The Productivity Effects of Stock Option Schemes: Evidence from Finnish Panel Data[1]

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Abstract:
While theorists differ sharply on the expected economic impact of stock options, typically empirical work has found a positive association between option schemes and firm productivity. However, existing data are limited and may not enable reliable investigation of the productivity effects. New panel data for all Finnish publicly listed firms during 1992-2002 enable us to distinguish plans that are broad based and others limited to particular employees and address issues of endogeneity concerning inputs and options. Diverse specifications are estimated including dynamic panel data models with a GMM estimator. For broad-based option scheme indicators the key result is that different estimators consistently find statistically insignificant associations with firm productivity. For selective option schemes, the baseline fixed effects estimator suggests a 2.1-2.4% positive and statistically significant effect on firm productivity. However, in empirical models in which endogeneity and dynamics are taken into account, no evidence is found of a link with firm productivity. Thus our findings are consistent with hypotheses that predict negligible effects of option plans for enterprise performance, such as those based on free riding, or psychological expectancy theory or accounting myopia. However, by finding weak evidence for productivity effects of targeted schemes (and none for broad-based plans) our findings tend not to support hypotheses based on managerial rent seeking.

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1. Introduction

During the 1990s, stock options became an increasingly popular compensation method in many countries (e.g. Hall, 1998; Murphy, 1999). Initially, option programs were typically allocated, almost without exception, only to executives.[2] But this association of stock options mainly with managerial compensation changed rapidly after more and more companies worldwide started to issue stock options to the workforce more broadly (e.g. Weeden, Carberry and Rodrick, 1998; Lebow, Sheiner, Slifman and Starr-McCluer, 1998; and Blasi, Kruse and Bernstein, 2003). In turn this growth of stock options has generated heated public discussion with some viewing stock options as a device by which managers transfer excessive benefits to themselves, while others see options as a major innovation in managerial and personnel compensation.

The growth of options has also been accompanied by a mushrooming of theoretical and empirical literature on stock options (e.g. Ittner, Lambert and Larcker, 2003). Whereas sharp disagreements exist among theorists on the expected economic impact of different types of option schemes, existing empirical work consistently finds that firm performance is enhanced by stock options. However, the findings based on existing empirical work are potentially limited. Empirical analysis has been forced to rely on data that may not be representative, typically are for only short time periods and are mainly for the U.S. and the U.K. (e.g. Conyon and Freeman, 2004; Sesil, Kroumova, Blasi and Kruse, 2002). Moreover, existing data may not enable a careful investigation of the productivity effects of different types of option plans.

By contrast in this paper we use new panel data that we have assembled. The data include all Finnish publicly listed firms during a relatively long period, namely 1992-2002. Thus it enables us to see if previous findings based mainly on evidence generated using US and UK data, are applicable to another country which once had a very different system of corporate governance but which has recently moved to adopt an Anglo-Saxon model. In our empirical work we estimate Cobb-Douglas production functions with different stock option program indicators. Furthermore, whereas earlier empirical literature has used cross-section and fixed effects models, we also address the potentially important issue of endogeneity of inputs and options by estimating dynamic panel data models with a GMM estimator.

For broad-based option scheme indicators the key result is that different estimators consistently find a statistically insignificant association with firm productivity. For selective option schemes, the baseline fixed effects estimator suggests a 2.1-2.4% positive and statistically significant effect of the dilution indicator on firm productivity. However, in empirical models in which endogeneity and dynamics are accounted for, no evidence is found of a link with firm productivity. Insofar as our findings do not provide support for hypotheses of a positive association between option schemes and firm productivity, our findings differ in important ways from earlier findings that are based on less rigorous methods and more limited data.

This paper is organised as follows. Section 2 provides the conceptual underpinnings for the study and also surveys relevant empirical research on the productivity effects of stock options and related schemes. In section 3 we describe the institutional framework and our most unusual data. Section 4 outlines our empirical strategy and this is followed by a presentation of our findings. In the final section we provide conclusions and implications of the paper.

2. Conceptual Framework and Previous Empirical Work

A key argument of proponents of stock options is that options can align the interest of employees and shareholders. For example, stock options may motivate employees to exert more effort and take actions that are mutually beneficial to both owners and employees. The motivational effect of stock options may be especially relevant in situations where alternative approaches, such as direct monitoring or piece rates, are not feasible, perhaps because of monitoring difficulties (Sesil, Kroumova, Blasi and Kruse, 2002).

Stock options change the compensation structure so that employee total compensation increases in good times and decreases in bad times. This has several consequences for labor turnover and firm survival. Stock options have been deemed crucial in recruiting and retaining employees, especially in markets where employees are potentially highly mobile (Rousseau and Shperling, 2003). Stock options may also help to retain key employees, both because they adjust pay according to current market conditions (Oyer, 2004) and because they are a deferred form of compensation. Finally, stock options may prevent inefficient firm closures, since they substitute for contractual payments and hence save cash in bad times (Inderst and Müller, 2005).

However, there has also been criticism of stock options. For example, stock options, when exercised, entail a cost to shareholders in the form of dilution of ownership. Also others note that the costs of stock options have not been included in income statements (e.g. Hall and Murphy, 2003.) Hence they argue that the increasing popularity of options in part reflects firms' mistakenly thinking that options are a cheaper form of compensation than their true cost.

Also, a potential performance impact has been questioned. Payment schemes that reward collective performance suffer from the free-rider problem: an individual who increases his effort will bear the full cost of the increase in effort, but will realise only a small part of the resulting increase in output (e.g. Alchian and Demsetz, 1972; Oyer, 2004). Another criticism comes from psychological expectancy theory (Vroom, 1995). According to the "line-of-sight" argument, rewards based on performance can only be motivating if, by their actions, employees can influence the measures on which performance-pay is based. This is typically not the case with stock option plans, where employees (with possible exception of top executives) can hardly perceive any direct link between their actions and the share price performance.

Both free-rider and line-of-sight arguments have been countered in the literature. First, when rewards are based on group performance, according to Kandel and Lazear (1992) it is in the interest of individual employees to develop a group norm where the employees monitor the performance of their peers and prevent free-riding behaviour. Second, equity schemes may create a common bond or "psychological ownership" among employees and thus change their behaviour so that it would match the collective interest (Pierce, Rubenfeld and Morgan, 1990; Baron and Kreps, 1999).

Another cost related to equity schemes is that they increase the risk employees are facing, since employees have both a substantial proportion of their financial capital and human capital invested in one workplace. Since they do not value their options to the same extent as an outsider would, employees may require higher total compensation (Meulbrok, 2002). However, there can be important differences between executives and employees in these respects. In the compensation of top executives, the line-of-sight and diversification problems are not as severe as with lower-level employees (Hall and Murphy, 2003). On the other hand, executive stock option compensation may be motivated by rent-seeking activities (Bebchuk and Fried, 2003).

Ultimately the impact of stock options on business performance is an empirical question. However, when turning to empirical work, it appears that there is only a limited amount of work that examines the consequences of stock options for firm-level performance.[3] Conyon and Freeman (2004) examine the economic outcomes of broad-based option schemes in a sample of UK listed firms during 1995-1998, i.e. during a period when the stock market was rising. They use three survey data sets for 284 firms, estimate fixed effects regressions and find evidence that the presence of a stock option plan is significantly associated with higher firm-level productivity. Sesil, Kroumova, Blasi and Kruse (2004) use survey data on broad-based option schemes from different sectors in the US provided by the National Center for Employee Ownership. They find that, in 1997, stock option firms had 28% higher productivity than non-option firms and 31% higher productivity than their non-option pairs. However, the response rate of the survey is only 10% yielding 73 firms in the final data set. In a subsequent study the same four authors (Sesil, Kroumova, Blasi and Kruse, 2002) compare the performance of 229 new economy firms, which offer broad-based option schemes to their non-stock option granting counterparts. They find evidence that productivity is higher in firms with broad-based plans. Their research methods include descriptive analysis, paired matching comparisons between broad-based and non-broad-based options firms within the same industry, and a cross-section regression for 1997. Ittner, Lambert and Larcker (2003) use survey data to examine 217 new economy U.S. firms during 1999-2000. By using cross-section analyses they examine the performance consequences of option and equity grants to senior-level executives, lower-level managers, and other employees. Their findings indicate that lower than expected option grants and/or existing option holdings are associated with lower accounting and stock price performance in subsequent years.

