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Empirical Effects of Performance Contracts:
Evidence from China
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Abstract.   Performance contracts (PCs)—contracts signed between the government and state enterprise managers—have been used widely in developing countries. China's experience with such contracts was one of the largest experiment of contracting in the public sector, affecting hundreds of thousands of state firms, and offered a rare opportunity to explore how PCs work. Our findings indicate that on average PCs did not improve performance and may have made it worse. But China's PCs were not uniformly bad. Surprisingly, PCs improved productivity in fully 38% of the participants. Successful PCs were those which simultaneously provided sensible targets, stronger incentives, longer terms, and were in more competitive industries. Selecting managers through bidding and requiring managers to post a bond against a target was not associated with performance improvement.

JEL code: D2, production and organization; L2, Firm objectives, organization, and market performance; and L3, nonprofit organization and public enterprises

I.   Introduction

Performance contracts (PCs) are widely used to reform state-owned enterprises (SOEs). Since France pioneered their use in the 1970s, PCs have been tried in more than 50 countries (Ghosh, 1997). The World Bank (1995) found 565 PCs in 32 developing countries as of June 1994, where they were principally used for large utilities and other monopolies, and another 103,000 in China, where they were also used for manufacturing SOEs.

This article analyzes the experience with PCs in China, the country that experimented most extensively with this tool. We define PCs broadly as written agreements between *SOE managers*, who promise to achieve specified targets in a given, usually short, time frame, and *government,* which usually promises to award achievement with a bonus or other incentive. PCs are thus a variant of pay-for-performance or incentive contracts, which are often used to motivate private managers. PCs have been suggested as a way to improve central government agencies (Mookerjee 1997),[1] as well as state enterprises (Jones, 1991, Ghosh, 1997).

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[1] New Zealand, for example, has used incentive contracts for ministries and other government bodies.

This paper, using a panel data set, analyzes the experience with PCs in roughly 500 Chinese SOEs. As a natural experiment, China's experience offered many advantages. First was the large number of contracts. The Chinese experiments with performance contracts could well be the largest experiment in contracting in the public sector ever conducted. Second, Chinese PCs exhibited interesting variations, differing in term length, targets, intensity of incentives, method of selecting managers, and whether the manager posted a bond as a pledge to improve performance. Third, the enterprises that signed PCs were in many different industries with large variations in size, capital-labor ratio, markup ratio, pre-contract performance and the level of jurisdiction of the government that owned them. Firms in this data set also faced different widely degrees of market competition (Li, 1997).

This paper addresses two questions. First, did PCs work? To what extent did Chinese PCs enhance firm productivity on average? Second, can PCs work? How was firm performance affected by different PC provisions, in particular, incentives, targets, bidding, contract length, managerial bonding, and the extent of product market competition? These are important questions given the wide use of PCs and the interest in ways to improve SOE performance when privatization is not an option.

Neither economic theory nor empirical evidence provides clear-cut answers to these questions. Principal-agent theory (see Ross 1973; Stiglitz 1974; Sappington 1991) suggests that PCs can be useful in reducing agency problem as long as they can systematically reduce information asymmetry and improve incentives. The principal (government officials in the case of state enterprises) 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 by its proponents as a device to reveal information and motivate managers to exert effort (Jones 1991, Ghosh 1997). Proponents also argue that the contract can translate the multiple objectives of the multiple principals who govern state owned firms (different ministries, the president, and the legislature) into clear targets measured by specified criteria and weighted to reflect priorities. Moreover, targets can be set to take into account circumstances where SOE managers have less control over their firms than comparable managers do 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.

However, several factors could reduce the positive effects on performance which PC proponents expect. The information problem may not be solved by contracts, because of lack of information on SOEs whose shares are not traded in a stock market and weak accounting and controls in developing countries. Empirical studies (Berle and Means 1932, Jensen and Murphy 1990, Crocker and Masten 1991) suggest that high-powered incentives are a problem even for private firms in developed countries. Owners may fear that because of information asymmetry they will not be able to measure achievement well and waste their bonus (Laffont and Tirole, 1993) or reward managerial self-dealing (Shleifer and Vishny 1994). In SOEs there may also be political barriers to paying successful managers considerably more than ministers or legislators.

Another problem common to state owned firms is that some of the multiple principles may derive benefits from objectives that run contrary to the PC's goal of improving efficiency. Politicians may benefit when SOEs maximize the employment of their constituents; bureaucrats might benefit from SOE activities that increase their power, prestige or perks (Shleifer and Vishny 1994). Many of these objectives are likely to be harder to contract on than profit maximization. Even if all parties agree, the full set of things they care about are rarely quantifiable (multi-task problem, Holmstrom and Milgrom 1991) and don't lend themselves to automatic or mechanistic types of contracts. But less formulaic contracts which leave grounds for interpretation ex post rely on institutions such as reputation, arbitration, or courts to reduce opportunistic behavior (Crocker and Masten 1991). These institutions are likely to be weak in developing countries and non–existent where one of the parties is the government.

Commitment (Williamson 1976 and 1985) is thus an especially severe problem for PCs because one of the signatories is the government. There are likely to be no neutral third parties with the power to compel government to meet its commitments. Furthermore, in developing countries the institutions that curb arbitrary actions by governments and bind administrations to the promises of their predecessors, such as checks and balances or reputation, are often weak (North and Weingast 1989, Levy and Spiller 1996). Managers may not exert effort if they expect government will renege on, for example, paying the promised incentive.

