ACTA UNIVERSITATIS UPSALIENSIS
Studia Oeconomica Upsaliensia 21

Thomas Lindh

Essays on Expectations in Economic Theory

UPPSALA 1992

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Distributor:
Almqvist & Wiksell International
Stockholm, Sweden

Printed edition of doctoral dissertation presented to the Faculty of Social Sciences at Uppsala University 1991

Abstract

Lindh, T. 1992, Essays on Expectations in Economic Theory. Acta Univ. Ups., *Studia Oeconomica Upsaliensia* 21. x + 112 pp. Uppsala. ISBN 91-554-2850-9.

This thesis consists of three essays with one common theme; the fundamental role of implicit and explicit assumptions about expectations and beliefs of individuals in economic theory. The viewpoint is however not methodological. Rather the thesis has the form of three case studies.

In the first essay so called "consistent conjectures" in oligopoly markets of various forms are scrutinized. The traditional conjectural variation models have been criticized because firms are "right for the wrong reasons". Several papers have attempted to meet this difficulty, as well as the indeterminacy of the models, by some condition of consistency or rationality imposed on what firms may believe about each other. Stronger versions of such conditions lead into paradoxes, while the weaker forms are of little use in solving the perceived problems. The problems with consistent conjectures illuminate fundamental problems with too demanding information assumptions like perfect foresight. Such assumptions may create non-trivial and non-obvious structures of self-reference.

The second essay reviews a growing literature investigating how economic agents may learn rational expectations. Fully rational learning requires implausible initial information assumptions, therefore some form of bounded rationality has come into focus and learning stability as a correspondence principle to differentiate among multiple equilibria show some promise in common macro models. But a new selection problem arises since different initial information and learning methods may give rise to different stable equilibria, making the sensitivity of economic modelling to assumptions on information and information processing more clearly visible. Another problem is that many convergence results rely on prior coordination of learning in some form or another.

In the third essay Leif Johansen's short run macro production function is used to explore the conditions for a productivity slowdown to take place simultaneously with an accelerated technical change in production. The mechanism emphasizes the decisive role of past investment and future price expectations in shaping the relation between technical change and productivity growth. Productivity slowdown is considerably more likely to take place when capital equipment on the extensive margin was installed under conditions of depressed investment, and would be even more likely if investors extrapolate preceding price trends to form expectations.

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ISBN 91-554-2850-9
ISSN 0585-539-X

gotab 94526 Stockholm 1992

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Preface

Research in economics is often lonely work and I have sometimes felt that my choice of subjects to study may have made me even lonelier. All the more important has it been to have the cheerful and encouraging atmosphere at the Department of Economics at Uppsala University around me. I have really appreciated working in these surroundings and can only hope that I shall be able to repay something for the kindness and helpful assistance I have had the privilege to receive here.

The work leading to the first essay started already in an undergraduate paper written under the supervision of Yngve Andersson and Olle Mellander. I am very grateful for their many valuable comments and above all for their encouragement at critical stages. With time and the very constructive help from my thesis advisor at that time, Bengt-Christer Ysander, the undergraduate paper grew into a licentiate thesis in Swedish. The comments and suggestions from the discussant at the licentiate seminar Karl-Göran Mäler at Stockholm School of economics are gratefully acknowledged. Drawing on this and supported by Bengt-Christers enthusiasm and deep insights I eventually was able to condense and complete the first essay. The bulk of that work took place during a very pleasant two months in a room with a magnificent view on the top floor of Uppsala Castle at the Swedish Collegium for Advanced Studies in the Social Sciences (SCASSS), the financial support of which I gratefully acknowledge. I also wish to thank participants to seminars at Uppsala University and at the Industrial Institute for Economic and Social Research (IUI) in Stockholm for valuable comments.

Many improvements of the first essay I owe to several anonymous referees, finally resulting in the paper being published and forthcoming in the Journal of Economic Behavior and Organization. I thank the Elsevier Publishing Co. for their cooperation in granting permission to reprint "The inconsistency of consistent conjectures" in this thesis.

The second essay grew out of a hastily written comment on the literature on rational expectations learning presented at the IUI conference "Markets for innovation, ownership and control" in June, 1988. It was a last minute "better than nothing" substitution for a presentation by a very much more competent researcher, Margaret Bray, who was unexpectedly prevented from attending. Without the support and interest of Gunnar Eliasson at IUI nothing more would have come out of that and this overview would not have been written. I am now very grateful to him for insisting that I should pursue this

work. My current thesis advisor, Peter Englund, and seminar participants at Uppsala University deserve my gratitude for reading and commenting on earlier versions of this essay. The character of this paper, with very condensed summaries of technically complicated research, I suspect, makes it rather dull to read, at least to those not familiar with the literature on rational expectations learning. I would also like to thank Claes Wihlborg, Seppo Honkapohja, Richard H. Day, Kenneth Burdett and three anonymous referees for comments and suggestions that have been very helpful to me in extending the scope of the overview.