In sum the empirical evidence from these studies suggests the existence of a positive and often quite sizeable link between stock option plans and productivity at the firm-level. In turn, this implies that the available evidence provides support for theorists who predict that potentially powerful economic effects will flow from options and dominate the effect of factors such as free riding, accounting myopia and managerial rent-seeking. However, before accepting this conclusion it is important to note some key shortcomings of these studies. For one thing, in all of these studies, often the data are based on surveys and are apt to suffer from various selection biases, for example three studies focus only on new economy firms. Second, nearly all studies are limited insofar as they concentrate on the time-period before the stock market collapse in 2000. Third, and at odds with most theory, no studies are able to reliably distinguish the productivity impact of selective versus broad-based plans. Fourth, the econometric methods that the available data enable researchers to use are sometimes less than desirable. Thus some studies have access only to cross-sectional data, and none appear to seriously address the potentially highly important issues of endogeneity of option schemes and inputs used in a production process.

While there are only a small number of empirical studies on the impact of stock options on firm performance, the empirical literature on the productivity effects of other forms of employee financial compensation that are alternative to the traditional fixed-wage arrangements, such as *employee profit-sharing* and *employee stock ownership plans (ESOPs*) is quite large.[4] Consequently, it is useful to briefly highlight some key issues and findings in that literature since this may help to shape the empirical strategy we adopt in this study.

Typically studies of firms with *employee profit-sharing plans* find a positive relationship between profit-sharing and firm productivity. This is the conclusion of several surveys including, for example, Weitzman and Kruse (1990) and Jones and Pliskin (1991). This typical finding emerges from the empirical studies that employ diverse methods to investigate profit sharing arrangements that exist in a variety of institutional settings including the former West Germany (Cable and Wilson, 1990), the UK (Wadhwani and Wall, 1990), the US (Kruse, 1992) and Finland (Kauhanen and Piekkola, 2002). Findings based on studies of firms with *employee stock ownership plans* also typically support the existence of a positive relationship between ESOPs and firm productivity or performance. However, as many surveys point out (e.g. Kruse, 2002), the evidence in support of this positive link is probably less robust than for profit sharing. Again there is evidence that employee stock ownership can be positively associated with enhanced business performance in a variety of institutional settings including Japan (Jones and Kato, 1995) and the U.S. (Kumbhakar and Dunbar, 1993).

However, this other literature also draws attention to the potential sensitivity of findings to several factors. Prominent among these is the need to have data that are not distinguished by various kinds of selection bias, a common problem with survey data. Another key matter is the issue of institutional detail—the form of the ESOP or profit sharing arrangement often matters. Also, some theorists argue that for sustained effects on enterprise performance, financial participation must be accompanied by changes in decision-making participation. Hence a failure to include controls for such other factors may lead to empirical models that are misspecified (e.g. Conte and Svejnar, 1988). Finally, there is the issue of the appropriate econometric approach. Amongst several potential matters, the sensitivity of findings to potential issues of endogeneity of plan schemes and inputs used in production is clear.

In devising our empirical strategy we will respond to these issues that are highlighted by the concerns as best we can. Our data will enable us to address most of these matters. The only exception is our lacking data for other HR practices such as participation in decision-making. Because we utilise public firm-level data on stock option plans, we cannot control for the level of employee participation in decision-making, the existence of profit sharing, and other human resource management practices. We can, however, separate all constant firm- and common time-specific effects from several other factors that possibly have effects on firm productivity by using fixed effects and GMM estimators.

3. Institutions and the Data

In this section we describe the institutional context and the data. To examine the impact of stock option compensation on firm productivity we assemble new panel data for Finnish firms for 1992-2002. This was a particularly turbulent period in Finnish economic history. In 1990, Finland had just entered a deep depression, which was the most severe of any OECD country since the Second World War. In 1995, Finland joined the European Union and, in 2002, adopted the common European currency, the Euro, in the first wave of adoptions.

In industrial relations, the most marked change during the period of interest was the increased use of performance-related pay (Kauhanen and Piekkola, 2002). On the other hand, collective bargaining and centralised income agreements remained intact. The unionisation rate was between 70 and 80 percent throughout the period.

The increase in stock option compensation reflects a deep change in the Finnish corporate governance system. In the end of 1980s, the Finnish corporate governance system in listed firms was very much bank-centred and resembled the German system. The stock market started its recovery after the recession in 1993, and the importance of the equity market in financial intermediation grew throughout the 1990s. Both the turnover and market value of firms listed on the stock exchange increased dramatically throughout the decade, with Nokia leading this development. In the end of the1990s the Helsinki Stock Exchange saw a wave of new listings.

Now stock markets are much thicker, more transparent and arguably provide more reliable information than in the past. At the same time, both monitoring of insider trading and legal punishments have become stricter. During the last 10-15 years Finland has shifted from a bank-based financial intermediation closer to a market-based Anglo-American system. As part of this institutional change publicly listed Finnish firms have adopted stock option schemes extensively in the 1990s. As discussed below, the most active period of stock option adoption coincided with the height of the stock market boom in the late 1990s. However, as stock market prices started to fall after May 2000, accelerating further in 2001 and 2002, the rate of stock option adoption decreased markedly.

All of our firms are traded on the Helsinki Stock Exchange (HEX). However, firms that were on the two smaller lists (i.e. the Over-the-counter and the Stockbroker's list), that were maintained by investment banks and stock brokerage companies, are excluded before 1997 due to their rather low economic significance compared to the main list. Since 1997 HEX has taken over the smaller lists and also has started to operate two additional lists besides its main list: the "I" (Investor) -list and the "NM" (New Market)-list. The "I-list" consists of firms that are traded infrequently and are often majority-owned by large investors. The "NM" list consists of smaller IT and high technology firms, similar to the NASDAQ or the Neuer Markt in Frankfurt. Thus, we have information on the presence of option schemes on the main list throughout the period and on the minor lists since 1997. However, we do not have option program information on firms that have not been listed in the HEX, since our option data are based on public information of listed firms. We are aware that some unlisted Finnish firms have adopted option schemes, at least during the bull market at the end of the 1990s. Unfortunately, there was no option data information available for these firms.[5] We expect that these programs were more likely to be located within the ICT sector than in other sectors. We believe, however, that the set of these unlisted firms is moderate, since stock option compensation works properly only in situations where the value of shares can be assessed by the stock market.

[Table 1 about here]

Our panel data on stock options were initially organised by Professor Seppo Ikäheimo from the Helsinki School of Economics. However we have used several sources to complement and update the original data. These include annual reports, stock market releases and option data obtained from Alexander Corporate Finance, an investment bank. We use these data to briefly describe the general evolution of Finnish stock option plans during 1992-2002.[6]

Column (1) in Table 1 gives the total number of firms during the period. The number of firms at the HEX fluctuates considerably, which partially relates to the business cycle. Column (2) describes the number of new option plans. The early peak year is 1994 (21 new plans). Then the number increases from 1997 (22) until 2000 (61). Thereafter it drops to 33 for 2001 and 2002. Column (3) shows the development of new broad-based plans. The introduction of such schemes is concentrated during the years 1999-2000, when one-half of new plans were broad-based. In Columns (4) and (5) we approach this issue from another angle and provide time series data on the existence of option schemes. In these columns, we also use information on the timing of the scheme as well as on the launching of the scheme. In Column (4), a quarter of listed firms had an option scheme in 1993. This proportion jumps to around 50% in 1994, where it stays until 1996. After a temporary drop in 1997, the proportion increases from 1998 (58%) until 2001 (77%). In 2002 74% of listed firms had an existing scheme. Column (5) shows the development for broad-based plans. Roughly 3-8% of listed firms had an existing broad-based scheme in 1992-1997. This fraction steadily increases until 1998-1999, and stays around 36% in 2000-2002.

By combining the option data set with firm-level financial statements obtained from Balance Consulting, a consulting firm, we assemble firm-level panel data for 117 publicly listed firms from 1992 to 2002.[7] For each firm there are between four and eleven observations. Finally, we deflate all our nominal monetary variables to real euros for 2000 by using industry-specific gross output deflators, published by Statistics Finland. Table 2 summarises the pattern of our panel data.