The existing few empirical assessments of PCs reached different conclusions. Song (1988) suggests positive outcomes based on experience in Korea, but he partly relied on employee and management opinions that could be biased. Trevedi (1990) finds that India's variant of a PC (Memorandum of Understanding) improved the dialogue between SOE management and government, but does not rigorously analyze their impact on firm performance. Ghoph (1997) examines econometrically the experience of 12 Indian companies in implementing PCs and finds positive effects; but he does not control for simultaneous reforms such as liberalization and government disinvestment. Nellis (1989)

finds ambiguous PC effects in France and many African countries, in part because at the time of the study the experience was still recent. Shirley and Xu (1998) find 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. The sample, however, was small (12 company cases in six developing countries), and limited to natural monopolies.

This study is, as far as we know, the first econometric study to systematically evaluate the productivity effects of performance contracts in China. More importantly, it is also the first study to relate PC effects to contract provisions along three dimensions suggested by the contracting literature: information, incentives and commitment.

Our primary findings are three. First, PCs on average were not significantly correlated with improvements in productivity in a large sample of competitive SOEs in China. In fact, on average PCs were found to have a negative and significant correlation with productivity when the endogenous nature of PC participation was taken into account. Our second finding is more surprising: PCs can improve productivity when they *simultaneously* specify sensible targets, offer strong incentives, and signal commitment, especially in a competitive environment. Thus, productivity was higher in more competitive firms whose PCs had higher wage incentives, longer terms, and profit-oriented targets. Bidding, in contrast, was associated with lower productivity, perhaps reflecting the design of the auctions or weak enforcement of bidding contracts. In addition, managerial bonding had no systematic effect on productivity. Third, most PCs in China did not include these productivity-enhancing provisions. Surprisingly, some 38% of the PCs were associated with productivity improvements.

Our analysis differs from other studies of Chinese contracts. Byrd (1991) describes in rich detail the principal types of performance contracts, and suggests some of the main advantage of PCs over the traditional mode of government oversight. He argues that the main problems with PCs are the strong bargaining power of managers, 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) 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. (1995) 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 quantitative importance of alternative specifications of performance contracts--such as

profit orientation, managerial bond, firm level pay sensitivity, competition, the length of contract term -- most of which Groves et al. did not examine.

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.

The Chinese government began to experiment with PCs for SOEs in the mid-1980s[2]. Not until 1986, however, did the government implement PCs on a significant scale; in fact, PCs became the national policy for reforming SOEs between 1987 to 1994.[3] In our data set, the share of state enterprises under PCs grew from 8 percent in 1986 to 42 percent in 1987; it then skyrocketed to 88 percent by 1989.

Table 1. Definitions and Descriptive Statistics of PC features:

	Definitions	Mean (standard error)
PC	Dummy variable: one if a firm was under a PC	
Conditioned on that a firm was under a PC:		
W.ELASTICITY	Firm-level wage elasticity, the percentage increase of total wage bill of the firm when the profit increase by 1 percent.	.525 (.314)
BID.INCUMBENT	Dummy variable that is one if the manager signing the PC was an *incumbent* manager.	.149 (.356)
BID.NEWCEO	Dummy variable that is one if the manager signing the PC was a *new* manager.	.020 (.136)
TERM	The length of the contract (in years).	2.858 (1.523)
PROFIT	Dummy variable that is one if the primary target of a PC was profit.	.427 (.495)

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[2] About Chinese SOE reforms, see Groves et al. (1994, 1995), Li (1997), Gordon and Li (1995), Jefferson (1994), Jefferson and Xu (1991), Jefferson and Rawski (1994), Johnson (1990), and Xu (1998a, b).

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

BOND	Dummy variable that is one if the manager of a PC posted a bond as a guarantee for performance targets.	.285 (.452)

We defined a firm as being under a PC if the questionnaire indicated the existence of a contract that the manager had signed with the government. We captured the differences between the types of contracts by analyzing the impact of the PC provisions summarized in table 1.

One provision was duration of contracts (LENGTH), which ranged from one to eight years. Seventy one percent of the contracts explicitly specified a wage elasticity (W.ELASTICITY) that was ex ante specified and remaining constant throughout the length of the contract; W.ELASTICITY is imputed from the questionnaire that asks the percentage by which total wages would increase when profits increased by 1 percent.[4] W.ELASTICITY varied from 0.1 for the 5th percentile to 0.8 for the 95th percentile, with a median of 0.6. The managers in charge of implementing the contracts could be selected by a number of methods: by the government, by election, or by bidding (BID)[5]. Bidding was used to select managers for 17 percent of the contracts signed. The more local the government in general, the more likely it was that bidding was employed to select the manager for the contract. Among firms whose managers were chosen through bidding, roughly 13% (11 firms) appointed new managers (BID.NEWCEO), and 87%, incumbent managers (BID.INCUMBENT).