The third essay is different from the others not only because expectations have a more subordinate place in it, but also because it is intended as a part of a more comprehensive research project. This project aims at an empirical evaluation of the importance of the interaction of expectations of the future and the heritage from the past with productivity growth.

In the work on this project I owe much to the encouragement and patience of both my thesis advisors Bengt-Christer Ysander and Peter Englund. Professor emeritus R. Bentzel and the growth seminar at the department have greatly contributed by reading and commenting on several less successful attempts to analyze the problems of non-balanced growth. Gunnar Eliasson has given me opportunities to present and discuss my ideas at seminars at IUI for which I am very grateful. I also owe a debt to to Ernst R. Berndt, Kenneth Burdett and Jonas Agell for sympathetic listening and valuable advice in the early stages of this study even though I have in this specific essay followed other directions than those recommended by these distinguished researchers. The inspiration to use Johansen's short run macro production function came from a seminar on a paper by A. B. Pomansky and G. Y. Trofimov at CEMI, Moscow, from which I also received good advice on how to proceed. Annika Alexius' excellent and quick translation from Russian to Swedish of a central paper on structural change in production was of great help.

The final form of this paper has benefitted much from comments by Lawrence J. Lau and Stefan Lundgren, especially the latters detailed suggestions about how to simplify the derivation of results were invaluable in simplifying and focussing the exposition. Peter Englunds repeated reading and detailed suggestions have also greatly contributed to a hopefully more readable essay. I would also like to thank Bengt Hansson for taking time from his own thesis work to discuss and share with me his experience of the empirical aspects of productivity growth.

My thanks also are due to professor Geir Asheim, who acted as faculty discussant during my dissertation and helped clarifying the crucial points of my work.

The helpful and non-bureaucratic assistance of the administrative staff at the department has contributed to my work in a very essential way. This kind of assistance is easily undervalued and overlooked so I want to emphasize the great value I attach to the fact that administrative tasks associated with teaching duties, seminars, courses etc. always have run smoothly without any unnecessary problems. I attribute this very much to the flexibility and cooperation of Eva Holst, Monica Ekström, Berit Levin and Gertrud Suo, all of which I wish to thank. Especially Eva Holst, for helping out an absent-minded graduate student with reminders and helpful interventions at appropriate occasions.

My room mate Mats Dillén also deserves recognition for moral support and the time consumed in diverse discussions on for him peripheral subjects.

To all those, too numerous to mention here, whose help and encouragement have made me feel at ease and thereby aided me in the writing of this thesis I express my gratitude collectively.

Finally, without the understanding and support of my wife Ingalill I would never have begun university studies in the first place, not to mention graduate studies. Therefore my debt to her is the greatest. My children, Bjarne, Ambjörn, Sanna, Ylva Li and Joar have endured the hardships of a strained household economy and an absent-minded and sometimes irritable father with varying degrees of patience and understanding, undoubtedly correlated to their respective ages. I can only hope they will eventually understand the pleasure I get from study and research. That pleasure is a heritage I owe to my parents, Lilian and Olle, for bringing me up with a sense of wonder before the achievements of human search for knowledge. Although my father is no longer with us, I am sure that he would have found the way I spent the last decade far more approvable than the preceding decade. That my mother does so, I know. It therefore gives me great pleasure to dedicate this thesis to the two most important women in my life, my mother and my wife.

Uppsala in October 1991
Thomas Lindh

Chapter 1: Introduction and summary

1.1 Expectations in economic theory

The three essays in this thesis have one common theme, although the aspects and treatments of this theme are vastly different. That theme is the crucial role of expectations in economic models. In all economic models some assumptions about the beliefs of the agents are critical to the conclusions drawn from the model. Those assumptions may not be explicit and they very often amount to the assertion that agents have more or less perfect information about all factors essential to their decisions, hence that their beliefs consist of perfect knowledge.[1]

In economic terminology expectations in modern usage most often refer to beliefs that are more or less identical to the mathematical expected value of a stochastic variable, and when reference is made to more imprecise beliefs, other terms, as conjectures or anticipations, are substituted. When beliefs are certain knowledge about the future it is commonly referred to as perfect foresight. Here the term expectations will be used in the wider sense of any beliefs that agents have about their economic environment and its future development.

