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Firm-Level Investment in France and the United States:

An Exploration of What We Have Learned in Twenty Years

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ABSTRACT. – Our two related goals in this paper are the following: Firstly and mainly, we want to examine the effects of major changes in modelling strategy and econometric methodology, over the past twenty years, on estimation of firm-level investment equations using panel data. Secondly, we try to assess whether the differences in the estimated investment equations, as between recent years and ten to twenty years ago in the French and U.S. Manufacturing industries, are "*real*" and economically meaningful. Thus our paper consists of a series of comparisons: a simple accelerator-profit specification versus one with error correction, traditional between- and within-firm estimation versus GMM estimation, the investment behavior of French firms versus that of U.S. firms, and investment behavior in recent years versus ten to twenty years ago. Although the important econometric advances of the past twenty years have been far from being as successful as we had hoped for, we do find some significant improvement in the specification, estimation and interpretation of firm investment equations; we also find some real changes in the investment behavior of French and U.S. firms during these twenty years.

L'investissement des entreprises en France et aux États-Unis : qu'avons-nous appris en vingt-ans d'économétrie des panels ?

RÉSUMÉ. – Dans cet article, nous examinons ce que les progrès de la modélisation et des méthodes de l'économétrie des panels au cours des vingt dernières années nous ont appris sur l'estimation des fonctions d'investissement des entreprises. Nous essayons également d'évaluer si les différences dans les estimations que nous trouvons pour les entreprises industrielles françaises et américaines, sur les années récentes et il y a dix à vingt ans, sont « *réelles* » et économiquement importantes. Notre travail consiste ainsi en plusieurs comparaisons : entre les estimations obtenues à partir de la spécification habituelle de l'accélérateur-profit et une spécification à correction d'erreurs, entre les estimations inter- et intra-entreprises usuelles et les estimations de type GMM, entre celles trouvées pour la France et pour les États-Unis, et pour les années récentes et plus anciennes. Même si les progrès importants en vingt ans des méthodes économétriques sont loin d'avoir eu tous les succès escomptés, nous concluons néanmoins à des avancées significatives en matière de spécification, estimation et interprétation des modèles d'investissement des entreprises ; nous observons aussi certains changements réels dans le comportement d'investissement des entreprises françaises et américaines sur les vingts dernières années.

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We are grateful to the late R. EISNER, S. BOND and two referees for helpful comments on earlier versions of this paper. The reader of this paper will see that our "*modern*" ("*vingt ans après*") analysis confirms the understanding of firm investment behavior that EISNER analysed so well long ago, using less technical but no less insightful language.

1 Introduction

Twenty years ago, at the first International Conference on the Econometrics of Panel Data in Paris, whose anniversary we celebrated in June 1997, Robert EISNER and Gilles OUDIZ presented some of the first firm level estimates of an investment equation on panel data, EISNER for the United States and OUDIZ for France.[1] Since then, a large number of advances have been made both in the econometric theory and in the econometric practice and technology for analyzing panel data. The anniversary of this conference seemed an appropriate moment to look back and ask what effects these advances have had on the estimation of this particular equation. Are we closer to having an investment equation that is a robust description of the investment behavior of industrial firms in developed economies? Has our new methodology really helped us to improve our understanding of the major determinants of firm investment?

In the past twenty years, important developments in the economic analysis and modeling of investment have also occurred, and many applied studies have been performed on investment at the firm level, based on increasingly available micro-data sets. This vast literature has given rise to a number of good surveys. Although we thought useful to provide a brief overview of the literature by way of showing precisely where we stand, our intent here is not to add one more survey to the list, but to focus on the purely econometric issues. We thus have two related goals. Firstly and mainly, we want to examine the effects of major changes in modeling strategy and econometric methodology, over the past twenty years, on estimation of a firm-level investment equation using panel data. We will especially consider two problems in the specification and estimation of the investment equation (these two problems are closely related and affect many other panel data econometric analyses): the biases that may arise from the presence of (correlated) firm specific effects and the simultaneity biases that may arise from the joint determination of output and investment. Secondly, we want to assess whether the differences in the estimated investment equations, as between recent years and ten to twenty years ago in the French and United States manufacturing industries, are "*real*" and economically meaningful.

To try to achieve such ambitious goals, we first proceed with a careful comparison of our results with the main results of the EISNER [1978 a, b] and OUDIZ [1978] papers, as well as with those in a related paper by MAIRESSE and DORMONT [1985], all of which are based on a standard accelerator-profit specification of the investment equation and on traditional between- and within-firm type estimations. We conduct an analysis as much like theirs as possible, using four samples of large manufacturing firms, for two different time periods, a recent one and an earlier one, and for both France and the United States. We then present and discuss the results obtained for these four samples, but now using an improved error correction specification of the

1. For earlier work using firm panel data to estimate an investment equation, see GRUNFELD [1960] and KUH [1963] for the U.S. or ECHARD and HENIN [1970] for France.

accelerator-profit equation and a more appropriate instrumental variables estimation method, the well-known GMM (Generalized Method of Moments). The main advantage of the error correction specification is in allowing us to better characterize the longer term and shorter run aspects of the investment relation; this was indeed a major focus of the EISNER, OUDIZ and MAIRESSE-DORMONT papers. GMM estimation should in principle be able to correct for the biases due to both the presence of correlated effects and simultaneity (and incidentally to those due to random errors of measurement). Our paper thus consists of a series of comparisons: a simple accelerator-profit specification versus one with error correction, traditional between- and within-firm estimations versus GMM estimations, the estimated investment behavior of French firms versus that of U.S. firms, and today versus ten to twenty years ago.

After a bird's eye view of the vast literature on firm investment (section 2), we begin with a discussion of the familiar and fairly eclectic class of accelerator-profit investment equations (section 3), and with a brief presentation of the recent GMM estimation methods (section 4). This is followed by a description of our four data sets, which cover about 400 to 500 manufacturing firms each, in France and the United States, for the 1968-1979 and 1979-1993 periods (section 5). Then we proceed to present the various sets of estimates based on these data-sets, assessing the differences with the estimates of EISNER for the United States 1961-1968, OUDIZ for France 1971-1975, and MAIRESSE-DORMONT for both countries 1970-1979 (section 6), and the differences related to the choice of the error correction specification and the use of the GMM estimations (section 7). We conclude with a discussion of what we have learned in twenty years from the advances in panel data econometrics.

2 The Changing Modeling of the Investment Equation

Over the past thirty years or so, a series of minor and major evolutions have taken place in the modeling of investment, often driven by the lack of success of previous models in explaining very much of investment behavior and by a continuing or even increased interest in policies that affect the investment behavior of private firms. In our view, the major changes can be summarized in the following way: 1) a shift in attention away from macro modeling towards micro modeling, partly driven by data availability, but also by increasing awareness of the inappropriateness of time series data for the structural models that are of interest if one wishes to understand the fundamental determinants of investment; 2) the revolution caused by the work of MODIGLIANI and MILLER, who pointed out the irrelevance of financial considerations for investment decisions in some circumstances, followed by a counter-revolution due to the introduction of asymmetric information and agency costs into the theoretical models; 3) with the move to the use of panel data, increased understanding of the complexities introduced in econometric analysis of dynamic models, and more generally models with endogenous right hand side variables (even if only with respect to the past history of the dependent variable).

We discuss each of these changes briefly.

First, as has been well-documented by HASSETT and HUBBARD [1997] and HUBBARD [1998] among others, dissatisfaction with the empirical results obtained when macro-economic data are used to estimate investment relations derived from economic theory (that is, relations that focus on the cost of capital and expected returns as investment determinants) has led to a re-examination of the econometric assumptions underlying the macro investment relation. In large semi-closed economies (which includes most developed economies during much of the post-World War II period), there is an obvious simultaneity between the dependent variable (the aggregate investment level or investment rate) and the independent price variable (the relative price, cost of capital, or TOBIN's *q*), and consequently their observed values trace out a sequence of equilibrium points that need not have any simple relationship to the investment demand relation assumed by economic theory. This is an old point, but it is frequently ignored in practice and in discussions of the "*failure*" of the aggregate investment literature. [2]

Thus as computing power and micro-level datasets became available, attention shifted to the estimation of investment equations using micro-data. The papers presented at this conference twenty years ago are some of the earliest examples of this shift, but they themselves do not fully reflect the state of economic theory at the time, since they do not incorporate any explicit price information into the specification of their investment equation. In fact, in an economy with a fairly flexible capital market, such as those in most developed countries, variation in investment prices or the cost of capital is difficult to come by in the cross section dimension, so it is often ignored or subsumed in a series of time dummies in the regression specification. There are exceptions to this rule where such things as "*exogenous*" variations in tax exposure affect the cost of capital to individual firms, and these exceptions have been recently exploited in a series of papers reviewed in HUBBARD [1998]. [3, 4]

Second, the same time period that saw a shift of attention from macro-economic investment equations to micro-economic ones has seen two major theoretical developments. The influential theorem of MODIGLIANI and MILLER [1958] (hereafter M-M) demonstrated that in a world of perfect capital

2. For a recent statement of the critique, see HALL's [1997] discussion of HASSETT and HUBBARD [1997]. A solution to the problem would be of course to have good instruments for the investment price (uncorrelated with other aspects of the macro-economy), but these instruments have proved hard to find.

3. In general, the results in these papers do demonstrate sensitivity of investment to the price of investment when there is indeed sufficient exogenous variation in the price data. For other most recent evidence, see also CHIRINKO, FAZZARI and MEYER [1999]. See also HALL [1993], where variation in the tax price of R&D is used to estimate the responsiveness of R&D investment to changes in the cost of capital, yielding a price elasticity for R&D investment of one or greater.

4. The shift of attention to micro data also means that two other considerations have come to the forefront. First, in order to say something about aggregate investment using the micro estimates, it is necessary to understand the implications of micro behavior for aggregates. Given the selectivity of most micro samples, this task is not always trivial. Second, as we analyse smaller and smaller units (*e.g.*, individual plants), it becomes more and more obvious that at this level investment is a lumpy rather than smoothly continuous process and that we may need to take account of this in constructing our theoretical models. See CABALLERO [1999] for a thorough discussion of these two points.

markets, investment decisions should not be affected by financing decisions or capital structure of individual firms, but only by the cost of capital faced in the market. This result implied that there should be no role for liquidity variables such as cash-flow in the investment equation, except to the extent that they reflected future profit opportunities that were not otherwise accounted for by such things as sales growth. At the time M-M was published, there were already empirical results available that suggested a strong role for cash-flow in the equation (MEYER and KUH [1957]), but the effect of the M-M proposition was to deflect attention for a time from the importance of cash-flow or profits in the investment equation towards a more neo-classical view of the firm's investment decision, such as that in JORGENSON [1963]. Weaknesses in the empirical implementation of JORGENSON's model led among other things to the development of a literature that explicitly allowed for adjustment costs or delivery lags in investment. This literature culminated in the empirical TOBIN's *q* literature (*e.g.* TOBIN [1969], SUMMERS [1981]), which attempted to provide a theoretically better measure of "*price*" or expected rate of return for investment than the current marginal product of capital used by JORGENSON (which was implemented in practice using *ad hoc* adjustment lags).

However, as is demonstrated by the EISNER and OUDIZ papers among others, interest in cash-flow effects on investment never entirely waned during the period following the M-M publication, and eventually theorists came to the rescue of those who continued to believe strongly in the importance of firm liquidity for investment decisions. This rescue took the form of a series of papers, beginning with JENSEN and MECKLING [1976], that demonstrated the breakdown of the M-M proposition in the presence of either asymmetric information between investors and the firm, or agency costs arising from the divergent goals of managers and shareholders. [5] Holes in the theoretical barrier between investment and finance soon widened to permit a flood of empirical papers that explore various implications of the potential cost wedge between external and internal funds on the investment behavior of individual firm. [6] Although it has become fairly clear from this work that cash-flow plays an important role in the investment equation at the firm level, a role *consistent* with the presence of financial market imperfections in some (but not all) cases, definitive evidence that cash-flow is not simply a proxy for news about expected future profits has been hard to come by. Our present work is no exception to this rule and we make no attempt to identify the source of the cash-flow effect; we merely document its presence.