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

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[4] This incentive measure differs from a related measure of wage incentive used in recent study of Chinese SOEs: bonus/total wage bill (Groves et al. 1994, for instance). This bonus share, observed both before and after PC adoption, was more susceptible to simultaneity bias. W.ELASTICITY, in contrast, was set ex ante and remained constant throughout the period, thus is less susceptible to simultaneity bias. Note that we have controlled for more complete list of reforms than similar studies using the same data (Groves et al. 1994, 1995; Li, 1997): for instance, another wage incentive is the managerial discretion to determine employee wage, which we control for but other studies do not. Viewing the bonus/wage bill ratio was a result of these reforms, and to avoid simultaneity bias, we do not control for the bonus ratio variable.

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

The targets specified by the PCs also varied in the weight given to output goals, tax receipts, or profits.[6] Almost 42 percent of firms in our sample reported that their PCs' primary target was total before-tax profits (PROFIT hereafter), another 28 percent reported profits plus taxes remitted to the government as the primary PC goal, and 26 percent, output quantity and value and labor productivity. 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

We draw on agency theory (Ross, 1973; Grossman and Hart, 1983; Laffont and Tirole, 1993) and contracting literature (Williamson 1983, 1985) to shed light on how the PC features might affect firm performance. A performance contract can be understood as a game between risk-adverse managers with disutility of effort, and a risk-neutral government with imperfect information about the managers' effort. Theory suggests 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.

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 that 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 information asymmetry, thus allowing the government to use strong incentives and reduce shirking. Hence bidding should be associated with better performance. As suggested by Groves et al. (1995), we also try to distinguish the effects when the bid was won by the incumbent or by a new manager. Incumbent managers' information advantage allows them to cherry-pick firms with better prospects. Consequently, we expect firms whose contract bids were won by incumbent managers to have better productivity than those won by non-incumbents.

Second, we expect that profit targets, by providing a more comprehensive signal about a firm's performance than targets based on taxes or output, 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

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

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. 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 that focus on profitability are more likely to improve performance than those focused on tax or output goals.[7]

Finally, we expect that PCs signed with firms facing more competition benefit from less information asymmetry and hence should be associated with higher productivity. In competitive sectors "there is more information about the circumstances in which the manager operates . . . competitive markets provide a richer information base on which to write contracts" (Holmstrom and Tirole, p. 96). As a result, shirking will be more evident in competitive firms relative to firms with more market power, and performance measures will be more meaningful there. Moreover, contracts with competitive firms may be more formulaic and easier to enforce, thus less likely to be exposed to renegotiations (Crocker and Masten 1991). In the empirical analysis competition was measured by the markup ratio (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--for instance, government revenue would drop--governments will try to assure that the incentive payment does not exceed the social gain from increased firm efficiency. Hence, when information asymmetry is severe, government will tend to set the incentive too low to motivate much improvement in performance. Thus, without solving the information problem, introducing incentives can bring about only limited change (Laffont and Tirole, 1994). In our analysis, the incentive we investigate is firm level wage elasticity. This incentive device has limitations. 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.

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[7] 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.

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 or optimal investment 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 and by bonding. Longer term contracts signal managers that government is more committed; managers then may invest with a longer time horizon and therefore expand the production possibility frontier. As for bonding, it has been suggested as a first-best solution to agency problems (Becker and Stigler, 1974; Williamson, 1983). Managers, concerned with the possibility of losing their bonds if performance tends out to fall under the targets, will naturally work harder. We thus 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 PCs 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.) We assume the following Cobb-Douglas production function (the use of translog production function produced quite similar results about PC effects):

$$y_{it}^j = \beta_0^j + \tau_t^j + \beta_L \ln L_{it} + \beta_k \ln k_{it} + \delta Z_{it} + \beta_R R_{it} + \alpha_{PC} PC_{it} + \phi_i + \varepsilon_{it} \qquad (1)$$

where i indexes firm, j indexes industry, and t indicates year. To take into account the potential differences in technology for firms in different industries, we decomposed the firms into four industries: chemical, light, machine, and material industries.[8] The variables are defined as follows (see the data appendix for details of variable construction):

y_{it}^j :   log(value added per worker) for firm i of industry j at year t (see the data appendix for
        the construction of variables and the associated deflators.)

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[8] We follow Li (1997), which uses the same data set. The data contains firms in 36 two-digit industries, but many industries contain too few observations to justify more disaggregated treatment.

β^j: industry-specific TFP level. [9]

τ_t^j :   separate year dummies for each industry (to control for industry-wide shocks such as overall credit cycle effects and other business cycle-related effects, and industry-wide technological progress). [10]

lnL_{it}:   the number of employees, excluding those absent for more than half a year.

lnk_{it}:   capital per worker with capital constructed by the perpetual inventory method.

$Z_{it}\,\delta$ captures the effects of other controls such as provincial-specific growth rates.

R_{it} :   other major reforms and changes in the market environment that were applied to both PC and non-PC participants, including marginal profit retention rates, managerial wage discretion (i.e., letting managers determine employee wages), delegating production autonomy to managers, the share of output under state plan, the presence of new managers, and reducing markup ratio.

PC: either the PC dummy or a vector of PC variables.

α_{PC} :   The corresponding coefficient of PC variable(s).

ϕ_i fixed effects for firm i, to capture all firm-specific time invariant factors to account for productivity.