Expectations have always been a problem to economic theorists, because expectations are so obviously dependent upon experience. Tastes and preferences could also be argued to be dependent upon experience but still they are commonly taken to be given facts of nature, since *de gustibus non est disputandum.* This stance among economists, I strongly suspect, is due to a deeply felt reluctance to prescribe what tastes people ought to have given a certain experience. Psychologists and other social scientists may have opinions about that, but economists generally do not. We impose some regularity assumptions on preferences in order to keep problems tractable but otherwise regard tastes as exogenous to the economic system. But beliefs about the economic system and how it evolves are different since this is the subject of our research, and we therefore have more or less wellfounded opinions on what rational people should believe about this given their information. Thus there is a general dissatisfaction with taking expectations as given facts of nature, even

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[1] A list of references to distinguished economists writing on this subject could easily fill several pages. For those interested a starting point may be Frydman and Phelps(1983), which is an easily accessible collection of papers on expectations.

if tractability considerations often force us into such assumptions anyway.

There are essentially two easy ways that economists have dealt with this problem. One way is to assume static expectations, i.e. asserting that people believe that things will remain the same in the future. The other way is perfect foresight, asserting that whatever people believe it will be verified as correct after the decision. In theoretical models both these assumptions may be perfectly reasonable given the context of the model, and even, if properly formulated, amount to the same thing. In a standard growth model for example that is assumed to be on a balanced growth path, where all variables change at some constant rate, static expectations about the rates of change and perfect foresight are equivalent. And it seems rather reasonable to assume that agents by experience should have adjusted their beliefs in the right direction since otherwise they would on the steady state path tend to repeat the same mistakes over and over again, or alternatively they would not be on a steady state path at all. However, in growth models static expectations often is the term used in practice, when it is assumed that agents do repeat the same mistakes over and over again.

But to anyone interested in real world economic issues it is obvious both that agents do make mistakes, and they, often at least, try to learn from their mistakes in order not to repeat them. Moreover, information is never perfect, totally unexpected events do occur and many events are predictable only in a stochastic sense.

The Arrow-Debreu model of general equilibrium can accommodate uncertainty if there is a complete set of contingent markets. But, of course, the real world is not that perfect. Nor are agents in general price takers, they act strategically at least in some roles and hence have a need to foresee how other agents will react, thus introducing the guessing about the opinions of other agents that Keynes' famous beauty contest refers to.

The problems of strategic interaction have provided the impetus for a thriving research in game theory, which, alas, as the celebrated Folk theorem indicates, provides us with very few definite conclusions regarding what we should expect agents to expect about their rivals on competitive markets. The first essay of the thesis is a critical assessment of an approach to oligopoly theory, called consistent conjectural variations. In the very simple setting of classical duopoly theory the logical traps in the idea that agents can correctly foresee each others' reactions by experience provide important insights into the problems that more sophisticated game theoretic modelling approaches are attempting to solve.

The problems of handling pervasive uncertainty about the future in economic models and the need to take account of how people form expectations about the future has led economists to widely adopt the rational expectations hypothesis in both empirical and theoretical work. That is the hypothesis that agents in a stochastic model predict the future by the mathematical expectations of that same model. That way there is a standard solution to which one can refer. At least as long as the model is not plagued by multiple equilibria or even non-existence of equilibria.

The hypothesis has been motivated in much the same way as perfect foresight, that it is inconsistent with rationality to assume agents to make systematic mistakes since they should learn from them. Although that argument carries some force in theoretical models designed to analyze what happens if the economy is static or stationary in some well defined sense, it becomes very much less convincing in empirical models. Stationarity in a theoretical model serves as a tool to logically deduce "what happens if". Stationarity in an empirical model serves as a convenience to describe the real world "as if" it worked like the theoretical model. Since it does not, and we know it does not, the reasons for adopting perfect foresight of agents in a deterministic theoretical model do not carry over to the empirical stochastic model. The empirical model may very well be a better approximation to reality if it is based on some more flexible scheme of expectations formation. But still, even if the logic of the motivation is faulty, rational expectations may be a reasonable "as if" assumption in some circumstances, especially as compared with even less motivated alternatives.

However, in the theoretical stochastic model postulating stationarity, one would think that the assertion about perfect foresight eventually being learned must carry over. Since the agents must learn about complicated probability distributions in a stochastic model it is far from obvious that the argument really applies. This problem have resulted in a growing literature on how agents learn their rational expectations. The second essay in this thesis is an overview of that literature. Since the field is still in rapid progress, conclusions on this stage are rather conjectural. However, the great disparity of results seem to indicate that rational expectations cannot generally be validated by the outcome of just any adaptive process, but require some imposed coordination of learning behaviour and information available to the agents.

Moreover, the problems encountered in this research have also begun to cast doubt on the validity of the arguments for perfect foresight in deterministic stationary models. If there is learning going on the stationary equilibrium has not been reached and it seems that some degree of coordination and common knowledge is needed to ensure convergence to that equilibrium even in the deterministic context.