Third, the modeling of investment equations raises issues that are more specifically econometric. Because it involves both adjustment costs and expectations about the future profitability of investment, it is inherently dynamic. In practise the issue of taking care of adjustment costs has been confronted either implicitly by introducing *a posteriori* lagged variables, with the hypothesis of some form of lag distribution, in the estimated investment equation,

5. See in particular STIGLITZ and WEISS [1981], MYERS and MAJLUF [1984].
6. See CHIRINKO [1993], SCHIANTARELLI [1996], and HUBBARD [1998] for three excellent recent surveys of the theoretical and empirical developments in the estimation of the investment relation in the presence of asymmetric information or agency costs.

or explicitly by specifying *a priori* a given adjustment cost function and deriving an estimated investment equation from the firm intertemporal optimisation problem. [7] The issue of incorporating expectations about the future profitability of investment has been dealt with (or side-stepped) in three main ways: 1) the traditional approach where the past variables are used as implicit proxies for the expectations of future profits in the estimated investment equation, or a more elaborate variant in which the projections of future profits on past variables are first considered and then used as explicit proxies; [8] 2) the afore-mentioned TOBIN's *q* approach, where the firm market values are viewed as directly measuring expected future profits; 3) and the more recent EULER equation methodology. [9] The EULER equation method in essence removes the problem created by the need to construct expectations into the infinite future by taking first differences in the derivation of the investment equation, so that the current marginal product of capital (the capital-sales ratio if the production function has the COBB-DOUGLAS form) and the expected one-period change in adjustment costs are all that is needed to describe the change in expectations about the future profitability of investment. [10] In fact, as we will shall see in the coming section, the desire to stay close to the models estimated by EISNER and OUDIZ, as well as the experience of the difficulties and fragile or implausible results of the more ambitious approaches, has led us to focus on an error corrected version of the accelerator-profit model, in which neither the adjustment costs nor the expectations are explicitly formulated. [11]

All these different approaches, however, usually result in estimating investment equations with an autoregressive specification *(i.e.*, in which lagged values of the dependent investment are among the right hand side variables). In this case, the mere presence of firm specific (and unobserved) effects (correlated or not with the other right hand side variables) create particular difficulties for panel data estimation, which cannot be solved by the usual within firm transformation (or by first differencing) as in a more simple static specification. [12] The within firm estimator is inconsistent in large samples (*i.e.*, in samples of size N with N increasing) for a given number *T* of time periods; however, its asymptotic bias decreases with the number of periods *T*. [13] Beginning with BALESTRA and NERLOVE [1966] and ANDERSON and HSIAO [1982], various instrumental variable solutions to this problem have been suggested in the literature, and the current state of art is the use of fully

7. See JORGENSON [1966] on rational distributed lags, and EISNER and STROTZ [1963], and GOULD [1968] on adjustment costs.
8. See ABEL and BLANCHARD [1986] for a recent application of such a variant.
9. See in particular ABEL [1980], WHITED [1992], BOND and MEGHIR [1994], and BLUNDELL, BOND and MEGHIR [1996].
10. As in all the panel data models described here, there will also be macro-economic changes in investment prices and interest rates that are subsumed in the time dummies. Only when there is variation in these quantities across firms for tax or other reasons will they enter the EULER equation, or any other investment equation for that matter.
11. See also OLINER, RUDEBUSH and SICHEL [1995] on the poor forecasting performance of the EULER equation and TOBIN's q approaches.
12. Note that for this reason EISNER, OUDIZ and MAIRESSE-DORMONT have been careful to consider a non-autoregressive formulation of the accelerator model. See below in next section.
13. See NICKELL [1981] for the computation of this asymptotic bias.

efficient GMM estimators that allow for heteroskedasticity across firms, and serial correlation over time. We pursue this strategy in this paper.

3 The Accelerator Model of Investment with Error Correction

Our approach is in the spirit of BOND, ELSTON, MAIRESSE and MULKAY [1997], in that rather than focusing on finding the "*correct*" model of investment, we use an error-correction formulation of the accelerator-profit model. This formulation encompasses the earlier literature as exemplified in EISNER and OUDIZ, and can also be related to the recent new wave of firm-level empirical work trying to ascertain the sensitivity of investment to financial constraints, which is based on the EULER equation approach. It has the advantage of allowing us to explicitly separate the specification of long run determinants of investment from that of short run adjustment and expectation lags. We can thus assume that sales and capital are proportional in the long run, as in the simple neoclassical theory, while in the short run the dynamics relating the two may be complex and specified in *ad hoc* distributed lag manner. [14]

More precisely, we start by considering a base model which implies that the long run capital stock of the firm is proportional to output:

(1) $$k_{it} = \theta s_{it} + h_{it},$$

where k_{it} is the log of the capital stock K_{it} for firm *i* at the end of year *t*, and s_{it} is the log of the output or sales S_{it} for the firm *i* in year *t*, and h_{it} denotes a function of the log of the user cost of capital (and of the parameters of the production function). This relationship is consistent with the simple neoclassical model of a profit-maximizing firm with a single type of capital, a CES or COBB-DOUGLAS production function, and no adjustment costs, as shown in Appendix A.

Next we specify a dynamic adjustment mechanism between *k* and *s* as an autoregressive-distributed lag of length two (an ADL(2,2) function), which nests equation (1) as its long-run solution, and we also assume that the variation in the user cost of capital/productivity term h_{it} can be controlled for in the equation by including year-specific and firm-specific effects. These assumptions yield the following accelerator-type equation:

(2) $$k_{it} = \alpha + \gamma_1 k_{i,t-1} + \gamma_2 k_{i,t-2} + \beta_0 s_{it} + \beta_1 s_{i,t-1} + \beta_2 s_{i,t-2} + \eta_{it},$$

14. Note that along the lines of recent theoretical work by PESARAN, SHIN, and SMITH [1999], it is possible to allow the short run dynamics to vary across firm. We intend to pursue this possibility in future work.

where the disturbance $\eta_{it} = \varepsilon_{it} + \alpha_i + d_t$ contains firm and year-specific effects α_i and d_t, as well as transitory shocks ε_{it}. Rewriting this equation in an error-correcting framework, we obtain:

$$\begin{aligned} \Delta k_{it} &= \alpha + (\gamma_1 - 1)\Delta k_{i,t-1} + \beta_0 \Delta s_{it} + (\beta_0 + \beta_1)\Delta s_{i,t-1} \\ &\quad + (\gamma_1 + \gamma_2 - 1)(k_{i,t-2} - s_{i,t-2}) \\ &\quad + (\beta_0 + \beta_1 + \beta_2 + \gamma_1 + \gamma_2 - 1) s_{i,t-2} + \eta_{it}. \end{aligned} \tag{3}$$

which expresses the growth rate of capital stock as a function of both growth rates and levels information: its own lagged growth rate, the growth in sales (current and lagged once), an error correction term (the log of the capital-output ratio) and a scale factor (the log of sales). Writing the equation this way is convenient because the last two terms provide simple *t*-tests for error-correcting behavior and constant returns to scale in the long run, while the first three variables capture the short-run dynamics. We expect that the error correction coefficient $\rho = \gamma_1 + \gamma_2 - 1$ will be negative, implying that if capital is less than its "*desired level*" future investment will be higher and conversely. We would also expect that the scale coefficient $\lambda = \beta + \rho = \beta_0 + \beta_1 + \beta_2 + \gamma_1 + \gamma_2 - 1$ (with $\beta = \beta_0 + \beta_1 + \beta_2$) would not be statistically different from zero, implying that the long run elasticity of capital to sales $\theta = -\beta/\rho = 1 - \lambda/\rho$ is unity. [15]

We then augment equation (3) with the current and lagged ratios of profits to beginning of period capital stock. These ratios should capture effects which are associated with financial or liquidity constraints, and/or with changes in profitability that are not fully accounted for by the sales growth variables. Finally, in estimation, we use the investment ratio $\frac{I_{it}}{K_{i,t-1}}$ as a proxy for the net growth in capital stock Δk_{it}, where I_{it} is the investment of firm *i* in year *t*.[16]

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15. This will be the case if a COBB-DOUGLAS function or a CES function with constant returns to scale are a good enough approximation to the underlying production function: see Appendix A.
16. We have :

$$\Delta k_{it} = \log\left[\frac{K_{it}}{K_{i,t-1}}\right] = \log\left[1 + \frac{\Delta K_{it}}{K_{i,t-1}}\right] \cong \frac{\Delta K_{it}}{K_{i,t-1}} \cong \frac{I_{it}}{K_{i,t-1}} - \delta$$

where δ is the (average) depreciation rate. The approximation of the growth in capital Δk_{it} by the net investment rate $(\frac{I_{it}}{K_{i,t-1}} - \delta)$ is likely to be fairly good, since their median values in our samples are quite small. Note that the variation in δ now enters directly in the disturbance in addition to the cost of capital/productivity term h_{it} and that we are assuming that this variation can also be controlled in estimation by year and firm effects.

Denoting profits as II, our final estimating equation is thus the following linear regression:

$$
(4)\quad \begin{aligned}
\frac{I_{it}}{K_{i,t-1}} &= \alpha + (\gamma_1 - 1)\frac{I_{i,t-1}}{K_{i,t-2}} + \beta_0 \Delta s_{it} + (\beta_0 + \beta_1)\Delta s_{i,t-1} \\
&\quad + (\gamma_1 + \gamma_2 - 1)(k_{i,t-2} - s_{i,t-2}) \\
&\quad + (\beta_0 + \beta_1 + \beta_2 + \gamma_1 + \gamma_2 - 1)s_{i,t-2} \\
&\quad + \varphi_0 \frac{\Pi_{it}}{K_{i,t-1}} + \varphi_1 \frac{\Pi_{i,t-1}}{K_{i,t-2}} + \varphi_2 \frac{\Pi_{i,t-2}}{K_{i,t-3}} + \eta_{it}.
\end{aligned}
$$

We would expect that the sum of the coefficients on profits $\varphi = \varphi_0 + \varphi_1 + \varphi_2$, (or the corresponding long-run profits coefficient $-\varphi/\rho$) will not be significant, if the profits variable captures only the transitory effects of financial constraints on firm investment. The dynamic properties of the equation depend on the values and profile of the individual coefficients.

One can test for the presence of sales or profits by considering the joint significance of β_0, β_1 and β_2 or that of φ_0, φ_1 and φ_2. One can also test for the presence of lag two effects by looking at the joint significance of β_2, γ_2 and φ_2.

With the omission of the level terms in capital and sales k and s, our error correction model looks superficially like the traditional accelerator-profit model. However, the dynamic properties of the two specifications are different. The traditional accelerator is derived by differentiating equation (2), thus removing the possibility that it can express an equilibrium relationship in the levels of variables. Using again the investment rate as a proxy $\frac{I_{it}}{K_{i,t-1}}$ for the net growth in capital stock Δk_{it}, (and not writing the profits variable terms), we obtain:

$$
(5)\quad \begin{aligned}
\frac{I_{it}}{K_{i,t-1}} &= \gamma_1 \frac{I_{i,t-1}}{K_{i,t-2}} + \gamma_2 \frac{I_{i,t-2}}{K_{i,t-3}} \\
&\quad + \beta_0 \Delta s_{it} + \beta_1 \Delta s_{i,t-1} + \beta_2 \Delta s_{i,t-2} + \Delta \eta_{it}.
\end{aligned}
$$

In this specification of the traditional accelerator the implied long-run parameter $-\beta/\rho = ((\beta_0 + \beta_1 + \beta_2)/(1 - \gamma_1 - \gamma_2))$, or so-called sales accelerator effect, characterizes the long-run relation between the rates of growth of capital and sales, and not between their levels.

Another specification of the traditional accelerator model is one with a non-autoregressive formulation but with possibly much longer lags on the independent sales variable, that is, if τ is the number of lags:

$$
(6)\quad \begin{aligned}
\frac{I_{it}}{K_{i,t-1}} &= \beta'_0 \Delta s_{it} + \beta'_1 \Delta s_{i,t-1} + \beta'_2 \Delta s_{i,t-2} + \beta'_3 \Delta s_{it-3} \\
&\quad + \ldots + \beta'_{\tau-1} \Delta s_{i,t-(\tau-1)} + \beta'_\tau \Delta s_{i,t-\tau} + \Delta \eta_{it},
\end{aligned}
$$

where the accelerator effect is then simply $\beta' = (\beta'_0 + \beta'_1 + \ldots + \beta'_{\tau-1} + \beta'_\tau)$.[17] This is in fact the specification used by EISNER, OUDIZ and MAIRESSE-DORMONT and by us here in the part of our analysis comparable to theirs (*i.e.*, section 6). One advantage of this specification relative to the autoregressive one is that the serial correlation of the disturbances is not *per se* a cause of bias for the usual panel data estimators.

In contrast to the traditional accelerator specification (5) or (6), the error correction specification (3) is just a reparametrization of the same equation (2) that retains information about the long-run equilibrium between *k* and *s* in addition to the short-run relationship between the rates of growth of the variables.[18] Moreover, controlling for firm specific effects in panel data estimation has very different consequences in the error correction model than in the accelerator model. In the former (error correction) case, these effects (the α_i's) correspond to *different levels* of the capital-output ratios, while in the latter (traditional accelerator) case these effects correspond to *different trends* in capital and output growth rates.[19]

The error correction specification also has the advantage of making our work more comparable to much of the recent literature on firm-level investment that uses an EULER equation framework to look for evidence of "*excess sensitivity to cash-flow*".[20] The typical EULER equation framework derived from the firm's intertemporal optimization problem under the assumption of a quadratic adjustment cost function leads to an estimating equation which is empirically not that different from the accelerator-profit equation with error correction. In particular the profit rate variable, measured by the operating income to capital ratio or by the cash-flow to capital ratio (both being highly correlated), should appear in the EULER equation with a negative sign in the absence of financial constraints on investment, reflecting that the marginal profitability of capital (average profit under constant returns to scale) is related to the change in the marginal adjustment costs in adjacent periods. The finding that the profit rate variable has usually a positive coefficient in estimation is in general interpreted as indicating that firms are in fact subject to financial constraints. [21] In the error-correcting specification, in addition to assessing the sign of the profit rate coefficients, we can test whether profit plays the role of a long-run determinant of investment, or whether it is only a short-run variable which can be interpreted as reflecting the transitory availability of funds for investment purposes.