[Table 2 about here]

In the analysis that follows, we distinguish between broad-based and selective schemes. The latter are mostly managerial schemes, although they can also include other key personnel (e.g. R&D workers). However, in order to qualify as a broad-based scheme, all employees (or at least a great majority) should be eligible. The classification is based on public stock exchange reports.[8] Finnish Law on Joint Stock Companies requires firms to report all relevant conditions about stock option schemes to shareholders prior to adoption. While a high rate of eligibility does not automatically guarantee a high participation rate, there are good reasons to believe that these are closely connected. For one thing, employees usually face only small costs when they subscribe to options—e.g. by providing a zero-interest loan to the company, with the company repaying the loan at face value after a certain period, usually 1-3 years. Thus, while employees face a cost in terms of foregone interest and liquidity, typically this cost is far below the real value of the options. Moreover, not all companies use this procedure, but rather they essentially give options to employees for free.[9]

In our empirical work, we develop three option program indicators. These measures reflect the presence or absence of an option scheme, the size of the scheme and whether the scheme is selective or broad-based. Two measures are binary variables and one is a continuous variable.[10] Our *first binary indicator* is *opt* measuring the presence of a scheme in a firm in given year t. It equals one for the group of option firms and zero otherwise. Thus, the indicator distinguishes option and non-option firms allowing us to examine the average impact of the presence of options on firm productivity.

Our *second binary indicator* also measures the presence or absence of a plan but it distinguishes between selective *(ssopt)* and broad-based *(bbsopt)* option plans. By a selective plan we mean a scheme that is targeted to a selected group of employees. These schemes include managerial programs, but also schemes that are targeted to key personnel. Broad-based plans are all encompassing, including managers, but they do not have to be egalitarian in the sense of all participants having the same number of options. By using these distinct dummy variables we can examine whether the average impact of plans on firm productivity differs between selective and broad-based option schemes.

Our *third program indicator* is potential dilution *(dilu)*. This indicator measures the potential size of effective schemes in firm *i* in year *t*.[11] This is a continuous variable—the ratio of the number of shares that may be awarded through effective stock option plans in a given year divided by the sum of total number of shares and the number of new shares that may be awarded through options at the end of a year. If a program ends in the middle of the year *t*, then the year *t-1* is the last year used in calculating dilution. The indicator distinguishes option and non-option firms allowing us to examine the average impact of options on firm productivity. To investigate whether the productivity impact varies by plan characteristics, we also use separate dilution indicators, namely *diluss* for selective and *dilubb* for broad-based plans. To capture possible dynamic effects of a program, we also use once lagged dilution indicators.

It is worth stressing that our panel data include almost all listed Finnish companies during the period 1992-2002. We exclude only a few firms with less than four consecutive observations. This is mainly because there is some entry and attrition of listed firms at the Helsinki Stock Exchange (HEX). Also, some firms merged during the period. In this case, we have included only merged firms and excluded all information prior to the merger. Also, we exclude a firm if data for a key variable such as value-added are missing. Finally, to exclude potential outliers, we delete observations where: employment is less than 50 (32 firm-year observations); fixed capital is less than €1,000,000 (23 firm-year observations); or employment is more than 50,000 (4 firm-year observations). Table 3 presents summary statistics.

[Table 3 about here]

Table 4 presents the key variables grouped by a firm's option program adoption status. We observe that option firms have higher value added, bigger labour forces and they also use more fixed capital compared to firms without stock option schemes. For example, the mean value added for selective scheme firms is 496 million euros, whereas for broad-based firms it is 166 million euros and for non-option firms only 105 million euros. Table 4 also shows that large Finnish firms have preferred targeted schemes to broad-based option schemes. Finally, the mean value added per employee is about 3.4% higher in selective scheme firms than in broad-based firms (57,064 euros compared to 55,205 euros).

[Table 4 about here]

4. Econometric Strategy

We test two key hypotheses, namely: (i) that firm-level productivity is expected to be higher in option than in non-option firms; (ii) that the impact of options on firm productivity is expected to be dependent upon whether the plan is broad-based or selective. Our basic empirical strategy is to use a production function approach and panel data estimators. First, we estimate a series of baseline fixed effects estimators by assuming that all explanatory variables are strictly exogenous. Second, we estimate dynamic panel data GMM estimators to account for the potential endogeneity of a firm's decisions on inputs and option schemes. The following issues have influenced the specific empirical strategy we adopt.

First, we assume a Cobb-Douglas form of technology, since it has been used frequently in the related literature such as the evaluation of the effects of ESOPs on firm productivity (e.g. Jones and Kato, 1995) and when analysing the effects of stock options on firm performance (e.g. Conyon and Freeman, 2004.) Second, although the Cobb-Douglas functional form is more restrictive than other functional forms such as the translog, we prefer the Cobb-Douglas production function since, when accounting for endogeneity of inputs and options in GMM models, the instrument matrix Z_i may become sizeable under the translog specification, thereby biasing estimates in finite samples.[12] Third, we assume that option schemes may only have a direct impact on firm productivity.[13] Fourth, since we do not have information on the detailed terms of option schemes, such as the exercise prices of options, we must bypass potentially important matters surrounding this issue.[14]

There are two reasons for using the fixed effects estimator in our baseline estimates. In part this is pragmatic – we use the fixed effects estimator because it has been used in the previous studies. For example, the estimator has been used in assessing the productivity effects of ESOPs (e.g. Jones and Kato, 1995) and stock options (e.g. Conyon and Freeman, 2004). Second, as is well known, firm fixed effects allow us to control for unobserved time-invariant differences in firms, such as managerial ability, employee quality and organization structure. We denote a firm's production function by $f(.)$, which relates firm value added[15] at time t, i.e. va_{it}, to inputs used in production and control variables:

(1) $va_{it} = f(k_{it}, l_{it}, eo_{it}, x_{it}, \eta_i; \tilde{\beta})$, where i=1,2, ..., N and t=1,2,...,T.

In Equation (1) subscripts *i* and *t* index firm and time, respectively. Firm deflated fixed capital is k_{it}, the sum of a firm's tangible and intangible assets at the end of the year, and labour input l_{it} is the mean number of employees in a given year. The option program indicator is denoted by eo_{it}, and x_{it} is a vector of control variables including industry-specific year dummies (for the ICT, the manufacturing and the service sectors) to control for industry-specific technological changes and economic shocks.[16] By η_i we control for unobserved heterogeneity among firms. The vector of parameters is $\tilde{\beta}$, and we are interested in the parameters for capital, labour and the option program indicator, i.e. $\beta_k, \beta_l, \beta_{eo}$. A baseline fixed effects specification for Equation (1) is the following Cobb-Douglas production function:

(2) $$\ln va_{it} = \beta_k \ln k_{it} + \beta_l \ln l_{it} + \beta_{eo} eo_{it} + \beta_x x_{it} + \eta_i + \varepsilon_{it}, \text{ where}$$
$$\varepsilon_{it} \sim iid(0, \sigma^2); \text{ i=1,2,..., N; t=1,...,T.}$$

In Equation (2) the variables are the same as in Equation (1). Thus, a firm's inputs in the production process are capital k_{it} and labor l_{it}; eo_{it} is an option program indicator; x_{it} is a vector of possible control variables including industry-specific year dummies; η_i's are individual firm fixed effects and ε_{it} is the error term. The estimation results for Equation (5) are reported in section 5.

If the assumption of strict exogeneity assumption is violated, the baseline fixed effects estimator is potentially inconsistent. Therefore, we relax the strict exogeneity assumption on capital and labor inputs as well as on option schemes and estimate dynamic GMM models.[17] We also use these models to explore the dynamic effects of stock options. To obtain asymptotically consistent parameter estimates when an explanatory variable is likely to have violated the strict exogeneity assumption, we estimate single equation GMM estimators[18] by assuming the following dynamic model:

$$\text{(3)} \quad \begin{aligned} &\ln va_{it} = \beta_{va} \ln va_{i,t-1} + \beta_k \ln k_{it} + \beta_{k\_1} \ln k_{i,t-1} + \beta_l \ln l_{it} + \beta_{l-1} \ln l_{i,t-1} + \beta_{eo} eo_{it} \\ &+ \beta_x x_{it} + \varepsilon_{it}, \text{ where } \varepsilon_{it} = \eta_i + v_{it};\ v_{it} \sim \text{iid}(0, \sigma^2);\ \text{i=1,2,...,N; t=2,3,...,T.} \end{aligned}$$

In Equation (3) all variables correspond to those in equation (2). The presence of individual effects η_i in the error term ε_{it} implies that the lagged dependent variable $va_{i,t-1}$ is positively correlated with ε_{it}. Thus, at least in large samples with serially uncorrelated error terms v_{it}, it can be shown that the OLS level estimator for β_{va} is inconsistent. Furthermore, the omitted variable literature implies that the OLS level estimator for β_{va} is biased upward in large samples (see, e.g. Bond, 2002).