An area where expectations on the future are especially important is long term investment behaviour. Rational investors have to make some guess concerning the long term rate of return on fixed investment. Given well functioning capital markets their personal losses from miscalculations can be hedged and limited, but to society sub-optimal decisions on fixed investment always mean a loss compared to what could have been achieved otherwise. Expectations on the future therefore are central determinants on what rates of growth an economy will enjoy. The third essay considers how the interaction between the existing capital structure and expectations on the future quasi-rents may actually result in a slowdown of productivity growth even if the rate of change in technical production possibilities is tending to accelerate.

The following sections of the introduction will give a short summary of the essays.

1.2 Strategic conjectures

The first essay is a critique of a concept called "consistent conjectures", or some similar name, within oligopoly theory around which a flare of articles showed up in the beginning of the 80's. Although the logical difficulties inherent in the approach soon discredited it, one of the most logically confused variants of it still shows up now and then in journal articles, cf. Costrell(1990) for a recent example. Probably the survival of a theoretical concept that is seriously logically flawed is due to the fact, that once the model frame is accepted, calculations are rather easy.

The idea of Consistent Conjectural Equilibrium(CCE) is closely related to that of the Rational Expectations Equilibrium(REE) which surely inspired it. The CCE concept is however concerned primarily with strategic interaction among economic agents. It should not be confused with F. Hahn's(1977,1978) conjectural equilibria, which despite similarities explicitly assume strategic considerations to be absent in the minds of agents.

What is then the idea of CCE? It is most easily explained in terms of a simple duopoly market, where two firms are competing with a homogeneous product. In order to decide what price to set or quantity to put in the market the firms must make some assessment of the actions the other firm will settle upon. The time-honoured Cournot and Bertrand solution is that firms effectively consider the actions of the rival as independent of their own. Later game theorists have identified the equilibrium of this model as an early example of Nash equilibrium. The main idea behind CCE is that firms should recognize that market outcomes depend on both firms and the optimal decision therefore should anticipate the actions of the rival and eventually by experience learn to do so correctly. In the deterministic framework of this theorizing the idea is that agents have perfect foresight regarding the actions of rivals.

So far nothing is neither new nor very problematic. Although the realism of perfect foresight is questionable it is a standard tool used by economic theorists in order to isolate and clarify the logic of models. The peculiar aspect of the CCE approach is that it required perfect foresight about the rival's reaction function, i.e. his optimal actions described as a function of the actions of the rival.

Many authors took that as a requirement that both firms would in some sense correctly foresee the first derivative of the rival's reaction function in a neighbourhood of equilibrium, thus correctly predicting the constant or autonomous component of his response. Another variation was to require the reaction functions to constitute solutions to differential equations implied by optimization with conjectures about the rival's reaction. These approaches turned out to yield no determinate solution for the equilibrium since there were whole families of reaction functions that would satisfy these requirements.

But the surviving approach went one step further and asserted that the reaction function of the rival should be completely known within a neighbourhood of equilibrium. This introduces a potential conflict in the model since both firms will at the same time determine both their own action and the action of their rival. As long as that action is the equilibrium action no problem arises. Naturally there can then be no well defined reaction function to know anything about since none of the agents consider the others action as independent. However it is possible to derive a determinate solution in the special case of linear first order maximizing conditions, by shifting perspective. First optimizing under the assumption that the rival's reaction function was known and then solving for the consistent conjectures on reactions under the assumption that the conjecture about the rival's output can be replaced with his independent output. For further details, see the next chapter.

Here it only remains to point out the main fallacy, which obscured the logical untenability of the CCE argumentation, viz. the confusion of the model itself and the story used to rationalize it. The story behind the reaction function model is that agents acting according to their reaction functions will, under appropriate conditions, converge to an equilibrium, where their reaction functions coincide. The inconsistency that CCE was intended to amend then was that agents ought to learn about the rivals behaviour during this adjustment. After all, you could just plot the actions of the other fellow against your own, and then fit a function to it. Sounds simple enough, but only if the other fellow acts according to a fixed reaction function will this work. If he, too, is out there experimenting to find out your reaction function, the data you have collected gives you essentially no clue about what he thinks is optimal.

The reaction function model is based on the idea that if at least one of the duopolists acts as if at least some part of a rival's actions is independent of his own, then there is a fixed point of the system where none is induced to change their actions. Despite its name the reaction function model is not dynamic but thoroughly static and the dynamic story about adjustments is only an application of the correspondence principle, not a consistent dynamic model.

The dynamic story behind the static reaction function model is indeed one of learning, although it should not be taken as learning about the reaction functions but about which actions to take in order to get expected profits. Out of equilibrium it would be inconsistent to assume agents to be at the same time endowed with perfect foresight and optimizing. Out of equilibrium they must necessarily be either wrong or not optimizing. In either case there is nothing inconsistent with the static reaction function model. It is only when the story behind it is narrated as a story about learning others' optimal reactions that confusion arises. But that is a story inconsistent with the model. Not an inconsistency of the model.