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17. This specification can be viewed as an approximation obtained by inverting the autoregressive specification (5) and neglecting the lag terms of order higher than τ.

18. See HENDRY, PAGAN and SARGAN [1984].

19. Such firm specific trend effects ζ_i would be implied by a disturbance η_{it} in equation (2) of the form: $\eta_{it} = \varepsilon_{it} + \alpha_i + d_t + \zeta_i(t)$. Furthermore note that even in the absence of such effects if ε_{it} is serially uncorrelated in (3) $\Delta\varepsilon_{it}$ is not, but follows an MA(1) process in (5) or (6).

20. See BOND, ELSTON, MAIRESSE and MULKAY [1997] for such a comparison.

21. See, however, KAPLAN and ZINGALES [1997] for a different interpretation.

4 From Traditional to GMM Panel Data Methods of Estimation

As it stands, the econometric model of the traditional or error correction specification of the accelerator-profit investment equation is the usual linear regression model written for panel data with firm effects and year effects:

(7) $$y_{it} = x_{it}\beta + \eta_{it} = x_{it}\beta + \alpha_i + d_t + \varepsilon_{it}$$

with $i = 1,\ldots,N$; and $t = 1,\ldots,T$, and where y_{it} is the rate of growth of capital or the investment rate to be explained and x_{it} is the vector of explanatory variables (including lagged y_{it}). The presence in the overall error terms η_{it} of firm and year effects α_i and d_t, in addition to idiosyncratic pure disturbances or shocks ε_{it}, is supposed to account for a variety of specific errors. As we have just pointed out, the significance of the firm effects is not the same in the error correction specification and the traditional specification. In the former, they correspond to differences in the technology of firms, in their capital-output ratios, in the rate at which their capital depreciates and the rate of return required by financial markets, or in the construction of the accounting measures used for estimation, while in the latter they would correspond to varying trends among firms in such differences.

Because in our samples the number of firms N is reasonably large (between 400 and 500) while the number of years T is relatively small (between 6 to 9), our focus is on the treatment of the "*permanent unobservable differences*" across firms, the α_i's, while we estimate the d_t's simply by including a full set of year dummies in all regressions. We consider and try to correct for the possible estimation biases in the parameters of interest β (or given functions of them) that can arise from the potential correlations of the x_{it}'s with the α_i's and the potential endogeneity of the x_{it}'s with respect to the past or current disturbances or shocks ε_{it}'s.

In current practise (and as in the work of OUDIZ, EISNER, and MAIRESSE and DORMONT), the first problem is dealt with by the usual within firm transformation or by first differencing, that is by estimating the following variants of equation (7):

$$y_{it} - \bar{y}_{i\cdot} = (x_{it} - \bar{x}_{i\cdot})\beta + (d_t - \bar{d}) + \varepsilon_{it} - \bar{\varepsilon}_{i\cdot})$$

or

$$(y_{it} - y_{i,t-1}) = (x_{it} - x_{i,t-1})\beta + (d_t - d_{t-1}) + (\varepsilon_{it} - \varepsilon_{i,t-1})$$

where a bar over a variable denotes its time-average over the sample period.[22] The α_i's being therefore eliminated, the "*within estimates*" or "*first differenced estimates*" are free from the potential biases of correlated effects. In current practise, the so-called "*total estimates*" obtained on the untrans-

22. Thus $\bar{x}_{i\cdot}$ is the mean of x_{it} for the *i*-th firm, and $\bar{d}$ is the average of the year dummies d_t.

formed equation (7), and the "*between estimates*" obtained on the equation on firm means:

$$\bar{y}_i . = \bar{x}_i .\beta + (\bar{\alpha} + \bar{d}) + (\alpha_i - \bar{\alpha} + \bar{\varepsilon}_i .)$$

are also usually computed. The between and within estimates are based on an orthogonal decomposition of the total variability of y and x into the variation between firm in the cross-sectional dimension of the data and the variation within firm in the time series dimension of the data. Given the potential correlations of the α_i's and the x_{it}'s, it is to be expected that these estimates, and also the total estimates (which can be viewed as a matrix weighted average of them), will not often be the same. [23] In his work, EISNER do not consider that the differences between these various estimates are just a mere reflection of biases, but he interprets them (and this interpretation is formalized by MAIRESSE-DORMONT) as expressing interesting differences between the longer run and shorter run aspects of investment behavior. [24]

Although the within and first differenced estimates will take care of the biases from correlated effects, they can also be biased for three other possibly important types of reasons: (i) random measurement errors in the right hand side variables x_{it} (which will probably tend to impart downward biases in the coefficients of the variables inaccurately measured); (ii) simultaneity between the contemporaneous x_{it} and the contemporaneous disturbance ε_{it} (which will probably tend to impart upward biases in the coefficients of the variables with positive correlation with the disturbance); and (iii) endogeneity of the contemporaneous x_{it} with respect to the past disturbances. A solution to these three sources of biases is to use an instrumental variables (IV); estimation method, where the instruments are an appropriate set of lagged values of the variables. Allowing for the heteroskedasticity of the disturbances across firms and their possible correlation over time, this method takes the form of what is now known as the GMM method of estimation. [25]

The biases from random measurement errors in the first differenced and within estimates may be very severe; they will tend to affect particularly the first differenced estimates but less so the within estimates (and the lesser the longer the period of study), even when they remain small or even negligible for the untransformed total estimates.[26] The biases from the endogeneity of

23. See for example MAIRESSE [1990].

24. We follow briefly this line of interpretation in our comments of the results in Section 6. However, as we already suggested, we rather favor the error correction specification of the investment relation which provides a neat distinction between the long run and the short run. Let us repeat again in this respect that the traditional accelerator specification should remove the firm specific effects being present in the error correction specification. Thus one would expect that the within estimates (or first differenced) estimates on the latter should be more comparable to the total (or between) estimates on the former. This is indeed what we will see in Section 7.

25. IV and GMM methods in the context of panel data have been developped over the years by a number of authors; see in particular BALESTRA and NERLOVE [1966], ANDERSON and HSIAO [1982], CHAMBERLAIN [1982], GRILICHES and HAUSMAN [1986], ARELLANO and BOND [1991], KEANE and RUNKLE [1992], AHN and SCHMIDT [1995].

26. This arises from the magnification of the "*noise to signal*" ratio *(i.e.*, the ratio of the variance of the serially uncorrelated measurement error in a variable and the net variance of this variable) by the differencing transformation. This magnification is larger for first differences than for the within (and for longer differences). See for example GRILICHES and HAUSMAN [1986], and MAIRESSE [1990].

variables with respect to past disturbances will also affect very differently the first differenced and the within estimates. In the case of the within these biases arise from the correlation of x_{it} with the average of past disturbances entering $\varepsilon_{i.}$ (*i.e.*, $[\varepsilon_{i1} + \varepsilon_{i2} + \ldots + \varepsilon_{i(t-1)}]/T$); they will thus fall and become negligible with the length T of the study period. In the case of the first differences the biases result from the correlation of x_{it} and the one year lagged disturbance ($\varepsilon_{i(t-1)}$) and hence will remain the same in a longer sample.

Because it is highly probable that investment rates, output, and profits are simultaneously determined and likely also that they are affected by a modicum of measurement errors, a major goal of this paper is to investigate whether the earlier usual estimates were subject to substantial biases from these sources and whether the newer GMM estimates are able to help. Our GMM estimates are based on instrumenting the differenced version of equation (7) by the lagged level-values of the variables. Under the assumption that the disturbances ε_{it} are not serially correlated, we expect $\Delta\varepsilon_{it} = \varepsilon_{it} - \varepsilon_{it-1}$ to be orthogonal to the past history of the *x* and *y* variables (after the first lag), so that $y_{i,t-2}, y_{i,t-3}, \ldots x_{i,t-2}, x_{i,t-3} \ldots$ are available as instruments for $\Delta\varepsilon_{it}$. If the disturbances ε_{it} follow a moving average process of order one MA(1), the first valid instruments are found at the third lag instead of the second because the differenced disturbances $\Delta\varepsilon_{it}$ follow an MA(2) process.

ARELLANO and BOVER [1991] and BLUNDELL and BOND [1998, 1999] have suggested that if equation (7) is the true model, it is also possible to instrument the untransformed equation, which contains the firm effects α_i's, using the lagged differences of the *x*'s and *y*'s, since these presumably do not contain such firm effects. The "*system*" GMM combining the two sets of instruments results in estimates which can be much more efficient than the "*first-differenced*" GMM alone. However, we shall not report here on these newer system-GMM estimates; in first experiments with them we typically found that they were rather different from the first-differenced ones, implying that the assumptions required for their consistency do not hold in our data. [27]

In the estimation, we pay close attention to two requirements, the exogeneity or validity of the instruments and their relevance. These are two basic issues with all instrumental variables methods and GMM in particular: first, the instruments should be uncorrelated with the error terms, or equivalently the GMM orthogonality conditions should be satisfied by the data; second, the instruments should have reasonably strong correlations with the instrumented variables. Clearly the two requirements will generally tend in opposite directions for the choice of an appropriate set of instruments. The validity of instruments is usually verified by the classical SARGAN test of the over-identifying restrictions; it can also be substantiated by additional LAGRANGE Multiplier (LM) tests of autocorrelation of errors so as to confirm the exogeneity of the (t – 2) or (t – 3) lagged instruments.[28] Although often neglected in empirical work, the issue of relevance is of great practical importance because

27. In future work, we intend to reexamine the question, since a major difficulty with the first-differenced GMM estimates is their poor precision, as we shall see in our results in section 7. See also MAIRESSE and HALL [1996], and GRILICHES and MAIRESSE [1998].

28. See HANSEN [1982] for the SARGAN test and ARELLANO and BOND [1991] for the autocorrelation test.

TABLE 1

Descriptive Statistics – France & U.S.

	FRANCE 1971–1979 (441 Firms)					**U.S. 1971–1979 (407 Firms)**				
	Median	Mean	St. Dev.	Minimum	Maximum	Median	Mean	St. Dev.	Minimum	Maximum
E (number)	628	1 511	2 364	17	16 539	9 186	24 135	56 416	180	853 000
S (MF or M$)	60.1	175.2	306.2	1.4	3 369.7	278.5	821.1	2 210.3	4.9	37 575.5
K (MF or M$)	24.0	74.2	146.1	0.6	1 238.1	90.2	400.5	1 160.4	0.7	16 999.3
I (MF or M$)	2.4	8.3	18.5	0.0	284.4	11.9	55.8	177.8	0.0	3 250.7
K / *S*	0.392	0.476	0.298	0.037	2.776	0.361	0.439	0.278	0.063	2.680
I / *S* (%)	3.58	4.99	5.13	0.00	90.79	4.43	5.64	4.65	0.19	85.16
I / *K* (%)	9.46	12.04	10.00	0.00	141.954	12.74	14.38	8.56	0.47	111.00
OPINC / K (%)	23.40	27.82	21.51	–50.64	212.39	32.59	37.61	23.58	–51.45	193.51
S Growth (%)	4.46	4.04	14.27	–81.47	70.29	5.27	4.31	12.28	–61.57	61.30
	1985–1993 (486 Firms)					**1985–1993 (482 Firms)**				
	Median	Mean	St. Dev.	Minimum	Maximum	Median	Mean	St. Dev.	Minimum	Maximum
E (number)	552	1 446	5 027	78	91 049	5 100	19 914	51 849	58	876 800
S (MF or M$)	220.2	794.1	3 558.3	12.3	66 332.7	501.9	2 411.6	7 294.7	5.2	110 677.9
K (MF or M$)	82.4	352.2	1 736.7	1.2	29 528.8	213.3	1 536.1	5 230.9	1.6	93 799.2
I (MF or M$)	8.3	37.6	192.5	0.0	3 479.2	25.5	182.9	667.6	0.0	13 279.8
K / *S*	0.395	0.431	0.230	0.032	2.034	0.476	0.547	0.313	0.060	2.157
I / *S* (%)	3.36	4.31	3.90	0.00	58.45	4.91	6.11	4.87	0.12	63.33
I / *K* (%)	9.18	11.24	8.94	0.00	111.64	11.21	12.93	9.12	0.46	101.08
OPINC / K (%)	21.37	25.01	22.81	–93.49	259.42	26.40	31.27	24.01	–57.34	269.16
CF / K (%)	13.84	15.15	16.24	–107.07	160.49	17.50	19.71	17.77	–80.87	157.04
S Growth (%)	1.98	1.89	11.93	–59.76	69.75	1.83	1.38	13.49	–68.08	66.50

Variables : *E* : Number of Employees;
S : Total Sales (in millions 1985 FRF or USD);
K : Capital Stock at the begining of the year (in millions 1985 FRF or USD);
I : Capital expenditures (in millions 1985 FRF or USD);
OPINC : Operating Income; CF : Cash-Flow = Gross income after taxes and interest.

using “*weak*” (weakly relevant) instruments reduces dramatically the precision of the estimates.