The fixed effects estimator (the within group estimator) removes this inconsistency by transforming each variable to be its deviation from its firm mean. However, if the number of time periods is small, the within group transformation introduces a non-negligible negative correlation between a transformed lagged value added $va_{i,t-1}$ and a transformed error term v_{it}. This result indicates that, at least in large samples (N large), the fixed effects estimator for β_{va} is biased downward (see, e.g. Bond, 2002).

The fact that the OLS level estimator for Equation (3) is likely to be biased upwards and the fixed effects estimator is likely to be biased downwards can be useful information in assessing whether an estimator is consistent. In other words, a consistent estimator would lie between the OLS level and the fixed effects estimator. If we do not observe this pattern or an estimator is close to either the OLS level or the fixed effects estimator, we might suspect severe finite sample bias or inconsistency (see, e.g. Bond, 2002).

We follow a dynamic panel data GMM estimation strategy and allow explanatory variables to be correlated with the individual effects η_i, since we exclude these effects from Equation (3) by a first-difference transformation:

$$\begin{aligned}(4)\quad &\Delta \ln va_{it} = \beta_{va}\Delta \ln va_{i,t-1} + \beta_k \Delta \ln k_{it} + \beta_{k\_1}\Delta \ln k_{i,t-1} + \beta_l \Delta \ln l_{it} \\ &+\beta_{l\_1}\Delta \ln l_{i,t-1} + \beta_{eo}\Delta eo_{it} + \beta_x \Delta x_{it} + \Delta v_{it}, \\ &\text{where } |\beta_{va}| < 1;\ \text{i=1,2,...N; t=3,4,...T.}\end{aligned}$$

In Equation (4) we assume that the initial conditions va_{i1} are predetermined, i.e. they are uncorrelated with the subsequent error terms v_{it}, $t = 2,3,...,T$ and that the error term v_{it} is serially uncorrelated. We then apply the lagged levels of $va_{i,t-1}$ dated at t-2 and t-3 as instruments for the corresponding first-differenced variables.

To account for potential endogeneity of inputs and options in equation (4), we proceed in two steps. In the first stage, we assume that only labour and capital inputs are endogenous variables. In the second stage, we also assume that option schemes are endogenous (as well as the input variables.) When addressing the potential endogeneity of capital k_{it} and labour l_{it} inputs, we assume that k_{it}, l_{it} are predetermined and use the lagged levels of k_{it} and l_{it} dated at t-1 and t-2 as instruments for the corresponding first-differenced variables. In other words, we assume that there is no contemporaneous correlation between the inputs and the error term v_{it}, but that the inputs may be correlated with $v_{i,t-1}$ and earlier shocks.[19] To address endogeneity of option schemes, we treat the dilution option program indicator $eo_{i,t}$ as predetermined.[20] Then we use the once lagged variable, denoted t-1, as an instrument for the corresponding first-differenced option program variables.

By accepting the moment condition restrictions above, we may construct an instrument variable matrix Z_i, where the lagged levels of explanatory variables are used as instruments for the corresponding first-differenced variables.[21] We follow the terminology suggested by Bond (2002) and call these estimators the differenced GMM estimators. As an extended estimator we apply the system GMM estimator[22] by also assuming that the levels of explanatory variables, i.e. k_{it}, l_{it} and $eo_{i,t}$, are uncorrelated with individual effects η_i and predetermined with respect to the error term v_{it}.[23] Thus we use lagged first-differences of $k_{i,t}, l_{i,t}$ and $eo_{i,t}$ as instruments for the GMM level equations. The estimation results for the differenced and the system GMM estimators are separately reported in section 5, under both assumptions concerning endogeneity.

5. Empirical Results

Table 5 reports the baseline contemporaneous fixed effects estimates for a Cobb-Douglas production function during 1992-2002.[24] The estimator deviates from the standard fixed effects estimator in that we have specified a first-order autocorrelation process in the residuals. This is the preferred estimation approach, since the autocorrelation tests strongly indicates that the disturbance term is first-order autoregressive.[25]

Three main conclusions emerge from Table 5. First, the estimates for capital and labor are highly significant in columns (1)–(4). The baseline elasticity of capital input is close to 0.15, whereas for labor it is about 0.62.[26] Second, in columns (1) and (2), where our option program indicator is the presence of a plan, we do not find statistically significant evidence of contemporaneous association between options and firm productivity. In column (1) the parameter estimate for the option program indicator is 0.002, but it is statistically insignificant. In column (2) the signs of parameters differ between selective and broad-based indicators. The selective scheme estimate is 0.015 and the broad-based scheme -0.028. However, both are statistically insignificant. Third, in columns (3) and (4), where our option program indicator is the size of a plan, we find statistically significant evidence of contemporaneous association between selective schemes and firm productivity (at the 10% level.) The parameter estimate for selective schemes is 0.84, whereas for broad-based schemes it is statistically insignificant -0.256. The mean dilution for selective schemes is 0.0286 indicating, on average, a 2.4% effect on firm productivity (0.0286*0.84=0.024).

[Table 5 about here]

Tables 6-8 show estimation results for the OLS level, the fixed effects, the differenced GMM and the system GMM estimators for a Cobb-Douglas production function.[27] The reported GMM estimates are based on the two-step GMM estimator with heteroskedastic-consistent asymptotic standard errors.[28] We also perform a finite-sample correction proposed by Windmeijer (2000), since simulation studies have shown that these standard errors are downward biased.[29] For all test statistics, we rely on the two-step GMM estimator.

In Table 6 we relax the strict exogeneity assumptions on inputs. Our program indicators are dummy variables, i.e. we measure the presence of a plan. The following key findings emerge from Table 6. First, the OLS level parameter estimates for va_{t-1} in columns (1) and (2) are substantially higher than the fixed effects estimates in columns (3) and (4). As noted earlier, a consistent GMM estimator would lie between these two estimators. Unfortunately we find that the differenced GMM estimates for va_{t-1} in columns (5) and (6) are below the fixed effects estimates. Thus we suspect severe finite sample bias or inconsistency which, in this case, is likely to be associated with weak instruments for individual series that are highly time persistent.[30] Another indication of inconsistency is that the differenced GMM parameter estimates for capital inputs reported in columns (5) and (6) are about twice as large as the OLS level and the fixed effects estimates reported in columns (1)-(4).

Second, Table 6 suggests that the estimates for the presence of a plan are statistically insignificant. In our preferred specifications, reported in columns (7) and (8), where we have controlled for simultaneity of inputs by treating them as predetermined, the signs of the option and selective scheme indicators are positive, but the coefficient for the broad-based scheme indicator is negative. In sum, we do not find statistical evidence of a contemporaneous association between option programs and firm productivity.

Third, the system GMM parameter estimates using a lagged dependent variable and reported in columns (7) and (8) are lower than the OLS level estimates but higher than the fixed effects estimates. This finding indicates that the system GMM estimator is likely to be consistent, at least for the lagged dependent variable.

Fourth, the autocorrelation tests, namely m1 and m2 reported in columns (7) and (8), provide support for the system GMM estimator. The tests indicate significant negative autocorrelation in the first-differenced residuals but not in the second-order residuals. This is exactly how it should be, if the disturbances are serially uncorrelated, indicating that the key assumption for the consistency of the system GMM estimator is fulfilled. Moreover, the Sargan test clearly accepts the validity of instruments in columns (5)-(8).

[Table 6 about here]

Next we focus on the endogeneity of an option program, since that may be driving the baseline fixed effects estimates reported in Table 6. The findings reported in Table 7 are based on a program's size indicator[31], but otherwise the estimation approach is similar to that underlying the findings presented in Table 6. Since the OLS level and the fixed effects estimates for the lagged dependent variable, the capital and labor inputs are almost the same in both the tables, we do not discuss these findings any further.

The following key findings emerge from Table 7. First, the fixed effect estimate for selective programs reported in column (4) support the positive association, reported previously in Table 6. Now the parameter estimate is 0.73 and it is statistically significant at the 10% level. The mean dilution for selective schemes is 0.0286 indicating, on average, a 2.1% effect on firm productivity (0.0286*0.73=0.021). Note, however, that the fixed effects findings in columns (3) and (4) are based on the assumption that the explanatory variables are strictly exogenous.

