The fact that PCs in China did not improve productivity, however, does not necessarily mean that they cannot work when properly designed. We next test whether some PC designs did improve productivity, and

whether the ways different PCs affected productivity is consonant with our previous conjectures.

Effects of PC Provisions

To calculate the effect of different PC provisions, we re-ran the FE and first difference GMM models for both the pooled and participants only samples, for each of the variables that we presumed would affect information, incentives and commitment.¹⁵ In exploratory runs, we allowed the PC variables to affect both the productivity level and growth rate. In many cases, only the level or rate effect was found to be statistically significant; in these cases only the significant term was kept in the final specification, although we kept at least one type of effect (level or rate, depending on which was more statistically robust) for each PC feature. In addition, to address the potential self-selection problem with PC provisions, we allowed the sample of firms with PC-feature- j to have a sample-specific growth rate throughout the sample period. Most of the sample control variables for the PC features were found to be insignificant; consequently, we dropped these controls. In the end, we only kept two sample control variables: (year $\times$ PC-participation dummy), to capture the specific growth rate of the sample of participants in the pooled sample, and, (year $\times$ bidding dummy) to capture the sample-specific growth rate for those PC participants that selected the CEO through bidding in both the pooled and participants samples. The positive and, for the GMM estimates, significant sign of

¹⁵ Since FE decisively outperformed OLS (and random effects) specifications in all tests we do not report the OLS (and random effects) estimates of the effects of PC provisions.

the PC participants growth rate dummy (Row 3 of Table 4) suggests that PC-participants had higher productivity growth rates than non-participants throughout the period.

[Table 4 inserted here]

We first tested whether PCs are correlated with productivity improvements when managers are selected through bidding, since we hypothesize that bidding will provide government with more information (row (a)). The fixed effects estimates for both the pooled and the participants samples suggest that bidding had a negative but statistically insignificant correlation with productivity (row 1 under (a)); when we treat capital and labor as endogenous in the GMM estimates, the association becomes positive, but it is still insignificant. On the other hand, and consistent with our expectations, all four specifications imply a large (around 10 percent) and (in three of the four) statistically significant, positive association between bidding and the productivity growth rates (second row under (a)). Interestingly, the sample-specific growth rate for the bidding-PC-participants is negative (third row under (a)), i.e., firms which selected their managers through bidding experienced lower growth rates than the rest of the sample, even before the PC was signed. This suggests that slow-growing firms were put out for bids, after which their productivity growth improved. The negative effects of bidding on the level of productivity may suggest that it took time for these firms to turn around, or it may simply suggest endogeneity, as the change of the signs for the GMM estimates seems to suggest.

Our hypothesis that PCs emphasizing profits should perform better than PCs focusing on taxes or quantity of output is supported. The coefficients for firms with profit-oriented PCs suggest a positive correlation with productivity levels in both samples for both the FE and the GMM estimates (row (b)). The magnitudes are also nontrivial:

the FE estimates suggest an advantage of roughly 6 percentage points, and the GMM estimates, roughly 13 percentage points.

A final piece of evidence on the importance of information is the correlation between the impact of PCs on productivity levels and competition, as proxied by the markup ratio (row (c)).¹⁶ A reduction in the markup ratio, which we assume to be associated with an increase in competition and hence a reduction in management's information advantage, is associated with positive effects of PCs on productivity levels for all four specifications, although it is statistically significant only for the FE specifications. Note that the effects of any increased competition throughout the period alone are already controlled for (as part of the other reforms), hence this interaction term captures the influence of competition on the productivity effects of PCs. The magnitudes of the FE estimates are large, and suggest that, if we assume causality, then reducing the markup ratio by one standard deviation would increase the effects of PCs on productivity by at least 5 percentage points. The large standard errors for the GMM estimates, however, suggest that we should be cautious in assuming that causality runs from competition to PC effects on productivity.

Our hypothesis that PCs work better when they provide higher powered incentives is partly supported: higher firm-level wage elasticity is correlated with higher productivity, although the results are statistically significant only for the FE estimates (row (d)). If we assume causality, then a firm signing a PC with a wage elasticity of, for example, 0.66, would experience a jump in productivity levels of more than 10

¹⁶ The markup ratio is constructed based on Li (1997), which uses the same data set as this paper. See the appendix for details.

percentage points according to the FE estimates. In contrast, the GMM specifications show a much smaller and insignificant association, and could suggest that, instead of higher powered incentives causing higher productivity, firms with more efficient technology choose to sign PCs with higher powered incentives. While further inquiry is required to be certain of the underlying mechanism underneath the impact of wage elasticity, to the extent that the GMM estimates control better for unobserved efficiency parameters, they tend to support the latter explanation.