In fact it has been shown recently that if one assumes complete common knowledge of costs, demand and optimization behaviour one can short-circuit the infinite regress of trying to outguess the rival. The solution is the Cournot equilibrium. Of course, under the perfect foresight assumption, we have no reaction functions, since if knowledge really is perfect and common there can be no optimal action outside of equilibrium, if the equilibrium is unique. Even if one can still define the optimal action for one agent contingent on the rival's action only one of these actions would really be optimal considering the knowledge possessed about the rival.

The main result in the essay here is that, unless it is deliberately excluded as a possibility, the Cournot equilibrium will even satisfy the formal conditions of CCE in Bresnahan's version, provided we stick to regarding the rival's output as dependent on the own also when evaluating the correctness of a conjecture. And it is the only solution that will do so for general demand and cost conditions. Of course, that is no proof of the consistency of the Cournot model, only of the inconsistency of the requirements of the CCE model.

1.3 Learning rational expectations

The second essay is an overview of the literature about learning of rational expectations. A common argument for adopting the hypothesis of rational expectations is that agents should not be assumed to make systematic mistakes in their forecasting. That argument implicitly assumes that they ought to learn the stochastic structure of the world they act in. But is that really true? What are the conditions that must be imposed to guarantee that agents really do learn the structure of the "true" model?

This issue has been thoroughly researched during the 80's and although much of the picture is still fragmented and disconnected, some contours have begun to emerge. The main emphasis of the essay is to tentatively bring together and shortly describe the essential features and results of different approaches to rational expectations learning. Some connections to more general adaptive learning literature in economics as well as to related problems in game theory are also pointed out.

One important feature of learning is that there is no learning where no mistakes are made. As can be deduced from the previous section this was an important observation in the first essay, too. It may seem a very trivial point to make, but it means that learning must necessarily be an adaptive process. In an economic environment where the errors you make in forecasting the future depend on both previous errors and errors made by other forecasting agents there is little reason to believe that learning will in general converge to some "true" model. The quotation marks are put there to emphasize that the model which would be true if everybody knew it to be true is actually not the model which generates the data from which it must be learned. This is the central problem of most of the research regarding rational expectations learning.

It has turned out that learning processes will converge to a rational expectations equilibrium in many simple and commonly used economic models. But the conditions for such convergence to take place are not altogether satisfactory. Very shortly and a little too simplified these conditions can be classified in two categories. One category is that the agents are supposed to possess common knowledge on the underlying fundamental model that enables them to use statistical procedures in a consistent way. The other category is conditions that somewhat loosely can be said to guarantee that they follow certain commonly recognized conventions in their behaviour and do not try to act in an overly sophisticated way.

The first category is often referred to as fully rational learning and is commonly modelled as Bayesian learning procedures. Learning in this context essentially boils down to statistical estimation of a correctly specified model or prior distribution of models. Although this type of learning models are highly sophisticated and generally converge most researchers have been dissatisfied with them since they seem to evade the main issue of learning by postulating too much to be known in advance.

The second category, commonly referred to as boundedly rational learning, therefore has constituted the mainstream in the literature. In this type of literature both results and modelling strategies have been much more diverse than in the first category. Recently methods from adaptive process control theory have provided a more unifying framework. But it is striking how convergence results often rely on a very mechanistic behaviour of agents. If agents try to outguess each other and experiment actively, learning processes often do not converge.

The analogy to the deterministic duopoly models treated in the first essay is very clear. If agents act mechanically according to some given rule they will often converge to equilibrium, and that is also the case if they already know everything except perhaps some constant parameter. Of course the analogy should not be taken too far. But it suggests that many learning problems of more general models may be profitably studied in the simpler duopoly context.

This is not all there is to learning. In the last few years papers have appeared using results from computer theory, pointing out that an economic system may not be learnable at all, in the sense that no algorithmic method exists that will allow us to infer the correct model from data without a priori information about its structure. There are also fundamental limits to what can be inferred from complex stochastic processes even if computability issues and the strategic interaction of learning agents is ignored.

Research using learning processes to establish a correspondence principle to choose the fundamental among multiple rational expectations equilibria has been partly successful in weeding out bubbles and sunspots by requiring stability of learning processes. The methods have however been seriously questioned on the grounds that the conditions for learning stability of one and not another equilibrium requires a coordination among agents on important features of the learning process. This coordination can be interpreted to mean that agents agree before any learning takes place on which equilibrium they want to converge to.

On the whole then the research on rational expectations learning cannot provide any general validation of the use of the rational expectations hypothesis in economic modelling of expectations. In certain simple situations where the underlying information assumptions can be reasonably thought of as valid, it may still have a place. In general however the theoretical research on learning seems to give little support to the idea that rational agents should learn to have rational expectations in situations where conventions and common knowledge cannot coordinate learning behaviour sufficiently. That would be especially true of long term expectations on the outcomes of irregular dynamic growth processes.