ε_{it} :   the time-varying error term 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.).[11] Note that we include "other reforms" in the production function; *It is important to bear in mind that these are not part of the contents of PCs; they had different timing and cross-sectional incidence than PC variables, and were regarded as different reform measures by the Chinese government.* Since other studies based on the same data have found the importance of such reforms in explaining performance (Groves et al. 1994; Li, 1997; Xu, 1997), one has to control for these reforms to isolate PC effects.[12]

Empirical Issues

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[9] We have also tried allowing industry-specific coefficients for capital and labor as well as industry-specific TFP levels. The results about the PC effects—both for PC status and for PC provisions—remain quite similar.

[10] Statistical tests--for both our FE model and our OLS models--show that the year effects across different industries are different.

[11] See Jensen and Meckling (1979), Coase (1991), McMillan et al. (1989), and Holmstrom and Tirole (1989).

[12] This is the first study about PCs--as far as we know--that control for other reforms in disentangling the PC effects. See, for instance, Song (1988), Nellis (1988), Trivedi (1990), Ghosh (1997), and Shirley and Xu (1998).

One issue is whether the PC variables could be endogenous. We have already controlled for firm and industry effects, but the PC variables could still be correlated with the firm's time varying error term, ε_{it}. This seems unlikely since a large portion of the variation of PC provisions across firms was due to local governments' discretion in implementing PCs. We can see this in Table 2, which gives the R squares for regressions in which the dependent variable is a PC variable for firm i and the independent variable is the mean of that PC variable for a province, excluding firm i from the calculation of the mean. The R squares in general are large.[13]

Table 2. Explanatory power of provincial patterns of PC variables on firm-level PC features

PC^j	R^2	PC^j	R^2
PC	0.667	TERM	0.234
W.ELASTICITY	0.415	PROFIT	0.473
BID.INCUMBENT	0.127	BOND	0.318
BID.NEWCEO	0.009 [a]		

Note. The regression is $PC_{it}^j = \gamma_0 + \gamma_1$ (provincial mean of $PC_{it}^{\ j}$), where *j* means PC status or a PC feature.
   [a] The coefficient is low because only 11 SOEs had this feature at the end of the period.

To address the possibility that the PC variables are correlated with the time-varying productivity shock, ε_{it}, we conduct the Hausman's test for the PC variable(s), treating the provincial-year mean(s) of the PC variable(s) as the maintained exogenous variable(s). There is no good reason why these regional means of PC variables should be correlated with the time-varying error terms specific to the firm under consideration, especially since (i) we have excluded that firm in computing the means and have controlled for provincial growth rates, and (ii) we have controlled for provincial-trend and industry-time dummies. When a PC feature is found to be endogenous, we use a fixed-effects two-stage-least-square estimation.

A second issue is the possibility that the results are skewed by firm outliers. To address this we also present the fixed effects regression using the median firm. When the outlier problem is not serious, the median regression should generate similar results to an ordinary regression. But when the outlier problem is severe, median regression generally provides better estimates in predicting the central

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[13] Staiger and Stock (1994) have emphasized the need to check if the instruments are significant predictors for the endogenous variables. If not, the bias of instrumental estimates may also be quite large.

tendency of the dependent variable (Narula, S.C. and J.F. Wellington, 1982). Moreover, the behavior of median firm itself is interesting.

Effects of PC participation: Benchmark Results

For the empirical application, we deleted any observation (1) missing the dependent variable or (2) missing capital-labor ratio, or (3) having an unbalanced panel.[14] This leaves us with a balanced panel of 503 PC participants and 63 non-PC participants. Table 3 reports the benchmark results of how PCs on average affected productivity.

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

	Including "other reforms			Excluding "other reforms"		
	FE	FE-median	FE-2SLS [b]	FE	FE-median	FE-2SLS [b]
No. Obs.	5660	5660	5660	5660	5660	5660
R. Square	0.474	.	0.471	0.468	.	0.467
ln(capital-labor ratio)	0.138**	0.123**	0.141**	0.141**	0.132**	0.144**
	(0.025)	(0.024)	(0.025)	(0.025)	(0.022)	(0.025)
ln (number of employees)	0.105	0.013	0.104	0.137*	0.068	0.140**
	(0.071)	(0.053)	(0.071)	(0.071)	(0.050)	(0.071)
markup Ratio	-0.537**	-0.641**	-0.540**	-0.544**	-0.650**	-0.547**
	(0.077)	(0.047)	(0.078)	(0.077)	(0.045)	(0.077)
PC	-0.005	-0.038	-0.175**	0.050	0.020	-0.069
	(0.033)	(0.033)	(0.081)	(0.033)	(0.030)	(0.075)
Marginal Profit Retention Rates	0.104*	0.122*	0.104*			
	(0.061)	(0.065)	(0.061)			
Dummy: Production autonomy	0.053**	0.064**	0.072**			
	(0.026)	(0.028)	(0.028)			
Dummy: Manager had discretion	0.010	0.036	0.026			
to determine wage (W_discretion)	(0.043)	(0.043)	(0.044)			
W_discretion × year	0.029*	0.021	0.028*			
	(0.016)	(0.014)	(0.016)			
Dummy: the presence of new CEO (NewCeo)	0.057**	0.072**	0.077**			
	(0.025)	(0.028)	(0.028)			
NewCeo × year	0.037**	0.046**	0.043**			
	(0.008)	(0.007)	(0.008)			

Note. *, ** represent significance at the level of 10 and 5 percents. Reported in the parentheses are heteroskedasticity-corrected standard errors. (a) For all the specifications, other controlled variables include: industry dummies, industry-specific year dummies, province-specific growth rates; missing

[14] Roughly nine percent of observations dropped because of (3). They involve larger measurement errors because the deflator for the output and capital stock were imputed based on industry-year cells. In addition, markup ratio, an important feature with which PC status will interact, could not be reliably estimated. The estimation of markup ratio requires the data about *each* year's price information on outputs and inputs (see the data appendix).

indicators for marginal profit retention rates, for firm-level wage elasticity. (b) PC is considered endogenous, and was instrumented by the average percentage of PC participation in the province year that a firm was affiliated with; in computing the average, the firm itself was excluded.