In order to assess the relevance of the instruments, NELSON and STARTZ [1990], followed by BOUND, JAEGER and BAKER [1995], propose using the *R*-squares and *F*-statistics from the regressions of the instrumented variables in the model on the set of instruments. Along the same line, SHEA [1997] shows that simple *R*-squares can be misleading because of the intercorrelations between the instrumented variables and he advocates instead the computation of partial R-squares taking care of these intercorrelations. A.R. HALL, RUDEBUSH and WILCOX [1996] suggest instead the computation of the canonical correlations between the set of instrumented variables and that of the instruments, and they propose testing the significance of the smallest of these canonical correlations. We report in details on the use of these various diagnostic indicators in Section 7 where we discuss our GMM estimates.

5 French and United States Firm Panel Data Samples

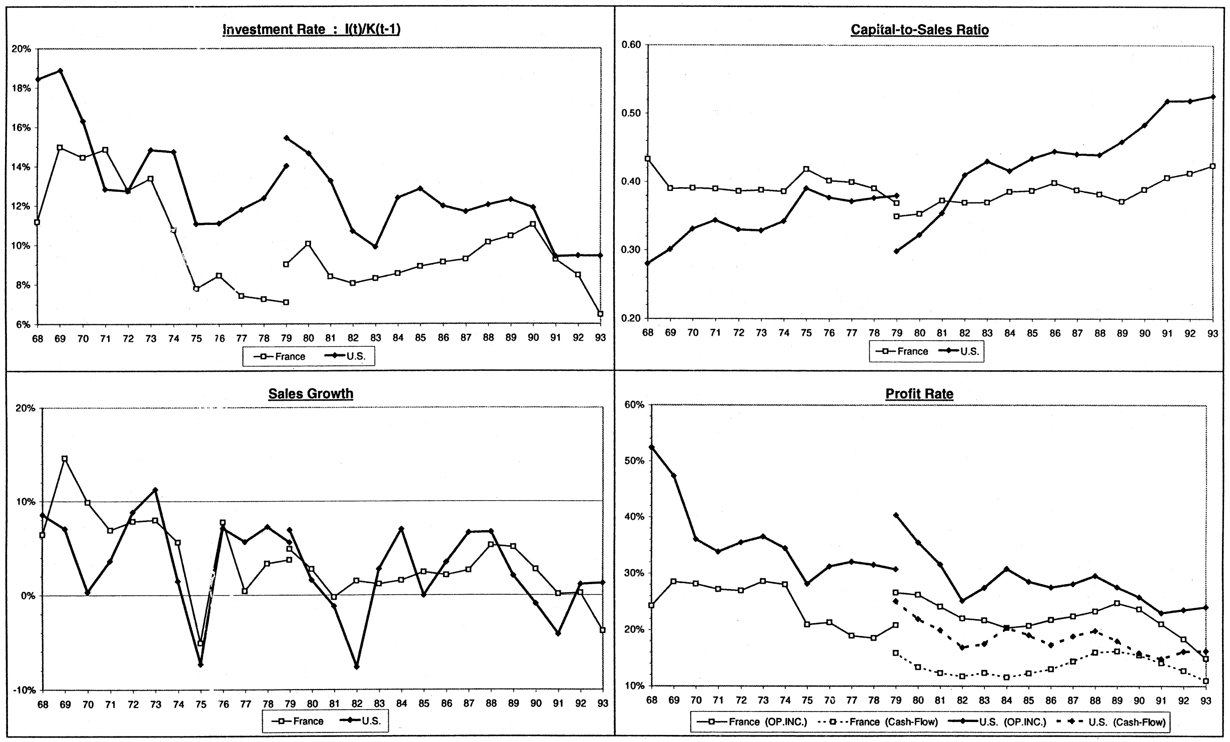
We consider in the analysis four balanced samples of about 400 to 500 firms: for France and the United States, and each of the two periods 1968-1979 and 1979-1993 respectively. Their construction is briefly described in Appendix B. Some of the differences across countries reflect the relative sizes of the economies (roughly 3 to 1) and others may result in part from the sampling frame: the U.S. sample consists of publicly traded manufacturing firms that report their accounting data to the Securities and Exchange Commission, whereas the French sample, which is drawn from a very large database, is closer to a Census of manufacturing firms, and includes a number of non-quoted firms. However, the variables used in our regressions have been constructed from the firm current accounts according to basically the same definitions (investment, sales, cash-flow and operating income) or computed in the same way (capital stocks); and the samples have been cleaned from outliers in the same manner.

Table 1 presents descriptive statistics for these variables in the four samples and the precise periods used in our GMM estimation (1971-1979 and 1985-1993). The median firm size in terms of employment is much higher in the U.S. samples than in the French samples (about fifteen times in the earlier samples and ten times in the recent ones), in spite of the fact that for the most part these samples consist of the largest manufacturing firms in each country. The order of magnitude and range of variation of the key variables in terms of ratios are nonetheless quite comparable: the investment rate I/K, the capital-output ratio K/S, the growth of sales Δs, and the “*profit rate*” as measured by the operating income to capital ratio (OPINC/K) for the first period and by both the operating income and cash-flow to capital ratios (OPINC/K and

FIGURE 1

Change in Main Variables

Median - French and U.S. Manufacturing (Balanced Samples)



CF/K) for the second period. Note though that the U.S. firms tend to have higher investment and profit rates on average than the French firms.

Figure 1 compares the evolution over time of the medians of these key variables for France and the United States. The two countries display roughly similar behavior overall between 1968 and 1993. The break in the series in 1979 is due to the composition of the samples which do not consist of the same firms; it is especially marked in the United States, where the earlier sample includes fewer smaller firms.

After a rapid fall during the earlier period, the investment rate for France seems to be somewhat more stable in the second period, while in the United States it tends to decline more or less throughout the first and second periods. These patterns are roughly consistent with those for the profit rates (but not for France in the first period): the operating income and cash-flow ratios decline in the U.S., while in France they are roughly constant and at a lower level than in the U.S. Even at this aggregate level, it does appear that higher investment rates are associated with higher profit rates and lower investment rates with lower profit rates. The growth rates of (deflated) sales are quite comparable in the two countries in the two periods, and show an important slowdown in the (post-oil shocks) second period. The capital to sales ratio appears to be growing rapidly for the U.S. samples over the two periods, while it remains more or less stable for the French samples.

Although the overall patterns of evolution of the investment and profit rates and the capital to sales ratios in the two countries seem plausible, one may be a bit suspicious of some of the differences in them and wonder if they could be possibly related to our computation of the capital stocks by the permanent inventory method (see Appendix B). However, experimenting with reasonable variants for such computation did not lead to very different evolutions, nor did it affect in any significant way the estimates in our econometric analysis.[29]

29. One might think in particular that the choice of the benchmark values adopted for our computation of K by the permanent inventory method could partly account for the more or less rapid evolutions of I/K, CF/K or OPINC/K, and K/S, specially in the first years of the two periods. In both countries, we haved used the net book value of fixed assets (roughly adjusted for inflation) in the first year of data for each firm as a benchmark. However, varying this benchmark value did not change these evolutions much.
Another source of discrepancy could be that in the French National Accounts the prices of equipment goods (which we used as deflators of nominal investment figures) were not adjusted for quality (especially computers and related equipment until 1985), contrary to the U.S. National Accounts (or at least not to the same extent). This is far from enough, however, to result in markedly different evolutions of our overall equipment capital stocks numbers.

6 The Traditional Accelerator: Now and Then

In this section we compare the estimates obtained on our four panel data samples to those of EISNER, OUDIZ, and MAIRESSE-DORMONT, using the same accelerator-profit specification of the investment equation and computing the same estimates. There are, however, many variants in the precise way in which the analysis can be carried out, and hence some differences in its actual implementation by these authors themselves and some other differences in its replication by us, that we could not fully avoid. These variants mainly concern the definitions and measures of the sales and profit variables, and in what exact form and with how many lags they enter the estimated equation. In fact, many have already been documented in the EISNER, OUDIZ and MAIRESSE-DORMONT papers, and we have also investigated several (those we could and thought might matter!). Most turn out to have negligible or little effect on the estimates, and only a few appear to be of some possible real significance. In these cases we have tried to reproduce the earlier analyses as precisely as possible. In total, we think we have been able to control for the potentially most influential differences, with, however, two notable exceptions: that of the profit measure, for which we do not have some of the relevant data for the earlier period, and that of the capital stock measure, for which we prefered a net value measure to a gross value one.

The estimates reported by both EISNER and OUDIZ are based on a cash-flow measure of profit, while those reported by MAIRESSE-DORMONT are based on an operating income measure. We have only this last measure for our earlier period samples but we have both measures for our recent period samples.

Experimenting with cash-flow versus operating income on the recent samples shows that the magnitude of the operating income coefficients tends to be smaller than that of the cash-flow coefficients by a factor of about one third to one half (which is what would be expected on the basis of the difference of their sample means), but that their statistical significance and the overall fit of the investment equation is very much the same.[30]

There are also differences in the way the investment, profit and sales variables are "*normalized*" to enter the estimated equation. The one which turns out to really matter is the use of a net capital stock measure rather than a gross measure. While our predecessors use a gross value measure (based on

30. Note also that in EISNER the cash-flow measure is net of depreciation (*i.e.*, net profit), while the one in OUDIZ is gross of depreciation *(i.e.*, gross profit). Like OUDIZ (see his section 4.1), we prefer the gross cash-flow measure, which corresponds to the internal funds available for investment. Moreover, the depreciation figures reported in the firm accounts reflect in part their economic situation and their dividend policy; in the case of France, they are computed on the basis of fiscal service lives which are much shorter than the economic service lives. EISNER and OUDIZ also report estimates of the accelerator-profit model in which they enter the accounting depreciation rate as a separate variable in addition to the net profit rate. Although they find some significant impact on some of their coefficient estimates (depending on the type of estimates), the basic picture remains about the same.

the gross book values of fixed assets given in the firm balance sheets), we prefer a net value measure, as computed by the permanent inventory method under the assumption of geometric depreciation at a constant rate (see Appendix B), which is more strictly in line with the underlying economic model (see Appendix A). MAIRESSE-DORMONT show that this choice can make a sizeable difference on the estimates of the accelerator effects (in a proportion which is also about what could expected on the basis of the difference in the average magnitude of the two types of capital stock measures).[31]

We have also experimented with lag lengths, finding as did our predecessors that one or two lags (and the current value) was enough for profits, and that six lags for sales like EISNER rather than three like OUDIZ and MAIRESSE-DORMONT could matter to some extent.

In addition to the variants in the measurement of variables, the earlier papers also differ in the variety of estimates on which they have chosen to report. EISNER thus presents five types of estimates: total overall, total with year dummies, total with industry-year dummies, between firm and within firm (which in his terminology, also adopted by OUDIZ, he calls respectively: firm overall, firm cross sections across industries, firm cross sections within industries, cross sections of firm means across industries, and firm time series). He also discusses a few others that were done using data aggregated to the industry level rather than the firm level data. OUDIZ considers four out of the five firm level estimates, omitting the totals with industry-year dummies. By contrast, MAIRESSE-DORMONT limit themselves to two: the total overall and between firm estimates, which they also call first differences or year growth rates estimates, and long differences or average growth rates estimates, where their terminology refers to the original capital-sales relationship and not to the derived investment equation. [32] For the same reasons they did (see their section 2.3 and footnote 11), we choose to focus on these two estimates, although we also show the within firm estimates for the sake of completeness. [33]

Chosen on the basis of these various considerations, our most comparable estimates to EISNER, OUDIZ and MAIRESSE-DORMONT are shown in Tables 2, 3,

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31. The other normalization differences do not make significant differences to the results. EISNER measures the firm current investment and profit rates relative to the firm fixed assets in a given year (*i.e.*, 1957) rather than in the beginning of the current year t (or end of the previous year $t-1$), as OUDIZ, MAIRESSE-DORMONT and we in this paper. He also normalizes the current change in firm sales by an average of firm sales around this same year (*i.e.*, 1956, 1957 and 1958), while OUDIZ normalizes it by a moving average of the current and previous two years firm sales, and while MAIRESSE-DORMONT and we simply take the log differences in firm sales. EISNER reports that taking the measures used by OUDIZ instead of his preferred ones his "*major results are essentially undisturbed*", (EISNER [1978a], p. 127). We also confirmed this observation using the more straightforward normalization of MAIRESSE-DORMONT rather than those of EISNER and OUDIZ.

32. Note that MAIRESSE-DORMONT include industry dummies in their total and between regressions. Note also that they run the between regressions without the lagged sales and profit variables. This is reasonable since lagged values are indeed highly collinear with the current values, both being computed as the firm average growth rates over overlapping periods. This, however, may result in somewhat smaller estimates of the accelerator and profit effects in between.

33. These within estimates are based on the deviations of the year growth rates from their firm averages, and thus, as we already stressed (in section 3), they involve a "*double differentiation*" of the basic capital-sales relation, implicitly assuming (correlated) firm specific trends in this relation in addition to the (correlated) firm specific effects.