Finally, our hypotheses about commitment received little support from the evidence. The duration of the contract does not show any significant positive association with productivity. This finding may suggest that other signals of commitment are necessary beyond contract length, or that the length of the PCs in the sample (from one to six years, with most from two to five) were too short for long-term investment to occur. As for posting a bond, little positive effect was found. Again, this may suggest that other conditions beyond bonding are necessary for commitment by managers. Alternatively, it could be that when the managers who won the bidding had the chance to reap net positive private gains (net of the bond imposed), no additional positive PC effects would come out of bonding.

Comparison of Alternative PC Provisions

Table 4 shows the association between productivity and each PC provision separately, but does not tell us the combined effects of PCs with different provisions.

Using the FE estimators for the participants sample of Table 4, we calculated the combined effects, reported in Table 5.

Column (2) reports the combined effects of 9 types of PCs.¹⁷ The first row shows that bare bones PCs-- PCs with wage elasticity equal to zero, without bidding or posting managerial bond, and with primary PC targets other than profits and the average contract duration, for firms with the mean markup ratio—are negatively correlated with productivity levels (significant at the 5 percent level) and growth rates (not statistically significant). Bare bones PCs such as this accounted for roughly 11% of all PCs in our sample (column one). Second, bare bones PCs with bidding only, in other words PCs that appointed managers by bidding, without any other “good” provisions, have ambiguous effects. They are correlated with a drop of productivity of roughly 28 percentage points (significant at the 1% level), but with an improvement in productivity growth rates of roughly 9 percentage points (significant at the 5% level). In the third simulation, bare bones PCs with profit orientated targets are found to be somewhat better than bare bones PCs with other targets, since the negative effects on productivity levels becomes insignificant and smaller. However, firms with this type of PC still would not perform any better than firms without PCs. In the fourth simulation firms with a bare bones PC where the manager posts a bond are found to be indistinguishable from firms without PCs in terms of growth rates (the correlation is negative but insignificant), yet they are associated with a significant and negative productivity level (of 12 percentage points). Roughly 9 percent of our sample had PCs similar to this one. The fifth line

¹⁷ Since contract duration is found to have little effects, we do not calculate PC effects by this dimension.

presents bare bones PCs coupled with a mean level of wage elasticity, 0.42, and the sixth, with wage elasticity one standard deviation above the mean, 0.75. Both types of PCs have no statistically significant association with productivity, suggesting that firms with PCs such as these are indistinguishable from firms without PCs. Seventh, a firm with a bare bones PC and a markup ratio one standard deviation below the mean, is found to be indistinguishable from a firm without a PC. Finally, combining all the “good” features with a wage elasticity at the mean, results in a PC with a negative, but statistically insignificant, association with productivity levels effects (13 percentage points). When the wage elasticity is one standard deviation above the mean, the effect on levels is still negative and insignificant, but smaller (2 percentage points). In both cases the growth rate effect is significant, positive and about 10 percent. This suggests that, in the long run, firms with this type of PCs would outperform firms without PCs. However, only 2.2% of the sample firms had PCs with “good” provisions and wage elasticity at the mean, and no firms had “good” PCs and higher wage elasticity.

Overall table 5 suggests that most of the PCs used in China were not designed in ways that enhanced efficiency; indeed, some of them may have reduced productivity. Only the combination of all the “good” provisions is associated with productivity gains, but such PCs were used with very few firms. Given the wasted effort and possible penalty of poorly designed PCs, one wonders why good provisions were not more widely used in China.

Byrd (1991) suggests some possible explanations. When the central government decided to implement PCs on a national scale, it left a great deal of discretion to the local governments. These governments had limited skills and resources to implement complex

contracts, and consequently he found a tendency to adopt the “lowest common denominator” (Byrd, 1991), or what we call bare bones PCs. Another explanation suggested by his work is that the design of the PCs was constrained by macroeconomic conditions. Throughout this period Chinese SOEs faced large macroeconomic fluctuations (as evidenced by frequent and cyclical credit tightening and loosening over time), which increased information asymmetry (since it was harder to distinguish between the effects of management’s efforts and the effects of economic fluctuations). We would expect that when information asymmetries are large, government would prefer contracts with low powered incentives and shorter duration. World Bank (1995) suggests an additional, political factor. Governments may not want managers under PCs to introduce reforms that injure any of their main political support bases, such as workers, and hence may prefer sub-optimal PCs in firms with large numbers of employees. Shirley and Xu (1997) suggest that managerial bargaining power might also affect PC design, leading to softer targets.