Column (1) presents the FE estimates, which finds that PCs were not significantly associated with productivity. In column (2), we present the FE median regression, which yields quite similar results. The PC dummy is again negative and insignificant, though now with a larger magnitude, -4%.

Next we conducted Hausman's test, [15] to see if the PC dummy might be endogenous, using the percentage of PCs adopted in the province as the instrument. The P-value of the F-test statistic is 0.031, thus rejecting the exogeneity of the PC dummy at the five percent level.[16] This suggests that we should rely on the fixed-effects two-stage least-square (FE-2SLS) results in column (3). In this estimate the PC dummy has a negative and significant coefficient of -17.5%. The FE-2SLS results imply that firms probably signed PCs when their productivity was higher.

Some observers have been concerned that PCs might have provisions similar to other reforms and thus by controlling for other reforms we might reduce the possibility that PCs have a positive correlation with productivity. To check this possibility, we re-run the column (1)-(3) specifications *excluding* other reforms. The results, in column (4)-(6), still imply that PCs did not have positive effects, and in all specifications they are statistically insignificant. Thus, all our estimates suggested that PCs on average did not improve productivity. This finding is similar to that of Shirley and Xu (1998) for PCs in market economies. However, we still wish to know if PCs can work when properly designed. We next test whether some PC designs did improve productivity, and whether the ways different PCs affected productivity are consistent with our previous conjectures.

Effects of PC Provisions

To identify the effects of PC provisions we allowed the PC variables to affect both the

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[15] Under the null hypothesis that the PC dummy is uncorrelated with ε_{it}, both the FE and the FE-2SLS will be consistent though the FE will be efficient but the FE-2SLS will not. Under the alternative hypothesis, the FE is inconsistent but the FE-2SLS is consistent. The maintained exogenous variables are the province-year-mean of the percentage of firms participating in PCs.

[16] We implemented the test as follows (see Berndt, p. 379-380). First we regressed the endogenous variable with the included exogenous variables and the maintained excluded instrumental variables, and obtained the fitted value. Then we use FE to run the expanded regression equation with the fitted value of the endogenous variables included. The Hausman's test amounts to testing the significance of the fitted value. If significant, the suspected endogenous variable is rejected as exogenous.

productivity level and growth rate.[17] Where only the level or rate effect was found to be statistically significant, we kept only the significant term in the final specification. If neither was significant we kept at least one type of effect for each PC provision, choosing whichever was closer to statistical significance. The results are reported in table 4.

Table 4. Effects of PC provisions

	Including "other reforms"		Excluding "other reforms"	
	FE	FE-median	FE	FE-median
No. Obs.	5660	5660	5660	5660
R. Square	0.478	.	0.472	.
PC	-0.142**	-0.185**	-0.102*	-0.133**
	(0.058)	(0.052)	(0.057)	(0.055)
Firm-Level Wage Elasticity	0.205**	0.179**	0.225**	0.178**
	(0.062)	(0.056)	(0.062)	(0.060)
BID.INCUMBENT	-0.095**	-0.130**	-0.121**	-0.177**
	(0.046)	(0.047)	(0.046)	(0.051)
BID.NEWCEO	-0.408**	-0.356**	-0.385**	-0.367**
	(0.087)	(0.109)	(0.087)	(0.119)
TERM	0.014	0.020*	0.016	0.026**
	(0.011)	(0.010)	(0.011)	(0.011)
PROFIT (i.e., profit-oriented targets)	0.056	0.061*	0.056	0.049
	(0.037)	(0.033)	(0.037)	(0.035)
BOND × (years since the posting of	-0.007	0.006	-0.005	0.015
managerial bond)	(0.023)	(0.020)	(0.023)	(0.022)
PC × markup ratio	-0.036	-0.057**	-0.037	-0.042
	(0.033)	(0.028)	(0.033)	(0.031)

        *, ** represent significance at the level of 10 and 5 percents. Reported in the parentheses are heteroskedasticity-corrected standard errors. The Markup ratio is standardized (i.e., normalized to have a mean of 0 and a standard deviation of 1). For all the specifications, other controlled variables include: (1) "Other reforms" , labor, capital-labor ratio. These coefficients are not reported here; they are quite similar to those reported in table 3.   (2) Industry dummies and industry-specific year dummies, province-specific growth rates; missing indicators for marginal profit retention rates, for firm-level wage elasticity.