1.4 Productivity growth and expectations

"It is by reason of the existence of durable equipment that the economic future is linked to the present. It is, therefore, consonant with, and agreeable to, our broad principles of thought, that the expectation of the future should affect the present through the demand price for durable equipment" (John Maynard Keynes(1936), end par. of chap. 11)

It could be added that durable equipment not only links the future to the present but also the present to the past. The third essay of this thesis treats the irregularities in past economic development as reflected in the existing capital structure. This structure interacts with the expectations on future quasi-rents to be earned from fixed investment and to a large extent this interaction determines how technical change is translated into productivity growth.

For the problem to be tractable many simplifying assumptions are made, but a basic condition can be derived showing that deceleration in labour productivity simultaneously with an accelerating change in technical production possibilities can occur for a wide range of variations of the model.

The main mechanism relies on the necessity to transfer labour from capital equipment on the verge of obsolescence to new investment. If that transfer is increasingly costly in terms of the wage increases necessary to ensure a given labour supply, deceleration in productivity growth is very probable. The mechanism is likely to be reinforced by expectations if investors expect changes in interest rates to be greater than changes in the rate of wage increases in some average sense over the life of the investment.

The change in total factor productivity will be in the same direction as that of labour productivity, if the capital/output ratio is assumed to be constant, and in many other cases, too. A low elasticity of substitution in production possibilities will also tend to slow down the transmission of gains in production possibilities into actual productivity gains.

Although the connecting theme of expectations in the third essay is less clearly visible I would like to point out that this lower visibility is in part due to the conclusions drawn from preceding essays. Since long term expectations on return of investment should not be characterized neither as static nor as perfect foresight in a model concerned with irregular dynamic growth, expectations have been modelled as exogenous due to lack of a better alternative. Of course it would be desirable to model them as endogenous and to a large degree determined by recent experience. How this endogenous component of expectations moves is only informally discussed, but it seems that the more investors rely on the recent past in forming expectations the more probable it is that expectations will accentuate slowdowns and enhance speedups of productivity growth.

Expectations in this kind of model therefore may provide a mechanism that reinforces variations in productivity growth induced by the variations in the past that has been recorded by the capital structure. A better understanding of the relation between the past, the present and the future, such as it is reflected in the stock of capital and its growth through investment may be very important in order to avoid unnecessary detrimental effects of animal spirits that are overly pessimistic, or optimistic, since the latter mood may well entail as much waste of society's resources as the former.

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Chapter 2: The Inconsistency of Consistent Conjectures Coming back to Cournot.

2.1 *Introduction*

In the beginning of this decade some debate arose concerning a concept called "consistent", "rational" or in some case "endogenous" conjectures.[1] The concept refers to the conjectures oligopolists are supposed to form regarding the reactions of their rivals. The attempts to model oligopolist behaviour "consistently" have however not been successful. The aim of this paper is to show that this is due to fundamental logical problems inherent in the concept of "consistent" conjectures. It is further shown that the simple Cournot model with zero conjectures can be interpreted so as to satisfy a property commonly used as a consistency criterion.

Intuitively the requirement of consistency in this context means that agents can correctly foresee the behaviour and predictions of other agents. That may seem reasonable at first glance. On reflection, however, it is clear that it implies that a specific agent correctly predicts actions which in turn are based on correct predictions of the action this specific agent will take if he correctly predicts these actions. Clearly, we may easily get involved in circular reasoning here. This paper argues that circular reasoning indeed is present in the concept of "consistent conjectures".

The literature treating "consistent" conjectures of oligopolists has been concerned with the well known reaction function models. These go back to Cournot(1838), Bertrand(1883) and the beginnings of oligopoly theory. The addition of a conjecture about the rivals' reactions has been attributed to Bowley(1924). The term "conjectural variation" is due to Frisch(1933) and has come to be the accepted term for this special kind of expectations. In the conjectural variation models the oligopolistic firm maximizes the perceived profit taking account of the reactions of the competitors. The equilibrium of a conjectural variation model is then defined as a fixed point, if it exists, of the system of reaction functions.

When Fellner(1949) made his much quoted remark that firms in reaction function models "are right for the wrong reasons" he elegantly captured the essence of a common critique against reaction function models. The firms will in equilibrium correctly predict the output level of competitors, but will in

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[1] "Consistent" is used by e.g. Bresnahan(1981) and Perry(1982), "rational" by Laitner(1980) i.a. and "endogenous" by Guttman and Miller(1983).

general have the wrong idea about the reactions that would follow according to the reaction functions if they were to deviate from equilibrium. This has been considered a major flaw in this type of models and has provided motivation to most of the work on "consistent" conjectures in oligopoly models.