TABLE 2

Accelerator Model for I/K
Comparing Eisner and New Estimates for the U.S.
Between, Total, and Within Estimates

	EISNER (1961–1968)						**U.S. (1974–1979)**						**U.S. (1985–1993)**					
	Between		Total		Within		Between		Total		Within		Between		Total		Within	
# obs. (# firms)	533	533	4518	533	4518	533	407	407	2442	407	2442	407	482	482	4338	482	4338	482
Ds (*t*)	0.150	(.064)	0.094	(.002)	0.068	(.008)	0.304	(.081)	0.204	(.016)	0.134	(.020)	0.400	(.077)	0.179	(.013)	0.116	(.013)
Ds (*t* – 1)	0.095	(.075)	0.097	(.009)	0.067	(.008)	0.352	(.124)	0.144	(.012)	0.053	(.020)	0.172	(.110)	0.083	(.009)	0.017	(.011)
Ds (*t* – 2)	–0.005	(.072)	0.086	(.008)	0.057	(.007)	0.031	(.123)	0.099	(.013)	0.018	(.019)	0.008	(.112)	0.095	(.009)	0.037	(.009)
Ds (*t* – 3)	0.182	(.064)	0.076	(.008)	0.039	(.008)	–0.078	(.098)	0.075	(.014)	0.014	(.018)	0.018	(.115)	0.057	(.008)	0.011	(.008)
Ds (*t* – 4)	–0.026	(.065)	0.073	(.008)	0.042	(.008)	0.009	(.079)	0.018	(.021)	–0.034	(.021)	0.079	(.104)	0.060	(.009)	0.021	(.009)
Ds (*t* – 5)	0.158	(.070)	0.069	(.009)	0.032	(.008)	0.132	(.089)	0.026	(.013)	–0.007	(.014)	–0.101	(.120)	0.034	(.008)	0.000	(.008)
Ds (*t* – 6)	0.129	(.062)	0.046	(.008)	0.016	(.008)	–0.014	(.060)	0.027	(.013)	0.014	(.014)	0.255	(.074)	0.056	(.009)	0.016	(.009)
Sum of sales coeff.	0.683	(.053)	0.541	(.021)	0.322	(.028)	0.736	(.048)	0.594	(.039)	0.193	(.080)	0.831	(.035)	0.564	(.024)	0.217	(.034)
CF/K (*t*)	–0.143	(.157)	–0.043	(.025)	0.052	(.024)	–0.225	(.065)	–0.058	(.018)	0.043	(.024)	–0.387	(.073)	–0.020	(.012)	0.065	(.015)
CF/K (*t* – 1)	0.301	(.166)	0.226	(.026)	0.282	(.024)	0.261	(.061)	0.127	(.018)	0.188	(.025)	0.425	(.074)	0.126	(.012)	0.193	(.015)
Sum of Π coeff.	0.157	(.023)	0.182	(.010)	0.334	(.022)	0.035	(.010)	0.069	(.009)	0.231	(.032)	0.038	(.011)	0.105	(.010)	0.258	(.019)
Std.err. (R-squared)	n.a.	0.354	n.a.	0.255	n.a.	0.188	0.0337	0.538	0.0686	0.283	0.0638	0.380	0.0300	0.679	0.0769	0.289	0.0725	0.367

EISNER reffers to EISNER [1978a], Unbalanced Sample, Table 2.3, p. 119 : Column (3) for Between, (4) for Total, and (2) for Within. Cfr. also EISNER [1978b], Table 4.6, p. 88.
All equations do not include time dummies, nor industry dummies.
For new estimates, heteroskedastic-consistent standard errors are shown.

and 4.[34] Let us start by checking how close, or different, they actually are for the earlier period.

We first note in Table 2 that the sales accelerator effects are quite close for EISNER and our early U.S. sample, being both about .70 in between and .55 in total, while in Table 3 they are lower, though not significantly so, for OUDIZ than for our early French sample, being respectively about .40 and .55 in between and about .30 and .45 in total. In Table 4, we also see that these effects are much lower, and significantly so, for MAIRESSE-DORMONT than for our early samples. However, one may find a nearly complete explanation for these lower estimates in the Appendix A of MAIRESSE-DORMONT paper, where they report much higher estimates when they normalize investment by the same net value measure of capital stock as we do (rather than by the gross book value of fixed assets).[35] Going across tables, one can also remark that our between and total estimates for France in Table 3 and the corresponding ones in Table 4 are close, but that for the U.S. they are higher in Table 2 than in Table 4. This last discrepancy, however, is largely due to the fact that our estimates given in Table 2 are obtained with six lags for sales (like EISNER's), while the ones given in Table 4 are obtained using only three lags for sales in total and none in between (like MAIRESSE-DORMONT estimates). All in all, it seems that our total and between estimates of the sales accelerator effects basically agree with the earlier studies.

The comparison of the profit effects is less satisfactory. Looking first at our results and those of MAIRESSE-DORMONT, which in the earlier period are based on the same operating income measure, we see that their estimates are significantly higher than ours, both in between and total and for the two countries, especially in France. These differences cannot be more or less fully imputed, unlike the accelerator effects, to our use of a net measure of capital (see the corresponding estimates in Appendix A of MAIRESSE-DORMONT which are only slightly less than the ones based on the gross book value of fixed assets). A similar conclusion seems likely when we compare our results to those of EISNER and OUDIZ. We find that their estimates of the profits effects are much higher than ours for the two countries and especially in between. The fact that we have to rely on an operating income measure in this earlier period while they use a cash-flow measure accounts very probably for a good part of these discrepancies, but not for the whole part. The differences in the period

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34. In order to be as comparable as possible, our early samples in Tables 2 and 3 do not cover the eight year period 1971-79, as in the rest of the paper: the U.S. sample only covers 1974-79, allowing us to have 6 lags for the sales growth rates as in EISNER, and the French sample covers 1971-75, which is the same period as OUDIZ. The corresponding estimates computed for the 1971-79 U.S. and French samples do not differ much in fact.
It remains of course that EISNER sample covers a period of about 10 years earlier than that of our (early) U.S. sample, and that the composition of OUDIZ sample and that of our French sample are quite different. In fact OUDIZ reports results for two data sets: the first of 195 medium and small firms, the second of 124 large and medium firms. The estimates found by OUDIZ for these two data sets show roughly the same picture; we document in Table 3 those obtained for the second one. For much more details on EISNER data and sample construction, see EISNER [1978 b]; for some more on OUDIZ samples, see MAIRESSE [1978].

35. The estimates of accelerator effects in Appendix A of MAIRESSE-DORMONT are the following: .60 in between and .35 in total for the U.S., and 55 in between and .35 in total for France. They are thus practically the same in between as ours, and somewhat smaller in total, but much less so than with their gross value measure of capital.

TABLE 3

Accelerator Model for I/K
Comparing OUDIZ and New Estimates for France
Between, Total, and Within Estimates

	OUDIZ (1971–1975)			**FRANCE (1971–1975)**			**FRANCE (1985–1993)**		
	Between	Total	Within	Between	Total	Within	Between	Total	Within
# obs. (# firms)	124 124	620 124	620 124	441 441	2205 441	2205 441	486 486	4374 486	4374 486
Ds (*t*)	0.196 (.090)	0.097 (.021)	0.047 (.021)	0.200 (.064)	0.142 (.019)	0.104 (.021)	0.227 (.094)	0.212 (.018)	0.178 (.018)
Ds (*t* – 1)	0.060 (.107)	0.080 (.025)	0.021 (.027)	0.158 (.079)	0.140 (.019)	0.071 (.023)	0.025 (.136)	0.109 (.013)	0.075 (.013)
Ds (*t* – 2)	0.030 (.153)	0.096 (.024)	0.048 (.025)	0.144 (.083)	0.105 (.017)	0.065 (.020)	0.217 (.150)	0.101 (.014)	0.061 (.014)
Ds (*t* – 3)	0.135 (.135)	0.042 (.022)	0.006 (.022)	0.042 (.066)	0.046 (.019)	0.022 (.022)	0.253 (.109)	0.076 (.012)	0.030 (.013)
Sum of sales coeff.	0.421 (.043)	0.315 (.052)	0.122 (.064)	0.544 (.050)	0.433 (.036)	0.264 (.056)	0.722 (.043)	0.498 (.029)	0.344 (.034)
Π/*K* (*t*)	–0.283 (.174)	0.051 (.037)	0.183 (.040)	–0.278 (.065)	0.020 (.023)	0.146 (.029)	–0.131 (.077)	–0.047 (.037)	–0.046 (.020)
Π/*K* (*t* – 1)	0.474 (.171)	0.181 (.037)	0.276 (.040)	0.331 (.063)	0.082 (.021)	0.174 (.027)	0.182 (.076)	0.116 (.037)	0.110 (.018)
Sum of Π coeff.	0.191 (.043)	0.232 (.078)	0.459 (.064)	0.052 (.044)	0.102 (.015)	0.320 (.039)	0.052 (.015)	0.069 (.012)	0.064 (.017)
Std.err. (R-squared)	n.a. 0.374	n.a. 0.206	n.a. 0.155	0.0487 0.343	0.1033 0.158	0.0993 0.223	0.0355 0.479	0.0802 0.195	0.0753 0.290

OUDIZ reffers to OUDIZ [1978], Table 3, p. 530 (Balanced Sample, Dataset 2: Large and Medium size Firms): Column 8 for Between, 9 for Total, and 10 for Within.
All equations do not include time dummies, nor industry dummies.
For new estimates, heteroskedastic-consistent standard errors are shown.

between our US sample and EISNER's (1974-1979 as against 1961-1968) and in the sample composition between our French sample and OUDIZ's (441 firms as against 124) remain of course another quite plausible part of the explanation.

Despite these more or less markedly different estimates, the overall finding about the "*more transitory*" nature of the profits effects in contrast to the "*more permanent*" nature of the sales accelerator effects, which is particularly stressed by all four previous authors, is also clearly confirmed by us. The "*between*" estimates of the profit effects in the investment rate equation (*i.e.*, the between first differences estimates, or long differences, in the log-capital to log-sales equation) are smaller than the "*total*" estimates (*i.e.*, first differences estimates); and these are themselves smaller than the "*within*" estimates (*i.e.*, within first differences estimates), which are also considered by EISNER and OUDIZ (and documented in Tables 2 and 3). The ordering of the different estimates for the sales accelerator effects is exactly the reverse. In simple words, the effects on investment of a given increase in sales changes (or a given acceleration in sales levels) are larger over a longer period than the same increase over a shorter period, while the opposite is true for a given gain in profits. EISNER, followed by OUDIZ, interprets this pattern of estimates along the same lines of explanation as M. FRIEDMAN's permanent income theory in accounting why cross-sectional type estimates of income elasticity in the consumption function are higher than time-series type estimates [see FRIEDMAN [1957], and EISNER [1967]. Past sales changes over a longer period relate more closely to expected "*permanent*" components of demand and hence to investment. On the other hand an (unanticipated) increase in cash-flow or operating income has a "*transitory*" impact by providing additional internal funds and alleviating liquidity problems when firms are financially constrained, and thus mainly tends to speed up investment. MAIRESSE-DORMONT propose to formalize this interpretation by computing underlying "*permanent*" and "*transitory*" effects for both the sales accelerator and profits; they make the extreme assumptions that sales and profits consist of two (unobserved) permanent and transitory components and that by definition these are respectively strongly and weakly serially correlated (their respective serial correlation going to 1 and 0 in the limit). We consider in the next section a different but more straightforward rationalisation in terms of the error correction specification.

Turning now to our results for the more recent period, we find that our estimates are on the whole close enough to the ones for the earlier period in both countries. In Table 4 all the corresponding estimates are practically the same in the two periods. In Tables 2 and 3 they are also not too different. The largest differences occur for the between estimates of the accelerator effects: .75 in the first period as against .85 in the second period for the U.S., and .55 as against .70 for France; these differences, however, are only at the verge of statistical significance (at the 5% conventional level of confidence). If anything one would have expected to find in these two tables that our estimated profit effects would be larger in the second period, where we use a cash-flow measure, than in the first period, where we use an operating income measure. The fact that they are close, and even smaller for the total estimates in France, could thus reflect some real decline of profits effects in the recent period, especially in France.

TABLE 4

Accelerator Model for I/K
Comparing MAIRESSE-DORMONT and New Estimates for France and the U.S.
Between and Total Estimates

FRANCE	**M-D (1970–1979)**				**FRANCE (1971–1979)**				**FRANCE (1985–1993)**			
	Between		Total		Between		Total		Between		Total	
# obs. (# firms)	307	307	3070	307	441	441	3969	441	486	486	4374	486
Sum of sales coeff.	0.349	(.049)	0.284	n.a.	0.502	(.044)	0.445	(.024)	0.534	(.042)	0.478	(.030)
Sum of Π coeff.	0.136	(.017)	0.175	n.a.	0.048	(.012)	0.099	(.011)	0.072	(.016)	0.066	(.012)
Std.err. (R-squared)	0.030	0.820	0.071	0.248	0.035	0.367	0.090	0.195	0.037	0.421	0.080	0.204
U.S.	**M-D (1970–1979)**				**U.S. (1971–1979)**				**U.S. (1985–1993)**			
	Between		Total		Between		Total		Between		Total	
# obs. (# firms)	422	422	4220	422	407	407	3663	407	482	482	4338	482
Sum of sales coeff.	0.349	(.035)	0.196	n.a.	0.639	(.041)	0.497	(.028)	0.617	(.035)	0.556	(.025)
Sum of Π coeff.	0.088	(.011)	0.135	n.a.	0.059	(.009)	0.089	(.009)	0.052	(.013)	0.104	(.010)
Std.err. (R-squared)	0.025	0.717	0.048	0.318	0.030	0.556	0.073	0.280	0.035	0.555	0.077	0.291

M-D reffers to MAIRESSE-DORMONT [1985], Table 3 for Between and Table 2 for Total (I/C equations for France and for U.S.).
All equations include industry dummies but not time dummies.
For new estimates, heteroskedastic-consistent standard errors are shown.