VI. Conclusions

Our analysis suggests that PCs did not on average improve the productivity of state enterprises in China; they may even have reduced it. This supports with a much larger sample our earlier findings (Shirley and Xu 1997), and suggests that PCs are ineffective in competitive firms as well as natural monopolies. Also supporting our earlier model, PCs did more harm when they: (1) did not reduce information asymmetry (although the evidence is mixed); and (2) provided only weak incentives. We have failed to find any connection between commitment variables and PC effects. Design mattered:

when firms signed PCs which contained all the “good” features--managerial bonds, profit orientation, higher wage elasticity, and lower markup ratios—one estimate suggests that their productivity growth rate would increase by 10 per cent.

Our findings cast strong doubts on performance contracts as a way of reforming or regulating SOEs. The Chinese government tried to implement PCs quite seriously, as demonstrated by the use of firm-level wage elasticity, large managerial bonds, bidding procedures, measures considerably more radical than those observed in other countries we have studied (Shirley and Xu, 1997). In fact, the central government once hailed the contract system as the official mode for SOE reforms on a national scale. Yet we still observed that the overwhelming majority of the PCs have little or negative growth rate effects, and that the observed frequency for contracts with “good” provisions was exceedingly low. These results, coupled with our earlier findings, suggest that the political economy of incentive contracts in government settings merits further study. Political determinants may preclude the design of incentive contracts for government actors that produce the sort of productivity gains they have achieved in private firms.

Appendix A. the Data Set

The data set we use is *A Survey of Chinese State Enterprises: 1980-1989*. It covers 769 SOEs in 21 cities of four provinces of China (Shanxi, Jilin, Jiangsu, and Sichuan). Chosen on the basis of a stratified random sampling, the 769 firms were all in manufacturing. The size of the SOEs shows large variations: while the median firm had 930 employees, the 10th percentile firm had 304, and the 90th percentile had 3175.

The data set has two parts. Part one is a quantitative table filled by the accountants of an enterprise. It includes 321 variables covering details about products, costs, wages and labor utilization, investment, financing, fixed assets, profit distribution, taxes, prices, and material inputs. Part two is a questionnaire answered by the manager of the enterprise. The manager answers questions about performance contracts signed with the

government, the relationship between the enterprise and the government, production autonomy, the characteristics of the management, and so on.

Appendix B. Construction of Variables

In constructing these variables, we have followed other users of this data set, especially Li (1997), and Gordon and Li (1995). All quantities (value added, capital stock) are valued at the market value in 1989, assuming that the 1989 prices reflected best the opportunity costs of the resources.

Capital Price Indexes and Capital Stock

The survey contains answers to questions about the inflation rate of the mixed price of equipment between the periods 1965-1975, 1975-1980, 1980-1984, and for each year between 1985 and 1988. Based on these answers we computed average inflation rates for equipment. For 1980-1984, equal yearly inflation rates are assumed. For 1989, since we do not observe equipment inflation, we used the output inflation rate in the machine industry in 1989 as a proxy.

Since the survey does not provide information on prices of buildings or plant, for the inflation of building prices we used the percentage increase in aggregate construction costs compiled by the State Statistical Bureau. This series has also been used by Li (1997).

The composite price index for capital goods are then computed by averaging the equipment price index and the buildings and plant price index, the weights being the investment expenditures on equipment and plant.

We based our measure of capital stock on capital assets “for productive use”, which includes plant and equipment for industrial production. (In contrast, capital assets “for non-productive use” are mainly buildings and expenditures on dormitories, cafeterias, employee housing, and other social welfare functions.) For our purpose of measuring the contribution of capital, the use of “productive” capital assets is more appropriate. Following Li(1997) and Gordon and Li (1994), we choose not to use the net value of capital stock as the base to compute capital stock because the former “tend to exaggerate the increase in enterprise capita stock during the sample period in which the inflation rate was high, because the accounting rate of depreciation was artificially low and the depreciation was based on historical costs” (Gordon and Li, 1994).