Another perceived drawback of the traditional models is the multitude of equilibria which a priori is possible. Obviously it would make economic analysis of imperfect competition more determinate if some "rationality" or "consistency" requirement could limit this unwanted abundance of equilibria. Whether this would make the model better or worse for real world applications may be hard to say. But it certainly would be easier to handle for the theorist. This is a second motivation for the work on consistent conjectures.

The quotation marks around consistent and rational will be skipped hereafter, though, as will be shown, the consistency or rationality of the model conjectures which are so called seems to be wanting in many respects. In the last decade quite a surge of research efforts came about, inspired by Fellner's remark and also by the hope to arrive at some restriction on the number of possible equilibria in oligopoly models. This latter hope was no doubt inspired by the rational expectations hypothesis.

An early attempt to solve these problems is the asymmetric leader-follower equilibrium of Stackelberg(1934). This equilibrium has the property, when constructed in a conjectural variation model, that any firm will have its conjectures about the reactions of other firms in equilibrium confirmed, if it were to deviate from the equilibrium output levels. In a review of Stackelberg's book, "Marktform und Gleichgewicht" , Wassily Leontief(1936) proposed the construction of a symmetric "leader-leader" equilibrium of this kind in which both firms expect certain reactions from the other and those expectations will be borne out in the case one of them would try to test them. The conjectural variation will be correct and the firms will in some sense be "right for the right reasons". However, that was largely forgotten and although Fellner(1949) refers to Leontief's review he does not comment on this point. Leontief's construction is essentially identical to the requirement of local consistency (cf. section 2.3) although he regards it as a stability property, quite in line with Stackelberg's own view that oligopolies are inherently unstable and must be stabilized by state intervention. The numerical solution Leontief derives conforms exactly with the algebraic solution derived by Bresnahan(1981) for the case of linear first order profit maximization conditions.

To keep the special problems associated with the analysis of this strategic interaction between oligopolistic firms as clear and simple as possible, it will be assumed in this paper that entry barriers of some kind make it unnecessary for the firm to contemplate the reactions from potential competitors. It will also be assumed that explicit cartels are not possible to enforce in the market. To simplify matters further only duopoly models will be considered in order to focus the discussion on the direct strategic interaction between two competing rivals. In that way we can avoid the complications of treating indirect reactions and the possibilities inherent in implicit coalitions. It will also allow the use of a somewhat more transparent formal notation. Even though the following discussion is cast in quantity terms, price or some other decision variable would do just as well except in extreme cases like homogeneous products.

Section 2.2 gives an overview of some approaches to the consistency or rationality requirement imposed upon conjectural variations in the literature and informally explores weaknesses of those approaches. In section 2.3 the most commonly used formulation of consistency, implying complete knowledge of the derivative of the rival's reaction function at least locally, will be scrutinized with the aid of a formal model and it will be demonstrated that the Cournot model paradoxically becomes consistent in this strong sense (unless some in this context rather strange information restriction is imposed). More precisely, if equilibrium output levels are observable by the agents then this paradox holds. It would be strange to suppose they were not, considering that reactions are supposed to be observable.

2.2 Some alternative concepts of consistent conjectural variations

An intertemporal consistency requirement imposed upon the conjectural variations of our duopolists might be, and indeed has been, interpreted in several ways. The intuitive notion of consistency desired is that firms with experience should learn how competitors react to changes, i.e. the conjectural variation ought to become a correct prediction of the optimal reaction of the rival. Two questions then arise. How does the firm come to learn this and in what sense will conjectures be correct? Only the latter of these questions will be directly addressed in this paper.

The strongest form possible is to require that the rival's reactions to all possible actions are known. However, most authors restrict this requirement to some local (possibly infinitesimal) neighbourhood of the equilibrium. Why they do so is not quite clear, as it will in most cases be equivalent to a global requirement anyhow, because of regularity conditions imposed on demand and cost functions. However that may be, this kind of condition will be referred to as local consistency conditions.

Still other authors are content if the rival's reaction is known correctly only at the equilibrium point. Such restrictions will be referred to as pointwise consistency. This latter approach of course just pushes the problem perceived by Fellner(1949) one step further, because agents will then in general be wrong about their competitor's *reactions* everywhere outside of equilibrium, so they are still right for the wrong reasons, although on another level. Some interesting papers with this approach are i.a. Kamien and Schwartz(1983) and Boyer and Moreaux(1983a and b). These papers also demonstrate that pointwise consistency requirements do not achieve any helpful limitations on the numbers of possible equilibria. Though different kinds of pointwise consistency may be interesting in their own right they do not solve any of the two problems perceived in conjectural variation models. We will therefore concentrate the discussion on the local consistency approaches.