Finally, if we also compare our results across countries, our conclusion at this point will be that they do not differ much in fact. At best one could find a slight indication that the sales accelerator effects are somewhat higher in the U.S. than in France, and more so in the earlier period. While EISNER's and OUDIZ's estimates seemed to indicate that these effects were significantly higher in the U.S. than in France, and MAIRESSE-DORMONT estimates seemed instead to show that they were rather close, the evidence of our own estimates (which, as we have just seen, do not change much between the earlier and recent periods) tips the scales more in favor of the latter. The picture for the profit effects is more mixed, with very limited differences between the two countries in the first period, but possibly more pronounced ones in the second period.

7 The Error Correction Specification: Usual and GMM Estimates

In this section we present estimates for our error correction specification (4) of the accelerator-profit model for our two periods and two countries. We present first the usual total and within estimates for comparison to the earlier work, and then the newer first-differenced GMM estimates using lagged levels instruments. Let us consider them in turn.

The total and within regression results are shown in Table 5. If we start by comparing them simply in terms of *R*-squares to our previous corresponding results (same estimators, same data except for the first period which is longer) given in Tables 2 and 3, we see that in all cases the error-correcting specification fits the data better and more parsimoniously (fewer lags of sales are required) than the traditional specification (without error correction). For the within regression (where the increase in R-squares is greater) this improvement is clearly due to the error correction term, which is quite significant and negative. [36]

However, if the comparison of goodness of fit can make sense for the same estimators of the two accelerator model specifications, the error-correction one (4) and the traditional one (5) or (6), it does not in terms of their properties. As we have stressed (in section 3), the first is an appropriate reparametrization of the log-capital to log-sales equation in levels while the second is a first differenced version of this equation. Thus the within estimator on the level equation should be more directly comparable to the total estimator on the first-differenced version, both correcting for the biases possibly arising from (correlated) firm effects and being consistent in the

36. For the total regressions, the improvement arises from the presence of the lagged investment rate; this term (which to some extent proxies for firm effects) is quite significant while the error correction terms are insignificant, contrarily to what we find in the within regressions.

TABLE 5

Error Correction Accelerator Model for I/K
Comparing the Estimation Periods
Total and Within Estimates

	France (1971–1979)				**France (1985–1993)**				**U.S. (1971–1979)**				**U.S. (1985–1993)**			
	Total		Within		Total		Within		Total		Within		Total		Within	
# observations (# firms)	3969	441	3969	441	4374	486	4374	486	3663	407	3663	407	4338	482	4338	482
I/*K* (–1)	0.214	(.025)	0.021	(.025)	0.286	(.025)	–0.003	(.026)	0.253	(.036)	0.003	(.031)	0.238	(.025)	–0.102	(.025)
Ds (*t*)	0.089	(.014)	0.070	(.014)	0.188	(.018)	0.179	(.019)	0.135	(.016)	0.135	(.018)	0.149	(.014)	0.146	(.014)
Ds (*t* – 1)	0.061	(.012)	0.054	(.015)	0.070	(.014)	0.100	(.015)	0.069	(.016)	0.062	(.018)	0.044	(.010)	0.077	(.011)
k–*s* (*t* – 2)	–0.023	(.005)	–0.282	(.031)	–0.013	(.003)	–0.208	(.016)	0.000	(.005)	–0.300	(.034)	–0.008	(.003)	–0.218	(.016)
s (*t* – 2)	–0.001	(.001)	–0.106	(.014)	0.003	(.001)	–0.086	(.011)	0.001	(.001)	–0.063	(.016)	0.002	(.001)	–0.091	(.012)
CF/K (*t*)	0.035	(.017)	0.053	(.018)	–0.044	(.020)	–0.067	(.019)	0.024	(.018)	0.021	(.021)	0.016	(.013)	0.010	(.014)
CF/K (*t* – 1)	0.064	(.019)	0.090	(.019)	0.084	(.019)	0.070	(.017)	0.108	(.029)	0.169	(.026)	0.090	(.012)	0.105	(.012)
CF/K (*t* – 2)	–0.023	(.015)	0.027	(.015)	0.018	(.015)	0.012	(.014)	–0.046	(.019)	0.012	(.016)	0.014	(.013)	0.032	(.014)
Long Run Sales	0.937	(.045)	0.623	(.041)	1.209	(.081)	0.584	(.046)	–2.956	(62.681)	0.792	(.048)	1.289	(.106)	0.582	(.038)
Long Run CF	3.222	(1.030)	0.601	(0.114)	4.465	(1.703)	0.076	(.087)	–273.7	(4255.2)	0.674	(.119)	15.674	(6.281)	0.673	(.120)
Std.error (R-squared)	0.0863	0.256	0.0815	0.336	0.0769	0.260	0.0706	0.376	0.0718	0.297	0.0660	0.405	0.0763	0.300	0.0680	0.444

All equations include time dummies but not industry dummies.
Heteroskedastic-consistent standard errors are shown.

absence of other types of specification errors. These two estimators make more sense here and we shall focus on them in our comments. The other two estimators, which correspond to two opposite (and extreme) cases, appear to be inappropriate for different reasons. The total estimator on the level equation will only be consistent if the firm effects are negligible or uncorrelated. This seems *a priori* unlikely since these effects are explicitly supposed to proxy in the equation for the unobserved differences in the user cost of capital (and depreciation rates); this is also contradicted by the very significant differences which can indeed be seen in Table 5 between our total and within estimates. On the other hand, the within estimator on the first differenced equation should be consistent even in the presence of correlated firm specific trends (in addition to firm specific effects) in the initial level formulation, which could possibly reflect varying technological trends among firms. However, this means that only a very small share of the variability in the data is kept as relevant information; and this comes at the cost of a much increased fragility of the estimator to other types of specification errors, such as even very modest amounts of random measurement errors (in the sales levels and sales growth rates and in the profit ratios).

Focusing then on the within estimates, we see no large differences in the long run sales accelerator coefficient θ between the two periods and the two countries. It is about equal to 0.6 (0.8 for the U.S. first period) corresponding to a good-sized error correction term λ on the order of –0.2 to –0.3 and to a scale coefficient on the order of –0.1 (–0.05 for the U.S. first period). These estimates, even if we prefer our error correction specification, are indeed about roughly equivalent to the comparable ones found for the traditional specification (*i.e.*, the total and between estimates in Tables 2 and 3). Once we correct for firm specific effects (in the model in levels) by going within or first-differencing, we find that the estimated accelerator effects are much less than one. [37] Although the within estimates for the long run profit effects $-\varphi/\rho$ appear to be much higher than before, they show the same pattern with a similar value (of the order of 0.6 instead of 0.3) in the earlier period for the two countries, remaining about the same in the recent period for the U.S. but dropping to insignificance in France. As before since our profit measures are unfortunately different in the two periods (being more extensive in the earlier period), this may reflect also a decline of the profits effects in the U.S., but a less strong one than in France.

Having established that the error-correcting accelerator gives us similar indications as the traditional accelerator when we consider the comparable usual estimates (within versus first differenced), we now turn to the newer GMM estimates. These are given in Table 6 for two different sets of instruments. Table 6 also give two types of tests for the validity of these instruments: the usual SARGAN test for overidentifying restrictions and the LAGRANGE Multiplier tests for serial correlation (of first, second and third order) in the residuals, while Tables 7(a), 7(b) and 7(c) present different

37. Note however that we would expect values closer to one on the basis of the derivation of the long run investment equation from a COBB-DOUGLAS or CES production function with plausible ranges of values for the returns to scale and the elasticity of substitution parameters (see Appendix A).

TABLE 6

Error Correction Model for I/K
GMM Estimates (First Differences Instrumented by Levels)

	FIRST INSTRUMENTS SET **Instruments :** **I/*K* (*t* – 3 to *t* – 6), *Ds* (*t* – 3 to *t* – 6), Π/*K* (*t* – 3 to *t* – 6)** **Predetermined Variables:** **None**				**SECOND INSTRUMENTS SET** **Instruments :** **I/*K* (*t* – 3 to *t* – 6), *Ds* (*t* – 3 to *t* – 6), Π/*K* (*t* – 3 to *t* – 6)** **Predetermined Variables:** ***D* (*k* – *s*) (*t* – 2), *D* (*s*) (*t* – 2), *D* (Π/*K*) (*t* – 2)**			
	France		**U.S.**		**France**		**U.S.**	
	1971-79	1985-93	1971-79	1985-93	1971-79	1985-93	1971-79	1985-93
# observations	3969	4374	3663	4338	3969	4374	3663	4338
# firms	441	486	407	482	441	486	407	482
# instruments	99	117	99	117	102	120	102	120
I/*K* (*t* – 1)	–0.130 (.072)	–0.205 (.106)	0.068 (.112)	–0.255 (.101)	–0.166 (.073)	–0.064 (.055)	0.006 (.103)	–0.099 (.064)
Ds (*t*)	–0.034 (.066)	0.177 (.086)	0.094 (.053)	0.163 (.056)	–0.058 (.058)	0.162 (.080)	0.052 (.046)	0.190 (.053)
Ds (*t* – 1)	0.053 (.065)	0.041 (.111)	0.008 (.060)	0.165 (.064)	0.058 (.061)	0.038 (.096)	0.046 (.046)	0.173 (.048)
k–*s* (*t* – 2)	–0.353 (.109)	–0.210 (.058)	–0.372 (.109)	–0.245 (.058)	–0.351 (.077)	–0.193 (.049)	–0.257 (.088)	–0.241 (.057)
s (*t* – 2)	–0.152 (.065)	–0.150 (.078)	–0.116 (.060)	–0.026 (.039)	–0.135 (.056)	–0.147 (.063)	–0.071 (.048)	–0.041 (.030)
Π/*K* (t)	0.093 (.066)	–0.197 (.079)	0.059 (.064)	–0.114 (.074)	0.081 (.064)	–0.197 (.075)	0.086 (.062)	–0.138 (.073)
Π/*K* (*t* – 1)	0.101 (.049)	0.046 (.068)	0.154 (.048)	0.058 (.063)	0.032 (.036)	0.094 (.045)	0.053 (.039)	0.080 (.045)
Π/*K* (*t* – 2)	–0.027 (.027)	0.055 (.058)	–0.084 (.036)	0.038 (.040)	0.029 (.018)	0.020 (.022)	–0.005 (.020)	–0.011 (.022)
Long Run Sales	0.569 (.189)	0.286 (.514)	0.687 (.140)	0.895 (.153)	0.616 (.164)	0.241 (.451)	0.722 (.157)	0.829 (.115)
Long Run Π	0.471 (.282)	–0.456 (.249)	0.349 (.204)	–0.075 (.396)	0.406 (.184)	–0.430 (.186)	0.523 (.296)	–0.286 (.325)
Wald test for Sales (DF = 3)	6.796 (.079)	6.445 (.092)	10.511 (.015)	15.182 (.002)	14.115 (.003)	5.147 (.161)	7.692 (.053)	16.783 (.001)
Wald test for Π (DF = 3)	10.704 (.013)	6.863 (.076)	14.400 (.002)	4.252 (.235)	8.977 (.030)	7.085 (.069)	11.157 (.011)	9.898 (.019)
Wald test for lag 2 (DF = 3)	6.788 (.079)	1.865 (.601)	31.433 (.000)	3.493 (.322)	27.549 (.000)	39.293 (.000)	51.758 (.000)	40.624 (.000)
Sargan test (*p*–value)	105.958 (.039)	116.556 (.123)	92.462 (.202)	99.155 (.505)	108.968 (.041)	116.132 (.178)	99.218 (.139)	118.936 (.135)
LM1 test : m(1) (*p*–value)	–4.287 (.000)	–3.179 (.001)	–4.272 (.000)	–3.053 (.002)	–4.091 (.000)	–7.581 (.000)	–3.812 (.000)	–7.690 (.000)
LM2 test : m(2) (*p*–value)	–2.913 (.004)	–1.224 (.221)	–0.387 (.699)	–1.915 (.056)	–4.787 (.000)	–0.036 (.971)	–3.739 (.000)	–0.372 (.710)
LM3 test : m(3) (p–value)	0.455 (.649)	–1.825 (.068)	–0.044 (.965)	–1.038 (.299)	0.583 (.560)	–1.355 (.175)	–0.370 (.711)	–0.308 (.758)

All equations include time dummies but not industry dummies. Heteroskedastic-consistent standard errors are shown. The GMM estimates reported are one-step estimates. See also footnote 38.

characterisations of their relevance. [38] Since the comparative advantage of the GMM estimators crucially depends on both the valididy and relevance of the instruments, we pay special attention to these various statistics before looking at the estimates themselves.