Second, the system GMM estimators in columns (7)-(10) suggest that, after controlling for potential endogeneity of the explanatory variables, all the estimated option dilution indicators are found to be statistically insignificant. In columns (7) and (8), where we have controlled for simultaneity of capital and labor inputs by treating them as predetermined, the signs of all indicators are positive. In columns (9) and (10) we also treat the dilution indicators as predetermined (to control for simultaneity), but even then we do not find any evidence that is statistically significant that programs can be associated with firm productivity. The parameter estimate for the selective scheme reported in column (10) is now about one third as large (0.25) as the statistically significant fixed effects estimate of 0.73 reported in column (4). The parameter estimates for the broad-based dilution indicator (column 10) is -0.542. In sum, after controlling for endogeneity, at conventional levels of statistical significance, we do not find any evidence that option programs affect firm productivity. This conclusion holds even in estimates that distinguish selective and broad-based option schemes.[32]

[Table 7 about here]

In Table 8 we expand our investigation to account for the dynamic effects of option programs, since a stock option program typically spans several years. Hence the effect on productivity may be realized with a lag. To account for dynamics we keep all the assumptions used in the models reported in Table 7, but in Table 8 we report estimates that use both contemporaneous and once lagged program indicators. As an option program indicator we use the size of a plan. A Wald test is used to determine whether contemporaneous and lagged indicators are jointly zero. We first estimate the OLS level and the fixed effects models in columns (1)-(4), thereafter the system GMM estimators in columns (5)-(8).[33] In columns (5) and (6) we treat capital and labor inputs as predetermined, and in columns (7) and (8), besides capital and labor, we also treat the program indicator as predetermined.

The key finding is that we do not find convincing statistical evidence of an association between option programs and firm productivity. The evidence for a selective scheme having positive effects on productivity when using fixed effects estimators and reported in Tables 5 and 7, is not supported in the dynamic models. While the sum of contemporaneous and lagged parameter estimates of selective schemes is 0.81, they are jointly statistically insignificant (p-value 0.24). Also, the system GMM estimates for program indicators are all found to be statistically insignificant.

6. Conclusions and Implications

In this paper we assemble new panel data for all Finnish publicly listed firms during a relatively long period, namely 1992-2002. Our data enable us to distinguish different types of option plans and to seriously address issues of endogeneity concerning options and inputs. Consequently we are able to see if previous findings that are based mainly on evidence generated using data that are less representative and for shorter time periods are sustained.

We proceed by estimating Cobb-Douglas production functions with three different option program indicators. These measures reflect the presence or absence of an option scheme, the size of the scheme and whether the scheme is selective or broad based. Furthermore, the long panel nature of our data allows us to estimate dynamic panel data models with a GMM estimator and thus address the potentially important issue of endogeneity of inputs and options.

The most important finding, yielded almost consistently in diverse specifications, is a statistically insignificant association between option programs and firm productivity. This result is exceptionally robust for broad-based schemes and is independent of what option program indicator is used in estimations. As such our findings are consistent with those who hypothesize that the performance impact of options will be limited because of reasons such as free-rider problems (e.g. Oyer, 2004), accounting myopia (e.g. Hall and Murphy, 2003) or line-of-sight arguments (e.g. Vroom, 1995). As such our results are consistent with much of the financial literature that does not find evidence of a link between options and business performance (e.g. Hall and Murphy, 2003.)

For selective programs, however, findings are less consistent. In our baseline fixed effects estimates we find a statistically significant productivity impact that is between 2.1-2.4%. Since most selective plans are allocated to executives and/or key employees, this finding also provides support for the line-of-sight argument – rewards based on performance can only be motivating if the action of employees can influence the measures on which the performance-pay is based. Equally, this evidence does not support those who stress managerial rent-seeking as the principal reason for introducing selective option plans. However in models where endogeneity is accounted for, we do not find any strong evidence of a link with firm productivity. Similarly in models that investigate the dynamics of selective programs, no association between selective options and firm productivity is found.

In sum, our findings differ in important ways from earlier findings that are based on less rigorous methods and use more limited data. In particular, our findings do not provide strong support for hypotheses of a positive association between option schemes and firm productivity.

Table 1. The evolution of Finnish stock option plans 1992-2002.

Year	(1) # of firms in Helsinki Stock Exchanges	(2) # of new option plans	(3) #of new broad-based option plans	(4) # of firms having option plan	(5) # of firms having broad-based option plan
1992	65	1	0	11 (16.9%)	2 (3.1%)
1993	60	6	1	15 (25.0%)	2 (3.3%)
1994	68	21	2	34 (50%)	3 (5.0%)
1995	74	7	1	38 (51.4%)	3 (4.1%)
1996	73	9	3	36 (49.3%)	6 (8.2%)
1997	115[1)]	22[1)]	4[1)]	46[1)] (40.0%)	7[1)] (6.1%)
1998	119	47	17	69 (58.0%)	21 (17.6%)
1999	137	42	23	91 (66.4%)	36 (26.3%)
2000	150	61	30	113 (75.3%)	54 (36.0%)
2001	145	33	11	112 (77.2%)	54 (37.2%)
2002	137	33	6	101 (73.7%)	49 (35.8%)
Total		282	101		

Notes:
1. Before 1997 data are only for main list firms. From 1997 onwards, the data also include the New Market and the Investor list firms.
2. Note that stock option data in Table 2 also includes firms that have less than four consecutive year observations.
3. Source: Helsinki School of Economics, Alexander Corporate Finance and authors' calculations.

Table 2. The pattern of firm-level panel data, 1992-2002.

Freq.	Percent	Cumulative	Pattern
40	34.2	34.2	11111111111
21	18.0	52.1	01111111111
12	10.3	62.4	00111111111
10	8.6	70.9	00001111111
7	6.0	76.9	00000111111
4	3.4	80.3	00000011111
3	2.6	82.9	00000001111
3	2.6	85.5	00011111111
3	2.6	88.0	01111111110
14	12.0	100.0	(other patterns)
117	100		

Notes
1. The last column describes the pattern of data: 1 means we have an observation for this year, 0 we do not. The first digit (0 or 1) in the pattern column is year 1992. Thus the first row indicates that in 40 cases we have data for all years, whereas the second row indicates that in 21 cases there is no data for 1992 but that data are available in all other years.

Table 3. Summary statistics.

Variable	Name	Firm-year obs	Mean	Std. Dev.
l	Employees	1042	4,066	7,123
k	Fixed capital (tan.+intan.), €1000	1042	464,000	1,500,000
q	Value added, €1000	1042	255,000	574,000
Dilu*	Potential dilution in the range of (0,1); a proxy of option program size	531	0.0547	0.0450
Diluss*	Potential dilution for selective stock option programs	364	0.0286	0.0285
Dilubb*	Potential dilution for broad-based stock option programs	167	0.0900	0.0533
Opt	Option program dummy	1042	0.5182	0.4999
Ssopt	Selective option program dummy	1042	0.3580	0.4796
Bbsopt	Broad-based option program dummy	1042	0.1603	0.3670
Ln(l)	Natural logarithm of employees	1042	7.10	1.62
Ln(k)	Natural logarithm of deflated fixca	1042	17.95	2.07
Ln(q)	Natural logarithm of deflated sales	1042	17.95	1.72

Notes
1. All value measures are deflated using an industry-specific gross output deflator at 2000 constant Euros obtained from Statistics Finland.
2. * Summary statistics for dilu, diluss and dilubb variables are only for those firms that have a stock option program.
3. The total number of firm-year observations is 1042 and data are for 117 firms.

Table 4. Summary statistics: option vs. non-option firms.

Variable	Broad-based option scheme	Selective option scheme	No option scheme
Value added, €1000			
Mean	166,000	496,000	105,000
(Standard deviation)	(366,000)	(834,000)	(238,000)
Employees			
Mean	2,215	7,542	2,100
(Standard deviation)	3,273	9,625	4,371
Fixed capital (tan.+intan.), €1000			
Mean	365,000	929,000	153,000
(Standard deviation)	1,580,000	2,130,000	466,000
Value added / employees, €			
Mean	55,206	57,064	52,191
(Standard deviation)	21,787	18,925	21,302
Firm-year obs.	167	373	502

Notes

1. Based on a firm's option program adoption status in a given year, all firms are classified into three groups, namely broad-based, selective and non-option firms.
2. All value measures are deflated using an industry-specific gross output deflator at 2000 constant Euros obtained from Statistics Finland.

Table 5. Baseline fixed effects estimates: Cobb-Douglas production functions, 1992-2002.