        The Hausman's test cannot reject the hypotheses that the PC variables are exogenous after we have controlled for "other reforms" and industry controls. [18] The maintained exogenous variables in the

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[17] There are plenty of PC-participants with sufficient post-PC histories to identify the rate effects. There were 15 firms with more than or equal to 6 years of experience, 16 firms with 5 years, 27 firms with 4 years, 265 firms with 3 years, and 313 firms with 2 years.

[18] As mentioned in an earlier footnote, the endogeneity test was done as follows: (1) regress the potential endogenous variables with respect to all exogenous variables (including excluded ones), and obtain fitted

test are the means of the PC variables for the province-year with which a firm was affiliated. In light of these results, we only report the FE and the FE-median regressions, in column (1) and (2) of table 4. A quick glance through column (1) and (2) suggests that the qualitative and quantitative conclusions are similar from both columns, though the FE-median regression provides better precision.

In both columns (1 and 2) the interaction term of PCs with the markup ratio is negative,[19] though only significant in the FE-median regression. This suggests that PCs were associated with better productivity in firms with lower markup ratios. Since we have controlled for the markup ratio in other reforms, the significance of this interaction term cannot be attributed to the effects of markup ratio itself, but to the complementary effects of competition on the contracts. The magnitudes indicate that a one-standard-deviation increase in the markup ratio is associated with a productivity improvement of roughly 4 to 6 percent. This finding is again consonant with Shirley and Xu (1998)'s finding that PCs did not improve productivity in natural monopolies (for which the markup ratio should be relatively high).

Bidding had a negative association with productivity, both for incumbent and new managers. The information asymmetry hypotheses put forth in Groves et al. (1995) is confirmed. Although both were associated with lower productivity, contracts under incumbent managers worked better than contracts under new managers, a fact consistent with the hypothesis that incumbent managers have inside information.[20] The failure of bidding in general may be because the auctions were not properly conducted, for instance, the process may not be transparent. This explanation is supported by the fact that incumbent managers won 83 of the 94 bids. Alternatively, bidding may only succeed when contract enforcement is effective, which is hard to achieve when government is one of the parties.

Our hypothesis that PCs with targets emphasizing profits should perform better than PCs focusing on taxes or quantity of output is supported. Although not significant for the FE results, the FE-median specification suggests that firms with profit-oriented PCs had a productivity advantage of roughly 6 percentage points.

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values, and (2) include these fitted values of the potential endogenous variables in the regression equation and run a second-step regression. If the fitted value of the a potential endogenous variable is significant, then its exogeneity is rejected; otherwise, it is not rejected (Wu, 1973; Hausman, 1978; Berndt, 1991). Note that the discussion here ignored the fixed effect specification; this is because in actual implementation, all variables are differenced with respect to their firm-specific means, therefore, the FE estimation is equal to an OLS. The p-values for the fitted values of the following PC variables are: PC, 0.520; W.ELASTICITY, 0.322; BID.INCUMBENT, 0.472; BID.NEWCEO, 0.796; DBOND×(year since posting managerial bond), 0.270; PC×markup, 0.079; PROFIT, 0.837; TERM, 0.780.

[19] Markup ratio was standardized to make interpretation easier. After normalization, the mean is zero, and the standard deviation is 1.

[20] The slight difference between our results and those of Groves et al. (1995) can probably be attributed to differences in measuring the outcome and the specifications. They used relative performance of a firm within an industry; and they did not control for capital, labor and other reforms.

Our hypothesis that PCs work better when they provide higher powered incentives is also supported: in both specifications, firm-level wage elasticity is positively correlated with productivity. The magnitude indicates that the productivity level of a firm signing a PC with a one-standard-deviation increase in wage elasticity (0.31) would go up 6 percentage points.

Finally, our hypotheses about commitment received mixed support. The duration of the contract, consistent with our hypothesis, had a positive association with productivity growth rates in both the FE and FE-median estimates, although it was only significant in the latter. An additional year in the length of the contract term is associated with a productivity increase of 1.4 to 2 percentage points. The posting of a managerial bond, however, was not correlated with productivity improvements in either specification. This finding could suggest that other conditions are necessary for bonding to secure managerial commitment, such as government's commitment not to agree to softer targets ex post. Alternatively, it could be that winning managers had the chance to reap such large positive private gains net of the bond posted that bonding gave PCs no additional positive impact.[21]

Once again we tested if the results would survive if "other reforms" were dropped (column 3 and 4). As is apparent from the table, the findings on PC effects remain largely intact, though in general less significant.

Comparison of Alternative PC Provisions

Finally we wish to know the effects of PCs with different combinations of provisions. Using the FE -median estimators in column (2) of table 4, we calculated the combined effects, reported in Table 5. (We get similar results if we use the FE estimates in column (1)). To simplify the presentation, for each dimension of PC provisions and market competition (mark up ratio), PC_j, we classify firms either as having PC_j^{good} or PC_j^{bad}.[22]

The diversity of PC effects appears to be enormous. The effect of a PC on productivity levels rises from -28% for a PC that used bidding, with no profit orientation, no managerial bond, short TERM, low W.ELASTICITY, and high markup ratio (see northwest region), to +15% for a PC without bidding

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[21] The winning managers seemed to have ample opportunity to reap private benefits. The managers had much more discretion in selecting managing positions in the firm and in hiring and firing decisions; as a result, many employees had more incentives to bribe the manager (personal observations of one author).