Realized investment at year t is imputed by subtracting the nominal value of productive capital assets at the end of year $t-1$ from that at the end of year t . The reported investment, usually different from our imputed figures, is not used because it measures the value of capital expenditure (rather than capital formation) in a given year. It includes, e.g., expenditure on ongoing construction projects; while it excludes prior investment projects completed in the year.

When assuming that investment occurs smoothly over the course of a year, we can compute the capital stock in 1980($K_{i,80}$), the initial year, as

$$K_{i,80} = 0.5(K_{i,79}^* + K_{i,80}^*)P_{89}^K / P_{80}^K$$

where K_{it}^* is the productive capital asset in year t , and P_t^K is the cumulative price index for the composite capital goods.¹⁸ The capital stock for the following years are then constructed by the following formula:

$$K_{it} = K_{i,t-1} + 0.5I_{i,t-1}^* \frac{P_{89}^K}{P_{i,t-1}^K} + 0.5I_{i,t}^* \frac{P_{89}^K}{P_{i,t}^K}, t = 81, \dots, 89$$

where I^* is the imputed realized investment.

With this procedure, there are still a little more than 700 missing K_{it} . Their values are imputed as the industry-year averages for 36 industries.

Price Index for Value Added

The price index for value added is based on the price indexes of output and material inputs. Let P_{vt} be price index of value added in year t , and P_{Qt} be that of output, and P_{Mt} be that of intermediate inputs. Let Q_t denotes output units, and M_t input units. By definition, the Laspeyres price index of value added is computed as follows:

$$\frac{P_{vt}}{P_{vt-1}} = \frac{P_{Qt} Q_{t-1} - P_{Mt} M_{t-1}}{P_{Qt-1} Q_{t-1} - P_{Mt-1} M_{t-1}}$$

Tyler expansion along (P_{Qt-1}, P_{Mt-1}) gives the following formula for the percentage price increase of value added based on those of output and of intermediate inputs:

$$\ln \frac{P_{vt}}{P_{vt-1}} = \frac{Q_{t-1}}{V_{t-1}} (P_{Qt} - P_{Qt-1}) - \frac{M_{t-1}}{V_{t-1}} (P_{Mt} - P_{Mt-1})$$

(Below we discuss the construction of the output price index (P_{Qt}) and intermediate input price index (P_{Mt}). In the empirical implementation, we value the value added for each year at the 1989 price of value added.)

The Output Price Index

The survey reports the mixed (plan and market) price index for the firm's main product. While most firms reported cumulative price indexes, some reported year-to-year price inflation. We checked carefully and corrected those obvious coding errors. When in doubt, we treated them as missing. Consequently, we have around 500 firms reporting a reasonable mixed price index. For the rest of firms, we compute the average year-to-year mixed price inflation rates for their industry-year sample, then assign that value as the imputed mixed price inflation rate. Then, we converted them to a cumulative mixed price index.

The market output price index is then estimated. The survey has information about the sales under the state plan and to the market, and their respective prices. Based on this information, we constructed the market price index for output. Again, firms with missing values for the market price index are assigned their industry-year averages.

¹⁸. $K_{i,79}^*$, unobserved in the data set, is extrapolated as in Li (1994):

$$(\text{beginning - of - year total capital})_{80} \frac{(\text{end - of - year productive capital})_{80}}{(\text{end - of - year total capital})_{80}}$$

These price indexes are then used to compute the gross value of output (GVO). The survey reports GVO in current mixed prices. We first obtain GVO in current market prices by multiplying the reported GVO with the ratio of market output prices to mixed output prices in year t , then that number is translated into GVO in 1989 market prices by being multiplied by the ratio of market price index in 1989 to market price index in year t .

Price index of Intermediate Inputs

Since the data set has detailed information about the plan and the market prices of the two primary materials while it does not tell about those of energy and other intermediate inputs, the computation of the price indexes for intermediate inputs is based on the assumption that the inflation rate for intermediate inputs were the same as that of materials. This is reasonable since materials accounted for the vast majority of intermediate inputs. A significant portion of the reported material price variables are missing: roughly 40 percent of the answers are useful.