Column	(1)	(2)	(3)	(4)
$\ln(k)_{it}$	0.150 *** (6.38)	0.151 *** (6.39)	0.150 *** (6.38)	0.150 *** (6.37)
$\ln(l)_{it}$	0.617 *** (15.26)	0.621 *** (15.32)	0.615 *** (15.05)	0.619 *** (15.18)
opt_{it}	0.002 (0.06)			
$ssopt_{it}$		0.015 (0.56)		
$bbsopt_{it}$		-0.028 (0.81)		
$dilu_{it}$			0.076 (0.24)	
$diluss_{it}$				0.840 * (1.79)
$dilubb_{it}$				-0.256 (0.73)
Firm-year obs.	925	925	925	925
Firms	117	117	117	117
Baltagi-Wu LBI[1)]	1.32	1.32	1.32	1.32
Modified Bhargava et al.[1)]	0.94	0.94	0.94	0.94
R^2 within	0.61	0.61	0.61	0.62

Notes
1. The dependent variable is ln(value added).
2. The estimator is a modified fixed effects estimator -xtregar-, where the disturbance is first-order autoregressive.
3. Absolute values of t statistics in parentheses. *** Significant at 1% level, ** at 5% level, * at 10% level, respectively.
4. Opt is a dummy variable for the presence of an option program, ssopt is a dummy variable for the presence of a selective program and bbsopt is a dummy variable for the presence of a broad-based option program. Diluss is an interaction variable between potential dilution and ssopt dummy. Dilubb is an interaction variable between potential dilution and bbsopt dummy.
5. Industry-specific year dummies for the ICT, the manufacturing and the service sectors are included in all models.
6. [1)] The tests are based on the standard fixed effects models without modelling first-order autoregression. Baltagi-Wu LBI is the Baltagi-Wu (1999) locally best invariant test statistic for $\rho = 0$. If a test statistic is far below 2, it is an indication of positive serial correlation. Modified Bhargava et al. (1982) also test if $\rho = 0$. If the test statistic is significantly different from zero, we have serial correlation. The tests indicate serial correlation supporting the modified fixed effects estimator.

Table 6. Contemporaneous GMM estimates for 1992-2002 when the option indicator measures the Presence of a program.

Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimator	OLS level	OLS level	Fixed effects	Fixed effects	Differenced GMM	Differenced GMM	System GMM	System GMM
$\ln(va)_{it-1}$	0.751 *** (17.80)	0.752 *** (17.80)	0.441 *** (5.60)	0.440 *** (5.71)	0.263 *** (3.80)	0.216 *** (2.91)	0.647 *** (8.40)	0.630 *** (9.97)
$\ln(l)_{it}$	0.614 *** (10.30)	0.617 *** (9.96)	0.575 *** (9.11)	0.585 *** (8.81)	0.384 *** (2.76)	0.438 *** (2.84)	0.538 *** (5.43)	0.573 *** (5.94)
$\ln(l)_{it-1}$	-0.430 *** (7.33)	-0.433 *** (7.08)	-0.248 *** (3.08)	-0.252 *** (6.63)	-0.129 (1.42)	-0.125 (1.41)	-0.335 *** (2.89)	-0.368 *** (3.36)
$\ln(k)_{it}$	0.120 *** (4.40)	0.120 *** (4.39)	0.128 *** (4.01)	0.128 *** (4.01)	0.217 *** (3.14)	0.209 *** (2.64)	0.182 *** (4.95)	0.160 *** (3.67)
$\ln(k)_{it-1}$	-0.057 ** (1.98)	-0.057 *** (2.00)	-0.005 (0.16)	-0.005 (0.18)	0.034 (0.78)	-0.030 (0.68)	-0.051 (1.40)	-0.038 (1.23)
opt_{it}	-0.019 (1.00)	-	-0.015 (0.62)	-	-0.053 (0.53)	-	0.044 (0.68)	-
$ssopt_{it}$	-	-0.014 (0.82)	-	0.003 (0.15)	-	0.024 (0.21)	-	0.115 (1.42)
$bbsopt_{it}$	-	-0.031 (0.92)	-	-0.054 (1.15)	-	-0.394 (1.45)	-	-0.097 (1.04)
m1 (p-value)	0.52	0.53	0.93	0.84	-0.01 ***	-0.02 **	-0.00 ***	-0.00 ***
m2 (p-value)	0.23	0.23	0.07 *	0.06 *	-0.34	-0.33	0.77	-0.75
Sargan (p-value)	-	-	-	-	0.21	0.26	0.20	0.34
Firm-year obs.	925	925	925	925	808	808	925	925
Firms	117	117	117	117	117	117	117	117
R^2	0.99	0.99	0.80 (within)	0.80 (within)	-	-	-	-
GMM Instruments	-	-	-	-	va_{t-2}, va_{t-3}, k_{t-1}, $k_{t-2,}$ l_{t-1}, l_{t-2}, dummies	va_{t-2}, va_{t-3}, k_{t-1}, $k_{t-2,}$ l_{t-1}, l_{t-2}, dummies	va_{t-2}, va_{t-3}, k_{t-1}, k_{t-2}, l_{t-1}, l_{t-2}, dummies, Δk_{t-1}, Δl_{t-1}	va_{t-2}, va_{t-3}, k_{t-1}, k_{t-2}, l_{t-1}, l_{t-2}, dummies, Δk_{t-1}, Δl_{t-1}

Notes
1. The dependent variable is ln(value added).
2. Absolute values of t statistics are in parentheses, standard errors are heteroskedastic-consistent. *** Significant at 1% level, ** at 5% level, * at 10% level, respectively.
3. GMM estimations are based on the two-step heteroskedastic-robust estimator with a finite-sample correction proposed by Windmeijer (2000).
4. m1- and m2 are tests for first- and second-order autocorrelation in residuals and the statistics are asymptotically standard normal under the null of no serial correlation.
5. Sargan statistic tests the validity of instruments, i.e. whether moment conditions are valid.
6. Industry-specific year dummies for the ICT, the manufacturing and the service sectors are included in all models.

Table 7. Contemporaneous GMM estimates for 1992-2002 when the option indicator measures the Size of a program.

Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Estimator	OLS level	OLS level	Fixed effects	Fixed effects	Differenced GMM	Differenced GMM	System GMM	System GMM	System GMM	System GMM
$\ln(va)_{it-1}$	0.751 *** (17.70)	0.752 *** (17.60)	0.440 *** (5.61)	0.438 *** (5.72)	0.266 *** (3.89)	0.227 *** (3.13)	0.653 *** (8.83)	0.657 *** (8.94)	0.682 *** (10.50)	0.681 *** (10.20)
$\ln(l)_{it}$	0.610 *** (10.20)	0.609 *** (10.30)	0.572 *** (9.03)	0.577 *** (8.97)	0.416 *** (2.48)	0.601 *** (2.75)	0.550 *** (5.17)	0.552 *** (5.32)	0.578 *** (5.85)	0.571 *** (5.48)
$\ln(l)_{it-1}$	-0.428 *** (7.26)	-0.428 *** (7.23)	-0.244 *** (3.10)	-0.242 *** (3.13)	-0.141 (1.48)	-0.115 (1.48)	-0.339 *** (2.97)	-0.341 *** (3.08)	-0.351 *** (3.28)	-0.340 *** (3.86)
$\ln(k)_{it}$	0.119 *** (4.40)	0.119 *** (4.40)	0.128 *** (4.02)	0.128 *** (3.98)	0.236 *** (2.68)	0.201 *** (2.50)	0.181 *** (4.99)	0.179 *** (4.71)	0.152 *** (4.31)	0.149 *** (3.20)
$\ln(k)_{it-1}$	-0.056 ** (1.96)	-0.056 ** (1.96)	-0.001 (0.16)	-0.007 (0.21)	0.027 (0.73)	-0.006 (0.12)	-0.057 (1.60)	-0.060 * (1.72)	-0.063 ** (1.98)	-0.072 ** (1.99)
$dilu_{it}$	-0.059 (0.30)	-	-0.017 (0.05)	-	-1.948 (1.20)	-	0.161 (0.20)	-	-0.381 (0.56)	-
$diluss_{it}$	-	0.019 (0.07)	-	0.728 * (1.72)	-	4.030 (1.07)	-	0.859 (0.55)	-	0.247 (0.29)
$dilubb_{it}$	-	-0.076 (0.34)	-	-0.276 (0.74)	-	-3.462 (1.61)	-	0.061 (0.08)	-	-0.542 (1.07)
m1 (p-value)	0.51	0.51	-0.96	0.89	-0.01 ***	0.01 ***	-0.00 ***	-0.00 ***	-0.00 ***	-0.00 ***
m2 (p-value)	0.22	0.22	-0.07 *	-0.06 *	0.51	-0.59	-0.76	-0.76	-0.79	-0.80
Sargan (p-value)	-	-	-	-	0.26	0.36	0.19	0.21	0.64	0.84
Firm-year obs.	925	925	925	925	808	808	925	925	925	925
Firms	117	117	117	117	117	117	117	117	117	117
R^2	0.99	0.99	0.80 (within)	0.80 (within)	-	-	-	-	-	-
GMM Instruments	-	-	-	-	va_{t-2}, va_{t-3}, k_{t-1}, k_{t-2}, l_{t-1}, l_{t-2}, dummies	va_{t-2}, va_{t-3}, k_{t-1}, k_{t-2}, l_{t-1}, l_{t-2}, dummies	va_{t-2}, va_{t-3}, k_{t-1}, k_{t-2}, l_{t-1}, l_{t-2}, dummies, va_{t-1}, Δk_{t-1}, Δl_{t-1}	va_{t-2}, va_{t-3}, k_{t-1}, k_{t-2}, l_{t-1}, l_{t-2}, dummies, Δva_{t-1}, Δk_{t-1}, Δl_{t-1}	va_{t-2}, va_{t-3}, k_{t-1}, k_{t-2}, l_{t-1}, l_{t-2}, $dilu_{t-1}$, dummies, Δva_{t-1}, Δk_{t-1}, Δl_{t-1}, $\Delta dilu_{t-1}$	va_{t-2}, va_{t-3}, k_{t-1}, k_{t-2}, l_{t-1}, l_{t-2}, $diluss_{t-1}$, $dilubb_{t-1}$, dummies, Δva_{t-1}, Δk_{t-1}, Δl_{t-1}, $\Delta diluss_{t-1}$, $dilubb_{t-1}$