[22] In particular, for the dummy variables, good PC features were no bidding, targets with a profit orientation and managerial bonding and bad was the opposite. (The bidding dummy is based only on BID.INCUMBENT; we ignore BID.NEWCEO since it would yield similar, albeit somewhat larger, negative effects on productivity and had only 11 firms). For the continuous variables, a good PC feature was those above the mean for that provision or the markup ratio below its mean. The evaluation was conducted with the following numbers: W.ELASTICITY^{bad} = 0.16, W.ELASTICITY^{good} = 0.66; TERM^{bad} = 2.08, TERM^{good} = 4.28; MAKRUP^{bad} = 0.55, MAKRUP^{good} =-0.81. The values are the means for the good or the bad subsamples.

and with profit orientation, managerial bond, long TERM, high W.ELASTICITY, and low markup ratio

(see southeast region). Since a positive effect is more

Table 5. Distribution of imputed PC effects by contract type (total PC participatns=499 [a])

	TERM: low W.ELAS.: low markup: high	TERM: low W.ELAS.: low markup: low	TERM: low W.ELAS.: high markup: high	TERM: low W.ELAS.: high markup: low	TERM: high W.ELAS.: low markup: high	TERM: high W.ELAS.: low Markup: low	TERM: high W.ELAS.: high markup: high	TERM: high W.ELAS.: high markup: low
BID: yes PROFIT: no BOND: no	-0.276 (4.56)	-0.197 (3.27)	-0.181 (3.01)	-0.103 (1.69)	-0.231 (3.79)	-0.152 (2.51)	-0.136 (2.28)	-0.058 (0.96)
BID: yes PROFIT: no BOND: yes	-0.266 (4.50)	-0.188 (3.16)	-0.172 (2.86)	-0.093 (1.54)	-0.215 (3.11)	-0.136 (1.97)	-0.120 (1.73)	-0.042 (0.59)
BID: yes PROFIT: yes BOND: no	-0.214 (3.49)	-0.136 (2.24)	-0.120 (1.96)	-0.041 (0.68)	-0.169 (2.68)	-0.091 (1.45)	-0.075 (1.20)	0.004 (0.06)
BID: yes PROFIT: yes BOND: yes	-0.205 (3.44)	-0.081 (1.32)	-0.110 (1.84)	-0.032 (0.53)	-0.153 (2.18)	-0.075 (1.07)	-0.059 (0.83)	0.020 (0.28)
BID: no PROFIT: no BOND: no	-0.146 (3.49)	-0.067 (1.55)	-0.051 (1.22)	0.027 (0.61)	-0.101 (2.38)	-0.022 (0.51)	-0.006 (0.15)	0.072 (1.66)
BID: no PROFIT: no BOND: yes	-0.136 (2.84)	-0.058 (1.16)	-0.042 (0.85)	0.036 (0.71)	-0.085 (1.33)	-0.006 (0.10)	0.010 (0.15)	0.088 (1.33)
BID: no PROFIT: yes BOND: no	-0.084 (1.90)	-0.006 (0.13)	0.010 (0.23)	0.089 (1.93)	-0.039 (0.84)	0.039 (0.82)	0.055 (1.21)	0.134 (2.84)
BID: no PROFIT: yes BOND: yes	-0.075 (1.51)	0.003 (0.07)	0.019 (0.38)	0.098 (1.88)	-0.023 (0.35)	0.055 (0.82)	0.071 (1.07)	0.150 (2.20)

For each cell, the first row refers to the increase in productivity level associated with a specific PC provision. The second number (in parentheses) is the *t statistic* of the level effect.

Here are the values used for imputing the PC effects in this table: (1) BID: yes, 1; no, 0. (2) PROFIT: yes, 1; no, 0. (3) BOND: yes, 1; no, 0; (4) TERM: low, 2.08 years; high, 4.28 years. (5) W.ELASTICITY: low, 0.16; high, 0.66. (6) markup: high, 0.55, low, -0.81.

The growth rate effects of BOND is translated into level effects by the following formula: $\beta_{bond} \times (1 + TERM) / 2$, which is the average level effects of BOND over the period.

[a]. We excluded those firms with bidding won by new managers (in total 11 firms). Its effects will be similar to those bidding firms won by incumbent managers except that there is an additional productivity disadvantage of -23 percentage points.

likely to appear when one sees cells in which W.ELASTICITY is high, the markup ratio is low, the term is high, the primary target is profit-oriented, and no bidding is involved, these five elements seem to be crucial to the success of PCs.

Finally, we projected the effects of PCs on total factor productivity (TFP) using the coefficient for each provision in column (2) of table 4, and the actual combinations of PC provisions used in China.[23] We projected that PCs on average would reduce TFP by roughly 4%. PCs had positive effects in about 38% of the PC-participants, and for these firms the mean gain in TFP attributable to PCs was 6.5%, with a standard deviation of 4.2%. For the other 62% of firms, the mean TFP loss from negative PC effects was –10.3%, with a standard deviation of 7.9%.

V.   Conclusions

Our analysis suggests that PCs on average did not improve the productivity of state enterprises in China. We also find that PC provisions mattered: PCs can improve productivity when they provide high powered incentives, use targets less vulnerable to information problems (profit orientation), and signal commitment through longer terms – and when they are implemented in a more competitive environment. The absence of these good features in PCs can hurt productivity.