Our choice of two sets of instruments was made after a careful consideration of several possibilities and a number of experimentations, including the use of levels and/or differenced information and keeping different variables with shorter or longer lags. We decided to focus here on these two sets since they appear to be the most acceptable and illustrate well the problems encountered. Our first set of instruments consists of the lagged values of the right hand side variables I/K, Δs, and Π/K , from lagged $(t-3)$ through $(t-6)$, and the year dummies. Because we have in fact respectively 12 and 15 years of data (1968 to 1979, and 1979 to 1973) available for our two 9 year study periods, we can use 9 estimating first-differenced year equations in each period, and the number of orthogonality conditions for this first set of instruments are of 99 and 117 for the first and second period respectively. [39] For our second set of instruments, we also assume that our three main variables $(k - \mathrm{s})$, s and (Π/K) lagged $(t-2)$ in the initial level form model are predetermined, that is we add to the first set of instruments the three instruments $(\Delta(k-\mathrm{s})$, $\Delta\mathrm{s}$ and $\Delta(\Pi/K))$ lagged $(t-2)$. Note that this amounts to using in practice the three variables I/K, Δs, and Π/K, from lagged $(t-2)$ through $(t-6)$ as instruments (instead of from lagged $(t-3)$ through $(t-6)$ as in the first set of instruments). [40] Note also that this amounts to adding only 3 orthogonality conditions and thus having 102 and 120 of them in total for the first and second periods respectively. [41]

Our two sets of instruments pass the SARGAN tests for both the earlier and recent U.S. samples and the recent French sample, and appear to be just barely rejected at the 5% conventional significance level for the earlier French sample (with p-values of 4%). The tests of serial correlation accept the absence of serial correlation in the differenced error term $\Delta\varepsilon_{it}$ at the third order but clearly reject it at the second order for the earlier French and U.S. samples, tending to indicate that the error term ε_{it} of the model (in level form) is a moving average of order one (and not white noise). The test thus confirms the validity of using the $(t-3)$ lags of the variables as part of the first set of intruments for all samples, but not that of using the $(t-2)$ lags as part of the second set of instruments for the earlier samples. Therefore it is

38. All our estimations have been made using TSP 4.4. Our GMM estimates are the same than those obtained with ARELLANO-BOND DPD software. They are the first-step estimates computed with a covariance matrix of errors coresponding to white noise errors ε_{it} in levels (and to an MA(1) process in the differenced errors $\Delta\varepsilon_{it}$). The SARGAN tests are computed using the second-step (optimal) estimates, while the LM tests of autocorrelation are based on the first-step differenced residuals $\widehat{\Delta\varepsilon_{it}}$.

39. For the second period 1985-1993, we have 117 = 9 (year dummies) + 9 (equations) by 4 (lagged instruments) by 3 (variables). For the first period 1971-1979, by comparison to the second one, we lose 18 instruments, that is 6 per variable which correspond to the three missing years 1965, 1966 and 1967 for the year equation 1971, the two missing years 1966 and 1967 for the year equation 1972, and the one missing year 1967 for the year equation 1973.

40. This is practically the case, but not strictly, because I/K is an approximation of Δk.

41. Since our three additional instruments are variables in the first differenced equation, it will not be right to count that we have three additional orthogonality conditions for each of the 9 first differenced year equations (that is 27 instead of 3).

TABLE 7 (A)

Error Correction Model for I/K
Validity of Instruments in GMM Estimation : R-squared and F-tests

	FIRST INSTRUMENTS SET **Instruments :** **I/*K* (*t* – 3 to *t* – 6), *Ds* (*t* – 3 to *t* – 6), Π/*K* (*t* – 3 to *t* – 6)** **Predetermined Variables:** **None**								**SECOND INSTRUMENTS SET** **Instruments :** **I/*K* (*t* – 3 to *t* – 6), *Ds* (*t* – 3 to *t* – 6), Π/*K* (*t* – 3 to *t* – 6)** **Predetermined Variables:** ***D* (*k* – *s*) (*t* – 2), *D* (*s*) (*t* – 2), *D* (Π/*K*) (*t* – 2)**							
	France				**U.S.**				**France**				**U.S.**			
	1971-79		1985-93		1971-79		1985-93		1971-79		1985-93		1971-79		1985-93	
# observations	3969		4374		3663		4338		3969		4374		3663		4338	
# firms	441		486		407		482		441		486		407		482	
# instruments	99		117		99		117		102		120		102		120	
	R^2	**F-test**	**R^2**	**F-test**	**R^2**	**F-test**	**R^2**	**F-test**	**R^2**	**F-test**	**R^2**	**F-test**	**R^2**	**F-test**	**R^2**	**F-test**
I/*K* (*t* –1)	0.094	4.090	0.069	2.713	0.130	5.427	0.061	2.358	0.228	11.283	0.376	21.532	0.251	11.844	0.399	23.561
Ds (*t*)	0.149	6.927	0.046	1.783	0.279	14.080	0.120	4.948	0.152	6.867	0.048	1.817	0.300	15.111	0.139	5.721
Ds (*t* – 1)	0.147	6.826	0.044	1.691	0.308	16.154	0.163	7.104	0.608	59.456	0.464	30.999	0.603	53.623	0.533	40.514
k-*s* (*t* –2)	0.162	7.636	0.090	3.610	0.272	13.587	0.161	6.995	1.000		1.000		1.000		1.000	
s (*t* –2)	0.208	10.346	0.082	3.293	0.299	15.517	0.198	8.964	1.000		1.000		1.000		1.000	
Π/*K* (*t*)	0.128	5.799	0.083	3.316	0.168	7.363	0.087	3.472	0.175	8.110	0.104	4.146	0.219	9.887	0.099	3.903
Π/*K* (*t* –1)	0.186	9.045	0.118	4.901	0.284	14.441	0.112	4.603	0.269	14.105	0.204	9.173	0.344	18.455	0.229	10.545
Π/*K* (*t* –2)	0.322	18.747	0.202	9.291	0.412	25.490	0.223	10.434	1.000		1.000		1.000		1.000	

Value of F distribution : for *p*-value = 0.05 : F (100, 4000) = 1.28 ; for *p*-value = 0.01 : F (100, 4000) = 1.43 ; for *p*-value = 0.001 : F (100, 4000) = 1.62.

TABLE 7 (B)

Error Correction Model for I/K

Validity of Instruments in GMM Estimation : Partial R-Squared and F-Tests

	FIRST INSTRUMENTS SET **Instruments :** **I/*K* (*t* – 3 to *t* – 6), *Ds* (*t* – 3 to *t* – 6), Π/*K* (*t* – 3 to *t* – 6)** **Predetermined Variables:** **None**								**SECOND INSTRUMENTS SET** **Instruments :** **I/*K* (*t* – 3 to *t* – 6), *Ds* (*t* – 3 to *t* – 6), Π/*K* (*t* – 3 to *t* – 6)** **Predetermined Variables:** ***D* (*k* – *s*) (*t* – 2), *D* (*s*) (*t* – 2), *D* (Π/*K*) (*t* – 2)**							
	France				**U.S.**				**France**				**U.S.**			
	1971-79		1985-93		1971-79		1985-93		1971-79		1985-93		1971-79		1985-93	
# observations	3969		4374		3663		4338		3969		4374		3663		4338	
# firms	441		486		407		482		441		486		407		482	
# instruments	99		117		99		117		102		120		102		120	
	R^2	**F-test**	**R^2**	**F-test**	**R^2**	**F-test**	**R^2**	**F-test**	**R^2**	**F-test**	**R^2**	**F-test**	**R^2**	**F-test**	**R^2**	**F-test**
Π/*K* (*t* – 1)	0.060	2.535	0.045	1.721	0.041	1.573	0.048	1.830	0.064	2.598	0.065	2.501	0.045	1.655	0.082	3.149
Ds (*t*)	0.047	1.949	*0.027*	*1.037*	0.082	3.261	0.043	1.626	0.056	2.267	*0.029*	*1.070*	0.090	3.482	0.047	1.750
Ds (*t* – 1)	0.061	2.584	*0.028*	*1.064*	0.096	3.866	0.058	2.234	0.079	3.281	*0.033*	*1.203*	0.108	4.261	0.068	2.595
k–*s* (*t* – 2)	0.078	3.337	0.107	4.394	0.099	3.990	0.139	5.890	0.282	15.028	0.138	5.700	0.208	9.261	0.155	6.499
s (*t* – 2)	0.090	3.929	0.095	3.839	0.117	4.803	0.160	6.926	0.111	4.786	0.119	4.848	0.179	7.700	0.207	9.259
Π/*K* (*t*)	0.078	3.320	0.054	2.078	0.074	2.895	0.057	2.207	0.100	4.232	0.063	2.421	0.080	3.069	0.062	2.361
Π/*K* (*t* – 1)	0.135	6.143	0.057	2.232	0.115	4.747	0.073	2.857	0.182	8.505	0.119	4.840	0.160	6.741	0.106	4.184
Π/*K* (*t* – 2)	0.230	11.817	0.112	4.636	0.199	9.017	0.159	6.903	0.641	68.388	0.769	118.838	0.591	50.981	0.553	43.859
YEAR 1972–1986	0.865	252.951	0.842	194.924	0.625	60.711	0.541	42.890	0.954	800.495	0.909	357.722	0.755	108.905	0.560	45.168
YEAR 1973–1987	0.771	133.001	0.852	211.869	0.542	42.994	0.567	47.668	0.946	669.752	0.973	1288.513	0.570	46.798	0.600	53.112
YEAR 1974–1988	0.747	116.517	0.886	283.826	0.560	46.349	0.649	67.260	0.928	491.917	0.909	357.483	0.584	49.563	0.662	69.297
YEAR 1975–1989	0.449	32.157	0.807	153.030	0.606	55.872	0.790	136.586	0.481	35.461	0.828	171.703	0.627	59.259	0.843	189.763
YEAR 1976–1990	0.419	28.432	0.766	120.271	0.544	43.385	0.705	86.767	0.487	36.312	0.819	161.997	0.597	52.179	0.733	97.275
YEAR 1977–1991	0.366	22.750	0.686	80.002	0.451	29.933	0.618	58.952	0.742	110.288	0.772	121.057	0.689	78.215	0.663	69.766
YEAR 1978–1992	0.554	49.039	0.757	114.366	0.704	86.355	0.457	30.676	0.654	72.434	0.816	158.563	0.740	100.234	0.506	36.313
YEAR 1979–1993	0.480	36.495	0.582	51.124	0.675	75.414	0.633	62.688	0.727	102.081	0.627	60.049	0.702	83.219	0.747	104.506

Value of F distribution : for *p*-value = 0.05 : F(100,4000) = 1.28 ; for *p*-value = 0.01 : F(100,4000) = 1.43 ; for *p*-value = 0.001 : F(100,4000) = 1.62 . Not significant at 1 % level in italics.

TABLE 7 (C)

Error Correction Model for I/K
Validity of Instruments in GMM Estimation: Canonical Correlations

	FIRST INSTRUMENTS SET **Instruments :** **I/*K* (*t* – 3 to *t* – 6), *Ds* (*t* – 3 to *t* – 6), Π/*K* (*t* – 3 to *t* – 6)** **Predetermined Variables:** **None**								**SECOND INSTRUMENTS SET** **Instruments :** **I/*K* (*t* – 3 to *t* – 6), *Ds* (*t* – 3 to *t* – 6), Π/*K* (*t* – 3 to *t* – 6)** **Predetermined Variables:** ***D* (*k* – *s*) (*t* – 2), *D* (*s*) (*t* – 2), *D* (Π/*K*) (*t* – 2)**							
	France				**U.S.**				**France**				**U.S.**			
	1971-79		1985-93		1971-79		1985-93		1971-79		1985-93		1971-79		1985-93	
# observations	3969		4374		3663		4338		3969		4374		3663		4338	
# firms	441		486		407		482		441		486		407		482	
# instruments	99		117		99		117		102		120		102		120	
	Corr.	***P*-value**	**Corr.**	***P*-value**	**Corr.**	**P-value**	**Corr.**	***P*-value**	**Corr.**	***P*-value**	**Corr.**	***P*-value**	**Corr.**	***P*-value**	**Corr.**	***P*-value**
Correlation #1	0.037	(.000)	*0.023*	*(.455)*	0.038	(.000)	0.036	(.000)	0.046	(.000)	*0.024*	*(.409)*	0.043	(.000)	0.040	(.000)
Correlation #2	0.053	(.000)	*0.026*	*(.221)*	0.052	(.000)	0.039	(.000)	0.056	(.000)	*0.030*	*(.089)*	0.058	(.000)	0.058	(.000)
Correlation #3	0.057	(.000)	0.033	(.012)	0.076	(.000)	0.051	(.000)	0.078	(.000)	0.051	(.000)	0.082	(.000)	0.070	(.000)
Correlation #4	0.077	(.000)	0.039	(.000)	0.089	(.000)	0.062	(.000)	0.094	(.000)	0.076	(.000)	0.100	(.000)	0.083	(.000)
Correlation #5	0.089	(.000)	0.050	(.000)	0.111	(.000)	0.074	(.000)	0.286	(.000)	0.254	(.000)	0.362	(.000)	0.209	(.000)
Correlation #6	0.104	(.000)	0.062	(.000)	0.137	(.000)	0.095	(.000)	1.000		1.000		1.000		1.000	
Correlation #7	0.153	(.000)	0.212	(.000)	0.181	(.000)	0.146	(.000)	1.000		1.000		1.000		1.000	
Correlation #8	0.484	(.000)	0.448	(.000)	0.602	(.000)	0.495	(.000)	1.000		1.000		1.000		1.000	

Not significant at 1% level in italics.

probably wise to view our estimates for these earlier samples with somewhat less confidence than those for the more recent ones. The relevance statistics shown in Tables 7(a), (b) and (c), however, bring out some other nuances.