We first compute the mixed price of each material input using the physical shares of the plan and the market inputs. Then we compute the year-to-year Laspeyres index of mixed material prices from one year to the next. The year-to-year Laspeyres indexes of market prices are computed similarly. Again, the missing values are imputed using the industry-year averages.

The quantity of intermediate inputs are then computed using these price indexes. We first obtain the quantity of intermediate inputs valued at the current market price by multiplying the reported intermediate inputs--in current mixed prices--with the ratio of the current market price and the mixed price of intermediate inputs. This number in year t is then translated into intermediate inputs in 1989 market prices when it is multiplied by the ratio of the cumulative market price index of intermediate inputs in 1989 and that in year t .

The Markup Ratio

We follow Li (1997) in constructing the mark up ratio. Specifically,

$M_{it} = \sum_{j=1}^4 D_{ij} \mu_j - \theta \sum_{s=t+1}^{89} C_{is}$, The first term in the right hand side is the industry-specific markup ratio, assumed to be the markup ratio for all the firms in four industries (Light, Material, Chemical, and Machine). It is assumed that the markup ratios are identical in 1989 within the industry but differed among the four. The second term is calculated by assuming that the change in markup ratio is proportional to the change in output prices relative to input prices ($C_{it} = \pi_{it} - \pi_{it}^m$, π_{it} being enterprise-specific inflation in market prices of output, and π_{it}^m , enterprise-specific inflation in input prices).¹⁹ Thus, the markup ratio, though assumed to be a industry-specific constant in

¹⁹ To see this, note that (see Li 1995) when P_{it} / MC_{it} and its lagged value are close to 1,

1989, is allowed to vary across over firms and time between 1980 and 1988. Li (1997) estimated it to be 0.158. In addition, μ_1 is normalized to be 1, μ_j for material, machine, and chemical industries are estimated to be 0.41, 0.35, and 0.48. These estimates are used to compute M_{it} . It is important to note that the μ_j 's are identified only up to the proportion with respect to μ_1 ; thus, if the markup ratio is 1 for the industry with the smallest markup ratio, the markup ratio for the rest of industries are $(1/0.35) * \mu_j$, respectively.

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$$\frac{P_{it}}{MC_{it}} \approx \ln\left(\frac{P_{it}}{MC_{it}}\right) + 1, \text{ which implies}$$

$$\frac{P_{it}}{MC_{it}} - \frac{P_{it-1}}{MC_{it-1}} \approx \ln\left(\frac{P_{it}/MC_{it}}{P_{it-1}/MC_{it-1}}\right) = \ln\left(\frac{P_{it}}{P_{it-1}}\right) - \ln\left(\frac{MC_{it}}{MC_{it-1}}\right) \approx \frac{\Delta P_{it}}{P_{it-1}} - \frac{\Delta MC_{it}}{MC_{it-1}}$$

The first term of the last equation is output inflation rate, and the second term is proxied by the inflation rate for intermediate inputs.

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Table 2. The Benchmark Results of PC Effects
(Dep.=ln(value added per employee)): Controlling for Other Reforms

Sample:	Pooled (Participants & Non-Participants)			PC-participants Only		
	OLS (1)	FE (2)	First Difference GMM	OLS	FE	First Difference GMM
Dummy: PC Participants (that is $d_i=1$ dummy)	.108 (.098)					
Preceding dummy * number of years in the sample	.008 (.019)	.004 (.010)	.033 (.029)			
Dummy: the year before PC-adoption (D_{t-1})	.018 (.053)	-.010 (.039)		.027 (.055)	-.037 (.039)	
Dummy: post- PC_{it} (that is, D_{it})	.034 (.069)	-.020 (.051)	.002 (.038)	.023 (.071)	-.058 (.052)	-.018 (.038)
Post- PC_{it} * the number of years since the adoption (i.e., $D_{it} * T_{it}$)	.037* (.020)	.034* (.020)	.022 (.036)	.033* (.019)	-.003 (.020)	.034 (.038)
Number of observations	6176	6176	3673	5471	5471	3253
R square	.412	.455		.414	.490	
N*objective			64.79			58.27

Note. (a) Standard errors (OLS and 2SLS estimates has corrected for heteroskedasticity) in parentheses. *, **, and *** represents significance levels of 10, 5, and 1 percent. (b) R squares reported under FE models are R squares within. (c) In all the contests between OLS, RE, and FE specifications, the FE models win decisively. (d) Other control variables for all specifications include year dummies of 1984 to 1989; other reforms: marginal profit retention rate and its missing indicator, the dummies of managerial wage discretion, of production autonomy, of the presence of new management; the coefficients for capital-labor ratios and the numbers of employees for different industry-sizes are reported below. Rows under "OLS, pooled" relate to OLS(1) above, and "FE, pooled" FE(2) above:

Corresponding coefficients for capital and labor in the above regressions for industry * firm category:

		Material, Large	Material, Medium	Material, Small	Light, Large	Light, Medium	Light, Small	Chemical, Large	Chemical, Medium	Chemical, Small	Machine, Large	Machine, Medium	Machine, Small
OLS pooled	ln(capital-labor ratio)	.750*** (.093)	.065 (.049)	.274*** (.046)	.500*** (.058)	.264*** (.051)	.411*** (.061)	.212*** (.096)	.050 (.074)	.017 (.091)	.261*** (.076)	.513*** (.051)	.547*** (.087)
OLS pooled	ln(number of employees)	.057* (.032)	.054* (.032)	.087** (.037)	-.0003 (.024)	-.048* (.027)	-.030 (.032)	-.004 (.038)	-.012 (.042)	-.077* (.045)	-.027 (.029)	-.016 (.033)	.003 (.037)
FE pooled	ln(capital-labor ratio)	-.020 (.100)	-.288*** (.075)	-.148* (.078)	-.021 (.090)	.020 (.056)	-.036 (.071)	-.083 (.104)	-.110 (.073)	.103 (.075)	.289*** (.064)	.368** (.053)	.195*** (.063)
FE pooled	ln(number of employees)	.326 (.305)	.002 (.197)	-.649*** (.152)	-.355 (.309)	.067 (.132)	-.025 (.144)	.043 (.242)	.491** (.193)	.295* (.159)	.674*** (.0192)	.113 (.111)	.998*** (.171)

© Empty cells means that the coefficient is constrained to be zero, so assumed because in no specification it approached statistical significance.

Table 3. The Benchmark Results of PC Effects (Dep.=ln(value added per employee)): Controlling for Other Reforms

Sample:	Pooled Sample: both PC Participants and Non-PC-Participants				PC-participants Only			
	FE (2)	FE (3)	FE (4)	FE (5)	FE (7)	FE (8)	FE(9)	FE (10)
PC-Participants dummy (i.e. $d_i=1$) * number of years in the sample	.004 (.009)	.010 (.008)	.010 (.008)	.010 (.008)	No need once control for year dummies			
Dummy: the year before PC-adoption (i.e., D_{t-1})	-.013 (.030)	-.025 (.030)	-.025 (.029)	-.025 (.029)	-.004 (.029)	-.008 (.029)	-.007 (.029)	-.006 (.029)
$PC_{i,t-1}$	.076** (.038)				.017 (.039)			
$PC_{i,t-2}$		.005 (.042)				-.034 (.041)		
$PC_{i,t-3}$			.008 (.073)				-.013 (.070)	
$PC_{i,t-4}$				.038 (.103)				.06 (.029)
Number of observations	6176	6176	6176	6176	5471	5471	5471	5471
R square	.455	.455	.455	.455	.489	.489	.489	.489

Note. (a) Standard errors in parentheses. *, **, and *** represents significance levels of 10, 5, and 1 percent. (b) R^2 's reported under FE models are R^2 within. (c) In all the contests between OLS, RE, and FE specifications, the FE models win decisively. (e) Other control variables for all specifications include capital and labor terms by industry-size category, dummies of year 1984-1989, as well as other reforms: marginal profit retention rate and its missing indicator, firm wage elasticity and its missing indicator, the dummies of managerial wage discretion, of production autonomy, of the presence of new management.

Table 4. The effects of PC participation and provisions:

Dependent Variable = log(value added per employee)