Our findings send a mixed message. On the one hand, the fact that on average PCs had negative effects, coupled with our similar earlier findings in six other countries, urges caution in using PCs as a tool to reform SOEs, and may explain why China stopped signing PCs after 1994. On the other hand, PCs, when properly designed and implemented, can indeed improve productivity.

A surprising finding in light of Shirley and Xu (1998) is that PCs had positive effects in fully 38% of the participants. Without further study of the political economy of incentive contracts in government settings, we can only conjecture about why most contracts were poorly specified but many were not. Lack of knowledge about how to specify an efficient contract is not very plausible explanation

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[23] The rate effect of managerial bonding is transformed into level effect by $0.04 \times BOND \times (1 + TERM)/2$, where 0.04 is the FE-2SLS estimates of DBOND $\times$ (year since posting managerial bond).

under the circumstances. Some of the reasons we discussed earlier may provide better explanations. One is that politicians or bureaucrats structured PCs to maximize their political benefits or rents rather than productivity (Shleifer and Vishny, 1994; World Bank, 1995). This explanation seems especially feasible given that PCs failed across many different institutional settings and contract designs. Successful PCs might arise when politicians or bureaucrats are constrained, by a hard budget constraint, perhaps.

A related explanation for poor PCs is that government's multiple principals may have targeted many goals (profitability, investment, worker benefits) such that the PCs' targets deviated from productivity, or the contracts incentives were lowered to avoid maximizing one goal at the expense of the others (multitask problem as in Holmstrom and Milgrom, 1991). The fact that SOE shares are not traded even where stock markets exist and the absence of good accounting practices may have given SOE managers an information advantage and bargaining power that PCs could not circumvent. Thus, our earlier research found evidence of strong managerial bargaining power in decisions about performance targets in six developing market economies (Shirley and Xu, 1998). This information asymmetry may also explain the generally low power of incentives since government would be reluctant to risk wasting its bonus if achievements cannot be measured (Laffont and Tirole, 1993).

To what extent are our results applicable in other countries? We believe the message is general. First, 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 most developing countries governments intervene widely in SOE operations, extend them protection from competition and bankruptcy, and provide subsidies and debt relief. Second, our study of PCs in China produced results strikingly similar to our earlier analysis of PCs in six market economies (Ghana, India, Korea, Mexico, the Philippines, and Senegal). Finally, we postulated that the productivity effects of PCs are a function of how well the contracts addressed 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 attempted to control for as many aspects of the unobservable as we could, such as other reforms, the competitive environment, etc., all of which should reduce the influence of factors special to China in our results.

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 (Shanxi, Jilin, Jiangsu, and Sichuan). The 769 firms constitute a stratified random sample of all SOEs in manufacturing. There was substantial variation in the size of these SOEs: the median SOE had 930 employees, the SOE at the 10th size percentile had 304, and that at the 90th percentile had 3175.

The data set has two parts. Part one is a quantitative table filled out 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 answered 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 Key Variables for the 1980-89 Data Set

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 expressed in 1989 market values. We assume 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, we assumed equal yearly inflation rates. For 1989, since we did not observe equipment inflation, we used the output inflation rate in the machine industry as a proxy.

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

We computed the composite price index for capital goods 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.) Following Li(1997) and Gordon and Li (1994), we did not use the net value of capital stock as the base to compute capital stock because it "tends to exaggerate the increase in enterprise capital 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.

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.[24] The capital stock for the following years is then constructed by the following formula:

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

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 the 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 $(P_{Q\,t-1}, P_{M\,t-1})$ gives the following formula for the percentage price increase of value added based on those of output and of intermediate inputs:

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

(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 computed the average year-to-year mixed price inflation rates for their industry-year sample, then assigned that value as the imputed mixed price inflation rate. Then, we converted them to a cumulative mixed price index.

We then estimated the market output price index. 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 were assigned their industry-year averages.

These price indexes were then used to compute the gross value of output (GVO). The survey reports GVO in current mixed prices. We first obtained GVO in current market prices by multiplying the reported GVO by the ratio of market output prices to mixed output prices in year t. That number was then translated into GVO in 1989 market prices by multiplying it by the ratio of the market price index in 1989 to the market price index in year t.

Price index of Intermediate Inputs

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

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

The data set has detailed information about the plan and the market prices of the two primary materials but it does not provide information about energy and other intermediate inputs. We therefore computed price indexes for intermediate inputs based on the assumption that the inflation rate for intermediate inputs was 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 was missing: roughly 40 percent of the answers were useful.

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

The quantity of intermediate inputs was then computed using these price indexes. We first obtained the quantity of intermediate inputs valued at the current market price by multiplying the reported intermediate inputs--in current mixed prices--by the ratio of the current market price to the mixed price of intermediate inputs. This number in year t was then translated into intermediate inputs in 1989 market prices by multiplying it 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 on 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 were identical in 1989 within an industry, but differed across the four. The second term was calculated by assuming that the change in markup ratio was 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, the enterprise-specific inflation in input prices).[25] Thus, the markup ratio, though assumed to be a industry-specific constant in 1989, is allowed to vary across firms and over 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 ratios for the rest of the industries are $(1/0.35) * \mu_j$, respectively.

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

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

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

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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