The *R*-squares of the projections of the first-differenced variables of our model on the two sets of instruments in Table 7(a) are generally low, but the F-tests remain quite significant at conventional statistical levels. However, if we follow NELSON and STARTZ [1990], who advocate the use of a value of about 2.0 for these *F*-test (corresponding to a very small significance level when the number of instruments is 100), we see that there are no acceptable instruments for the contemporaneous sales growth for the recent French sample. The partial *R*-squares of the different instrumented variables on the instruments which take into account the collinearity between instruments and the corresponding *F*-tests given in Table 7(b) show that the weak instrument problem clearly concerns both the lagged and contemporaneous sales growth variables for the recent French sample. To a lesser degree it seems also to affect the lagged investment variable for all our four samples in the case of the first set of instruments, and only for the earlier U.S. sample in the case of the second set of instruments. The inspection of the canonical correlations between the set of instrumented variables and our two sets of instruments given in Table 7(c) largely conveys the same diagnostic. Even though most of these correlations are quite small, they appear statistically very significant with the only exception being the recent French sample. For this sample, there is no relevant instrument among both sets of instruments for two of the instrumented variables (*i.e.*, the two sales growth variables as evidenced by the partial *R*-squares).

Turning now to the GMM estimates in Table 6, we see that they are practically the same for both sets of instruments, with somewhat smaller standard errors for the second set as expected. In both cases, we find, however, that they are strikingly less precise than the corresponding within estimates from Table 5, and particularly so as concerns the long run effects and the recent French sample. If we abstract from such imprecision in comparing the point estimates, we also find no major differences for most coefficients. Since the GMM estimates purport to correct for simultaneity and measurement error biases which possibly affect the within estimates, this can be taken as modest evidence that these kind of biases may not be too important in our context. The main difference between our GMM and within estimates affects the long run profits effects in the recent period in the U.S.; they seem to have disappeared or even become negative as they do in France (where this is shown by the two types of estimates). The current coefficient of profits is now sizeably negative for both the recent U.S. and French samples, and this is not entirely compensated by the two lagged ($t-1$) and ($t-2$) positive coefficients.[42] There is no such difference in the estimates of the long run sales effects, which remain roughly the same. The fall of the GMM estimates for France that can be seen in the recent period is not significant, these estimates being particularly imprecise due to the lack of relevant instruments for sales growth which we have noted.

42. Note that this is what one might expect if one is willing to view the error correction specification as also providing an approximation of the EULER equation in the absence of financial constraints on firms.

To summarize, both the within and GMM estimates convey the view that whatever profit effects might have existed twenty years earlier, these effects are greatly reduced in the late 1980s and early 1990s, in France and to a lesser degree perhaps in the U.S., and at least for the large and fairly long-lived manufacturing firms of our samples. This is consistent with the profound deregulation of financial markets during the eighties in both countries, and especially in France.

8 Conclusions

> *"Business investment is ideally decided on the basis of anticipations of the future... If our implicitly and explicitly assumed relations between past and future prove different from those of business decisionmakers, we can hardly expect to estimate a stable or reliable relation between business investment and past or current variables."*
>
> *Quoted from EISNER [1978 b, in pages 12 and 13.]*

Our motivation for this study was to assess the effect of changes in modelling and estimating investment equations at the firm level during the past twenty years, focusing on the implications of improvements in panel data econometrics. Although we gave a brief overview of the evolution of the theoretical modeling of investment at the firm level from the traditional Jorgensonian approach through TOBIN's q theory to the modern EULER equation specification, we have chosen to concentrate on a fairly robust error correction specification of the accelerator-profit model.

We thus began with the traditional accelerator-profit specification of the investment equation estimated with classical least-squares methods in the total, within- or between-firm dimensions of the data, using methods that were relatively new at the time of the first Conference on Panel Data Econometrics in 1977. We compared this econometric specification to one that adds an error-correction mechanism. We showed that this new specification is both parsimonious and helps to disentangle the long-run from the short-run behavior of investment in a theoretically consistent way and thus preferable to the less precise intuition that cross sectional (between) estimates represent the long run and time series (within) estimates the short run. We also stressed that firm-specific effects have a different meaning in the two specifications: for the traditional accelerator, they imply heterogeneous growth rates for the capital stock, whereas for the error-correction specification, they imply heterogeneous capital-output ratios.

We then investigated the use of the more modern GMM estimation methods. GMM should correct for biases coming from the endogeneity of variables and random errors in variables, as well as for the biases arising from the presence of correlated firm-specific effects. But we found that the estimation results are not statistically different when we use GMM instead of within firm estimation, the potential gain of GMM being offset by a large imprecision in the estimated parameters. This is a clear instance of the "*weak*" instruments problem. Relying only for instruments on the lagged values of the variables in the model does poorly for us. If we want to obtain precise enough estimates and are confined to the use of internal instruments, we would clearly need much larger samples than the present ones. But it would be much better of course to find good external instruments.

Finally what can we conclude on the changes in the economic determinants of the investment behavior of the French and U.S. firms in twenty years? Our primary finding is that the profit or cash-flow rate no longer enters the firm-level investment equation in either France or the U.S., once we control for biases from correlated firm effects as well from random measurement error and simultaneity. Although our GMM estimates are very imprecisely determined, we find for the recent 1985-1993 period no role of profits in the long run and little (and negative) in the short run especially in France but also in the U.S.; this result contrasts with our estimates for the 1971-1979 earlier period and may reflect the deregulation of financial markets of the eighties in the two countries.

Perhaps the most disappointing feature of our investigation here is the low precision we obtain when using the newer GMM methods of estimation that are intended to correct for simultaneity, measurement error and firm effects biases. However, we do not think that our conclusion should be too pessimistic. Even if the important econometric advances of the past twenty years have been far from being as successful as we had hoped for, we do find significant improvement in the specification, estimation and interpretation of firm investment equations and more generally we have reached a better comprehension of what can and cannot be measured. Firm investment equations are most difficult relations to estimate empirically and it should not surprise us that progress is slow. Indeed, as we are aptly reminded by the above citation of EISNER, the major determinant of firm investment behavior and decisions is their anticipations of the future (Keynes "*animal spirits*"), and trying to capture them in an econometric model will always remain a formidable challenge.

We see several ways in which we hope to make ourselves some progress within the framework we have outlined here, and we intend to pursue them in future work. As a close to this paper, let us indicate what they are. First, recent work by ARELLANO and BOVER [1995] and BLUNDELL and BOND [1998, 1999] has suggested that if equation (7) is the true model, it should also be possible to instrument it using level information (*i.e.*, using as instruments on the untransformed equation lagged differences of the *x*'s and *y*'s variables, since these presumably do not contain firm effects). Although our initial attempts have not been satisfactory, we plan to pursue in this direction. More generally, there is still much to be learned on how to use in practise GMM methods in panel data econometrics.

Second, our samples of manufacturing firms are typically heterogeneous and thus far we have forced them all into a "*one size fits all*" investment model; it seems more plausible to expect that firms adjust their capital stock at different rates and in response to the different shocks. Recent work by PESARAN, SHIN, and SMITH [1999] suggests a reasonable generalization to allow for such heterogeneity across firms in our model. They propose estimating a model of long-run relationships in heterogeneous dynamic panels by specifying a cointegrating relationship that is the same for all units, but allowing the short-run adjustment dynamics to vary across them. Preliminary exploration using this model in our data yielded quite plausible results with a long-run sales coefficient of nearly unity (when the data are in logarithms) and a range of short-run coefficients which were quite reasonable.

Finally, BOND, HARHOFF, and VAN REENEN [1999] present evidence that the investment behavior of R&D-performing and non-R&D-performing firms differs in the United Kingdom and Germany. We plan to investigate both whether this fact is also true in France and the United States, and whether R&D investment itself displays behavior similar to that described for physical investment in this paper.

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APPENDIX A

Derivation of the Model

The firm solves the problem:

$$\text{Max} \sum (\varpi)^t [p_t f(K_t) - p_t^I I_t] \quad s.t. \quad K_t = (1-\delta)K_{t-1} + I_t.$$

When the prices of output and capital p_t and p_t^I are constant over time, this yields the steady state solution

$$f'(K_t) = \frac{p^I}{p}\left(\frac{r+\delta}{1+r}\right),$$

where δ is the depreciation rate and r is the interest rate implicit in $\varpi = (1+r)^{-1}$. When prices are allowed to vary over time, the solution is

$$f'(K_t) = \frac{p_t^I}{p_t}\left(\frac{r+\delta}{1+r} - \frac{\Delta p_{t+1}^I}{p_t^I}\frac{1-\delta}{1+r}\right) = C_t.$$

Thus there is an additional term in the relative price of investment that comes from the capital gain or loss on the existing capital. In panel data estimation, this will imply that year effects belong in the equation, regardless of whether real or nominal values of capital and output are used.

If the production function is COBB-DOUGLAS $S_t = f(L_t, K_t) = A_t L_t^\beta K_t^\alpha$, we obtain:

$$K_t = \alpha \frac{S_t}{C_t}$$

or in logarithmic form:

$$k_t = s_t + h_t \quad \text{where} \quad h_t = \log(\alpha) - c_t$$

More generally for a CES production function where σ and ν are respectively the elasticities of substitution and scale:

$$f(L_t, K_t) = A_t \left[\beta L_t^{\frac{\sigma-1}{\sigma}} + \alpha K_t^{\frac{\sigma-1}{\sigma}}\right]^{\left(\frac{\sigma}{1-\sigma}\right)\nu},$$

we obtain:

$$k_t = \theta s_t + h_t$$

where $\theta = \left(\sigma + \frac{1-\sigma}{\nu}\right)$ and $h_t = \sigma \log(\alpha\nu) - \frac{1-\sigma}{\nu} \log A_t - \sigma c_t$.

Note that the elasticity of capital to sales is unity ($\theta = 1$) if the production function has is constant returns to scale ($\nu = 1$) or if its elasticity of substitution is unity ($\sigma = 1$), *i.e.*, if it is COBB-DOUGLAS.

APPENDIX B

Description of the Data

B.1. Old Samples

The old samples for the period 1968-1979 for France and the U.S. are very similar to the new ones (see below). They have been used in MAIRESSE [1988 and 1990] and described in some details there. The investment, sales and operating income variables have comparable definitions (but unfortunately the cash flow variable has not been recorded). The capital stock variable has been recomputed in the same manner as for the new samples, based on the permanent inventory method with a 8% depreciation rate.

B.2. New Samples

The data for the U.S. firms are drawn from the 1995 Standard & Poor edition of Compustat (the active and research files for Annual Industrial and Full Coverage firms were merged using both the current 1976-1995 files and the historical 1957-1976 files). Firms that are incorporated in a foreign country and wholly-owned subsidiaries were deleted. All firms are publicly-traded on U.S. stock markets. For more details see HALL [1990].

The data for French firms are drawn from the SUSE datafiles of INSEE, which give the balance sheets and income accounts of all firms with more than 20 employees in all industries since 1978. These are collected from the fiscal statements of the firms concerned (BIC) and their answers to the firm annual surveys (EAE). Most of the firms are not publicly-traded.

Our samples are balanced samples restricted to the Manufacturing firms which were present for the whole period 1979-1993 and for which all the necessary information on the variables of our analysis was available in all fifteen years, after some preliminary cleaning of outliers. Basically we have trimmed our key ratio variables (I/K, K/S, Δs, CF/K and OPINC/K), so that one percent of the observations in the tails of each of them were removed.

The firm variables we use in our analysis are defined in the following way:

- S is the total sales (or turnover) deflated by production price indices at a comparable two-digit level in the two countries (expressed in 1985 millions of dollars or French francs: M$ or MF).
- E is the average number of employees during the year in thousands for US and at the end of the year for France.
- I is the investment in fixed assets deflated by an overall price index for investment in Manufacturing industries (expressed in 1985 millions of dollars or French francs: M$ or MF).
- CF is the after-tax cash-flow (net profit plus depreciation allowances), deflated as sales (expressed in 1985 millions of dollars or French francs:

M$ or MF). It is also equal to the operating income variable (OPINC), plus net financial profits, plus net extraordinary items, minus profit taxes.

- K is the net capital stock at the end of the year (expressed in 1985 millions of dollars or French francs: M$ or MF). It is computed by the so-called permanent inventory method with a constant rate of depreciation δ where $K_t = (1\text{–}\delta)K_{t-1} + I_t$. We have adopted an average value of 8% for δ and used as the benchmark value for the capital stock K_{t_0} in the first year the net book value of the firm fixed assets after some adjustment for historical inflation (based on an estimate of their average age).