	PC Participants, Non-PC-Participants		PC-participants Only	
	FE	first difference GMM	FE	first difference GMM
Dummy: Post-PC _{it}	.007 (.074)	-.087 (.057)	-.019 (.073)	-.106* (.018)
Post-PC _{it} * the number of years since the adoption of PC (T_{it})	.007 (.029)	.024 (.046)	-.035 (.029)	.072 (.045)
(Dummy: the firm is a PC participant, i.e., $d_i=1$) * years in the sample	.009 (.010)	.070** (.028)		
PC features				
(a) The manager appointed by bidding	-.150 (.105)	.067 (.069)	-.159 (.100)	.037 (.069)
Preceding dummy * T_{it}	.117** (.050)	.078 (.054)	.117** (.048)	.099* (.055)
Dummy: the firm eventually adopted a PC under which the manager was appointed by bidding	-.030*** (.011)	-.045 (.029)	-.028*** (.010)	-.032 (.030)
(b) The primary PC target being profit (instead of tax, quantity, etc)	.063* (.036)	.137** (.050)	.064* (.034)	.132*** (.050)
(c) PC adoption * markup ratio	-.205** (.092)	-.212 (.726)	-.181* (.094)	-.692 (.672)
(d) Firm-level wage elasticity	.197*** (.060)	.030 (.074)	.192*** (.057)	.037 (.070)
(e) The manager posted bond * T_{it}	.015 (.022)	-.021 (.032)	.006 (.021)	-.025 (.033)
(f) The duration of the contract * T_{it}	.002 (.005)	-.005 (.008)	.004 (.005)	-.008 (.008)
Number of observations	6166	3673	5461	3253
R square	.458		.492	
N*objective		65.98		60.35

Note. (a) Standard errors in parentheses. The GMM estimates are robust to heteroskedasticity. *, **, and *** represents significance levels of 10, 5, and 1 percent. In all the contests between OLS, RE, and FE specifications, the FE models win statistically. (b) Other controlled variables are the same as in table 3. (c) In choosing which terms (level, rate, or both level and rate) to include for PC participation and provisions in the level+rate specification, we drop the terms which are far from being significant; in addition, we keep at least one term for any provision; both level and growth rate effects of PC participation are always kept.

Table 5. Alternative PC provisions and Their Associated Productivity Level and Growth Rate Effects: a simulation based on the FE estimators of table 4 for the sample of PC-participants

PC provisions:	(1) % firms in this category for the participants used in the estimation	Before-After estimates (FE)	
Bare bones PC without other provisions (evaluated at the wage elasticity of zero and the mean markup ratio)	10.7	Level	-.120** (.053)
		Rate	-.022 (.022)
Bare bones PC + CEO by bidding (evaluated at the wage elasticity of zero and the mean markup ratio)	2.2	Level	-.279*** (.101)
		Rate	.094** (.047)
Bare bones PC + Primary PC target being profit (evaluated at the wage elasticity of zero and the mean markup ratio)	25	Level	-.056 (.056)
		Rate	-.022 (.022)
Bare bones PC + Manager posting bond (evaluated at the wage elasticity of zero and the mean markup ratio)	9.2	Level	-.120** (.053)
		Rate	-.017 (.031)
Bare bones PC + mean wage elasticity (evaluated at the mean wage elasticity of 0.42 and the mean markup ratio)	23 $\mathfrak{N}$	Level	-.039 (.047)
		Rate	-.022 (.022)
Bare bones PC + wage elasticity a standard deviation above mean (evaluated at the mean wage elasticity of 0.75 and the mean markup ratio)	3.1	Level	.024 (.052)
		Rate	-.022 (.022)
Bare bones PC + Reducing markup ratio by one standard deviation: (evaluated at the wage elasticity of zero and the mean markup ratio)	.1	Level	-.071 (.058)
		Rate	-.022 (.022)
Bare bones PC + CEO by bidding + Primary PC target being profit + Manager posting bond + contract being three years + Reducing the markup ratio by one standard deviation (evaluated at the mean wage elasticity of 0.42 and the mean markup ratio)	2.5	Level	-.134 (.099)
		Rate	.100** (.050)
Bare bones PC + CEO by bidding + Primary PC target being profit + Manager posting bond + contract being three years + Reducing the markup ratio by one standard deviation (evaluated at the mean wage elasticity of 0.75, and, the markup ratio of one standard deviation below the mean)	.0	Level	-.022 (.104)
		Rate	.100** (.050)

Note. Reported in the parentheses are the standard errors for the estimated coefficients. *, ** and *** represent significance levels of 10, 5 and 1 per cent respectively.

†. Here the mean or the percentile of wage elasticity is based on the PC-participants sample.

$\mathfrak{N}$. When evaluated at a positive wage elasticity, the percentage of firms refers to that with wage elasticity equal to or greater than the specified threshold. Note that when evaluated at 0 wage elasticity, however, it refers strictly to firms with zero wage elasticity.

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