Notes
1. The dependent variable is ln(value added).
2. Absolute values of t statistics are in parentheses, standard errors are heteroskedastic-consistent. *** Significant at 1% level, ** at 5% level, * at 10% level, respectively.
3. GMM estimations are based on the two-step heteroskedastic-robust estimator with a finite-sample correction proposed by Windmeijer (2000).
4. m1- and m2 are tests for first- and second-order autocorrelation in residuals and the statistics are asymptotically standard normal under the null of no serial correlation.
5. Sargan statistic tests the validity of instruments, i.e. whether moment conditions are valid.
6. Industry-specific year dummies for the ICT, the manufacturing and the service sectors are included in all models.

Table 8. Dynamic GMM estimates for 1992-2002 when the option program indicator measures the Size of a program.

Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Estimator	OLS level	OLS level	Fixed effects	Fixed effects	System GMM	System GMM	System GMM	System GMM
$\ln(va)_{it-1}$	0.750 *** (17.60)	0.751 *** (17.40)	0.438 *** (5.68)	0.433 *** (5.77)	0.628 *** (9.09)	0.659 *** (8.47)	0.673 *** (10.50)	0.681 *** (10.20)
$\ln(l)_{it}$	0.609 *** (10.20)	0.608 *** (10.20)	0.573 *** (8.99)	0.580 *** (8.91)	0.555 *** (5.13)	0.555 *** (4.90)	0.584 *** (6.04)	0.557 *** (6.36)
$\ln(l)_{it-1}$	-0.425 *** (7.18)	-0.426 *** (7.12)	-0.240 *** (3.13)	-0.235 *** (3.16)	-0.323 *** (2.98)	-0.338 *** (3.00)	-0.343 *** (3.42)	-0.326 *** (3.41)
$\ln(k)_{it}$	0.119 *** (4.42)	0.119 *** (4.42)	0.129 *** (4.02)	0.128 *** (3.98)	0.179 *** (4.45)	0.173 *** (3.87)	0.150 *** (4.04)	0.147 *** (4.03)
$\ln(k)_{it-1}$	-0.056 ** (1.96)	-0.056 ** (1.97)	-0.005 (0.15)	-0.006 (0.20)	-0.051 (1.43)	-0.062 * (1.72)	-0.061 ** (1.98)	-0.066 *** (2.06)
$dilu_{it}$	0.209 (0.74)	-	0.177 (0.59)	-	1.093 (0.20)	-	0.089 (0.19)	-
$dilu_{it-1}$	-0.340 (0.94)	-	-0.310 (0.87)	-	-1.536 (0.96)	-	-0.555 (1.12)	-
$diluss_{it}$	-	0.407 (0.77)	-	0.500 (1.04)	-	2.800 (1.00)	-	1.192 (0.77)
$diluss_{it-1}$	-	-0.540 (0.85)	-	0.314 (0.71)	-	-3.424 (1.20)	-	-1.610 (1.09)
$dilubb_{it}$	-	0.106 (0.38)	-	0.043 (0.13)	-	0.425 (0.32)	-	-0.016 (0.03)
$dilubb_{it-1}$	-	-0.229 (0.60)	-	-0.432 (1.01)	-	-0.726 (0.44)	-	-0.576 (0.78)
Wald test (p-value)	0.64 (dilu)	0.69 (diluss)	0.64 (dilu)	0.24 (diluss)	0.63 (dilu)	0.49 (diluss)	0.53 (dilu)	0.55 (diluss)
Wald test (p-value)	-	0.83 (dilubb)	-	0.59 (dilubb)	-	0.91 (dilubb)		0.72 (dilubb)
m1 (p-value)	0.51	0.51	0.96	0.93	-0.00 ***	-0.00 ***	-0.00 ***	-0.00 ***
m2 (p-value)	0.23	0.23	-0.07 *	-0.06 *	-0.73	-0.80	-0.77	-0.78
Sargan (p-value)	-	-	-	-	0.17	0.17	0.60	0.95
R^2	0.99	0.99	0.80 (within)	0.80 (within)	-	-	-	-
GMM Instruments	-	-	-	-	va_{t-2}, va_{t-3}, k_{t-1}, k_{t-2}, l_{t-1}, l_{t-2}, dummies, Δva_{t-1}, Δk_{t-1}, Δl_{t-1}	va_{t-2}, va_{t-3}, k_{t-1}, k_{t-2}, l_{t-1}, l_{t-2}, dummies, Δva_{t-1}, Δk_{t-1}, Δl_{t-1}	va_{t-2}, va_{t-3}, k_{t-1}, k_{t-2}, l_{t-1}, l_{t-2}, $dilu_{t-1}$, dummies, Δva_{t-1}, Δk_{t-1}, Δl_{t-1}, $\Delta dilu_{t-1}$	va_{t-2}, va_{t-3}, k_{t-1}, k_{t-2}, l_{t-1}, l_{t-2}, $diluss_{t-1}$, $dilubb_{t-1}$, dummies, Δva_{t-1}, Δk_{t-1}, Δl_{t-1}, $\Delta diluss_{t-1}$, $\Delta dilubb_{t-1}$

Notes
1. The dependent variable is ln(value added).
2. Absolute values of t statistics are in parentheses, standard errors are heteroskedastic-consistent. *** Significant at 1% level, ** at 5% level, * at 10% level, respectively.
3. GMM estimations are based on the two-step heteroskedastic-robust estimator with a finite-sample correction proposed by Windmeijer (2000).
4. m1- and m2 are tests for first- and second-order autocorrelation in residuals and the statistics are asymptotically standard normal under the null of no serial correlation.
5. The Wald test tests whether contemporaneous and lagged dilution option indicators are jointly statistically significant.
6. Sargan statistic tests the validity of instruments, i.e. whether moment conditions are valid.
7. Industry-specific year dummies for the ICT, the manufacturing and the service sectors are included in all models.
8. The number of firms is 117 or 925 firm-year observations in all models.

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Endnotes

[1] Earlier versions of this paper have benefited from comments by participants at the ASSA/ACES meeting in San Diego, January 2-5, 2004, the 12th IAFEP Conference in Halifax, July 8-10, 2004, the 16th EALE conference in Lisbon, September 9-11, 2004, the FPPE Industrial Organisation Workshop in Helsinki, December 9-10, 2004, and the EALE/SOLE World Conference in San Francisco, June 2-5, 2005. We are especially grateful to Kevin F. Hallock, Kari Hämäläinen, Pekka Ilmakunnas, Uwe Jirjahn, Jeffrey Pliskin and Otto Toivanen for their very helpful comments. Also we acknowledge Professor Seppo Ikäheimo and Alexander Corporate Finance for allowing us access to their databases on options in publicly traded Finnish companies, and to Mikael Katajamäki for his outstanding research assistance. We also thank Balance Consulting for financial statement data. Kalmi and Mäkinen gratefully acknowledge funding from the LIIKE-programme of the Academy of Finland. Kalmi also gratefully acknowledges financial support from the Marcus Wallenberg Foundation and the Helsinki School of Economics Research Foundation. In addition, Mäkinen thanks the Yrjö Jahnsson Foundation, the Helsinki School of Economics Research Foundation and the Foundation of Kluuvi for financial support. Support from the Research Institute of the Finnish Economy (ETLA) is gratefully acknowledged.

[2] Mäkinen (2001) describes the evolution of stock option programs in Finland. Jones, Kalmi and Mäkinen (2006) study the determinants of option schemes adoption in Finland. They also summarise the evolution of options in Finland and discuss the institutional background in more detail.

[3] One reason for the paucity of such studies is the lack of firm-level stock option data. For example, the S&P ExecuComp database contains only employee stock options grant values for the top five highest paid executives. However, financial economists have studied the links between stock option plans and appropriate accounting measures such as contemporaneous stock returns or stock market returns in the following year. Since our focus is on productivity, we do not comprehensively review such studies though we note that many finance studies do not find evidence of strong links between options and firm performance (e.g. Hall and Murphy, 2003.) For a broad review of pertinent empirical work see Rosen (2006).

[4] These are part of a broader class of studies that employ an augmented production function methodology. For example, for a review of such work in investigating the impact of ownership forms on firm performance in transition economies, see Djankov and Murrell (2002).

[5] However, we have included option schemes prior to the listing for such firms that enter the HEX before 2002.

[6] This is done to provide the reader with a better understanding of the development and prevalence of option schemes in Finland. See more detailed evolution and institutional background description in Jones, Kalmi and Mäkinen (2006).

[7] We omit 8 firms or 15 firm-year observations due to their having fewer than 4 consecutive observations. To utilize all possible firm-level financial information data we also collected data on income statements prior a firm’s listing on the Helsinki Stock Exchange.

[8] Our classification is thus different from Kroumova et al. (2000; 2002), who use 50 % threshold as a criterion for broad-based schemes. Our data do not include this information, but have the important advantage of being derived from publicly reported sources that must be externally verifiable, rather than from confidential surveys.

[9] We also interviewed Mr. Erkki Helaniemi, a partner in the investment bank Alexander Corporate Finance, who has been personally involved in setting up dozens of option schemes. He confirmed that there are dramatic differences in the participation rates for option schemes, depending on eligibility.

[10] The first two are option program dummy variables, which measure the presence or absence of an option program. The dummy variable captures a program's "introduction effect": when an option program is adopted, a dummy variable switches from 0 to 1 shifting a linear production function immediately and without anticipation. This effect of an option plan on firm-level productivity is modelled as a constant over firms. The third measure is a proxy for the size of a program.

[11] Unfortunately, we do not have information on stock option program details such as exercise prices to calculate Black-Scholes values.

[12] When instrumenting inputs in a dynamic GMM estimation, the instrument matrix becomes substantially larger in the translog case. If the full set of instruments is used in a finite sample, this may bias estimates.

[13] While this assumption substantially simplifies modelling, it may not be harmless. For example, the adoption of a scheme may not be less important than how a plan is actually implemented within a firm. Thus, if adoption is coupled with the introduction of new workplace practices, such as self-managed teams with some decision-making power, or increasing the number of regular employees meetings, the effects on firm productivity can be expected to differ than if the option scheme were to be introduced alone. Unfortunately, we do not have information on workplace practices besides options.

[14] For example, presumably the terms of option schemes differ among firms. Thus, when an option scheme is substantially out of the money (i.e. the current stock price is substantially below the exercise price), options may not provide strong incentives for employees and managers to improve their performance.

[15] On theoretical grounds firm value added is a preferable measure to sales, since value added does not include intermediate inputs that are purchased from other firms.

[16] This decision is supported by the fact that the ICT sector experienced both boom and bust during 1997-2003.

[17] Our decision to use a GMM estimator has been guided by the fact that we do not have suitable instrument variables for option schemes.

[18] Besides these dynamic panel data GMM estimators, Maximum likelihood (ML) estimators have also been developed. Unfortunately, one drawback of the ML estimators is that different distributional assumptions of the initial conditions are needed in the estimation process to imply different likelihood functions. It follows that ML estimators may produce inconsistent estimate for the lagged dependent variable

(i.e. $\ln va_{,t-1}$), if an initial condition distribution, i.e. the distribution of va_{i1}, is not the correct one. By contrast, the dynamic panel data GMM estimators do not need such a strong initial condition assumption.

[19] This seems to be a reasonable assumption, since firms may not adjust their capital and labour inputs immediately on economic shocks within a year. For example, firms may be unaware as to whether a shock is permanent or temporary.

[20] Since option plans typically are introduced publicly in early spring, a few weeks before the annual general meeting of shareholders, this seems to be a reasonable assumption. Therefore, a potential correlation is likely to be with a previous period rather than being contemporaneous.

[21] For more on dynamic GMM estimators and constructing an instrument variable matrix see Arellano and Bond (1991), Arellano and Bover (1995), and Blundell and Bond (1998).

[22] The benefit of using the system GMM estimator is that it is shown to be more efficient than the differenced GMM estimator in large samples.

[23] To us the non-correlation seems more plausible approach than assuming that the first-differences of capital and labour are uncorrelated with individual effects η_i.

[24] We use Stata/SE 9.1 for Windows statistical package in estimating the models of Table 5.

[25] We also performed the modified Wald statistics to examine a groupwise heteroskedasticity in the residuals when estimating a standard fixed effects estimator. The statistics indicate a violation in assumption that the error term is homoskedastic. Thus, the modified Wald statistic indicates that the fixed effects estimator is not efficient, which challenges our conclusions on the significance levels of the standard fixed effects parameters. However, the modified Wald statistics should be also used with caution, since its power can be low in the context of fixed effects with “large N, small T”. Unfortunately, we are unaware of any fixed effects estimators that account simultaneously for autocorrelation and heteroskedasticity. We estimate heteroskedastic robust estimates in the GMM models; therefore we prefer accounting for autocorrelation in Table 5. The autocorrelation ρ is calculated by -tscorr- option in -xtregar- model by using the Stata/SE 9.1 for Windows statistical package. The benefit of using -tscorr- is that ρ is bounded in [-1,1].

[26] While these estimates would imply decreasing returns to scale, it is well known that fixed effects estimators tend to underestimate the coefficients, especially capital (Griliches and Mairesse, 1998).

[27] All estimations use the Ox/DPD statistical package. This econometric software package allows tests for the first- and second-order autocorrelation and the Sargan test for over-identifying restrictions. It also calculates asymptotically heteroskedastic robust standard errors. For more on Ox/DPD statistical package see http://www.doornik.com.

[28] See e.g. Bond (2001) and Arellano (2003) for technical details on a two-step GMM estimator.

[29] We prefer a two-step GMM estimator for two reasons. First, it should be more efficient in the presence of heteroskedasticity, especially under Windmeijer's (2000) finite sample correction. Second, the Sargan statistic based on the minimised value of the two-step GMM estimator has an asymptotic χ^2 distribution regardless of heteroskedasticity.

[30] The estimation results suggest (not reported here) that the individual series are highly persistent (but not an exact unit root) indicating that the instruments in first-differenced equations are likely to be weak. Being the case, the existing evidence from standard instrumental variable literature suggests that IV estimators can be subject to serious finite sample biases when instruments are weak. See more Bound, Jaeger and Baker (1995). Unfortunately, Blundell and Bond (1998), and Blundell, Bond and Windmeijer (2000) have demonstrated that this also applies for the differenced GMM estimator, when individual series are highly persistent biasing the estimates downwards.

[31] The information on β*'s* in the first-difference regressions depend on the ratio of var (Δv)/var (Δx). Variation of x over time t is necessary, as is variation over individuals i. Since in our data the i,t -variation of option program dummy indicators is less than variation in the dilution indicators, we focus on the correlation between the dilution option program indicator and the error term.

[32] As in Table 6 previously, also now the differenced GMM estimates for va_{t-1} in columns (5) and (6) are clearly below the fixed effects estimates in columns (3) and (4). This again indicates a serious finite sample bias for the estimator, which is likely to be associated with weak instruments. Moreover, the autocorrelation tests m1 and m2 indicate that the key assumption for the system GMM estimators is fulfilled. Also, the system GMM estimates for va_{t-1} in columns (7)-(10) are lower than the OLS level estimates but higher than the fixed effects estimates indicating that the system GMM estimator is likely to be consistent, at least for lagged dependent variable.

[33] As in Tables 6 and 7, the differenced GMM estimators appeared to be inconsistent in Table 8. Consequently, we do not report the estimates for the differenced GMM estimator in Table 8.