The local consistency approaches entails two quite different but at first sight similar formulations. Laitner(1980) makes the assumption that the reacting firm always believes that the output of the acting firm is an optimal output, and hence every deviation from equilibrium is taken to indicate a shift in the reaction function of the acting firm. He calls this "rational conjectures" but I find "good faith conjectures" to be a more descriptive name. The framework effectively excludes every possibility of *testing* conjectures, because a non-optimizing test output will be believed to be optimal, and the reacting firm obviously does not know the true reaction function of the testing firm. It seems that this kind of model must implicitly assume that the profit function of the rival is not completely known. Otherwise the conjectures could not possibly be as diverse as Laitner e.g. concludes. This is by no means any fault to be criticized, on the contrary the notion of conjecture should be closely associated with imperfect knowledge.

Makowski(1987) emphasizes the crucial importance of initial conditions in models like Laitner's. Though the reacting firm may be perfectly happy to remain in the new situation, given this as an initial value, that is in no way to say that the move to this new situation is optimal given the original situation as initial value. Makowski calls this a "deus ex machina" assumption but it may in fact be rationalized in different ways. Laitner expresses his view as:

> "The first firm should not be required to predict responses on the part of its rival that maximize the rival's profits of[if?] the rival initially shows no inclination to act in its own best interests"(Laitner(1980), p. 646)

A firm which will always believe that the rival offers only equilibrium quantities has hardly any way to come to know the other's reactions. This asymmetry between the acting and the reacting firm implies the former can test and confirm the latter's behaviour when this reacting firm clings to the false belief that the acting firm never tests its conjectures. It seems obvious to suppose either firm could take up any of the roles. Why should it then be ignorant in the one role and not in the other? Marschak and Selten(1977,1978), whose concept of restabilizing responses underlies Laitner's approach, provides a somewhat different rationalization. The players of a strategic game have to make some assumption regarding the psychology of the opponent. In this case the assumption is that every choice of a rival is in his best interest and that he intends to stick to it in the long run. Such an assumption of course effectively prohibits the recognition of testing behaviour as well as outright attempts to mislead. In the limited information framework we are discussing here, it seems contradictory to presume that agents will not test each other or commit mistakes. It is after all exactly these possibilities that provided the motivation in the first place for dismissing the alternative but simpler concept of traditional exogenous conjectural variations, on the grounds that firms sooner or later ought to discover that their output predictions will be wrong out of equilibrium. The good faith conjectures then relies heavily on an assumption which makes them rather unnecessary in this context. Even if they were accepted in spite of this, Laitner shows that the multiple equilibria problem cannot be solved anyhow by this approach.

The remaining local consistency approach used by i.a. Bresnahan(1981), and Perry(1982) makes no such assumptions. As this is the concept that has been applied in a number of more recent papers it will simply be referred to as the local consistency concept.[2] In this case an optimal reaction is calculated given the new output combination as a starting point. The difficulty pointed out by Makowski(1987) for this definition is that the two firms will have differing views as to when the chain of reactions will end. A testing firm will expect the two step process of changing its output and then observe the other's reaction. The reacting firm however will choose its reaction assuming the former firm will in turn react on this choice.[3]

There is however a much more fundamental problem involved in this latter concept. A problem which goes beyond the difficulties in a dynamic interpretation of the static model. In equilibrium, where none of the firms wishes to change its decision, the optimal reaction may well be argued to be: not to react to any changes in the rival's output. That is so because assumptions are made ensuring that the rival's reaction function is known and hence there is no reason to react to tests as the optimizing decision then has become independent of the other's real output. This claim is obviously true in the case of simultaneous decisions when, however, no conjectures about reactions are

[2] One of the most recent applications is Scafuri(1988).

[3] Makowski also claims that a common result in the literature is false. In models with homogenous products and constant marginal costs it is easily shown that so called Bertrand or competitive conjectures are consistent according to e.g. Bresnahan's definition. Now Makowski shows that due to non-differentiability in equilibrium the definition of consistent conjectures used leads to non-existence. This is quite true, but the relevance is questionable since the problem disappears if the products are allowed to differentiate or marginal costs become non-constant. Considered as a limiting case the formal definition of consistency then must be somewhat amended, but the general result that local consistency tends to produce more competitive results remains true.

needed anyhow, as there will be none in a one-shot static model. In the more appropriate alternating decision case treated by Makowski, the same claim would apply nevertheless unless the decision rules of the firm is changed. If the firm treats the rival's reaction function as known, then observations of the output of this rival simply will not enter the decision problem of the firm. Given a unique solution to this problem there will be only one possible action. The same reasoning of course applies to the rival and hence the known reaction function can be correct only if it prescribes no reaction so the reaction function is a constant. Which constant output you assign to the rival is dependent then on which constant output he assigns to you and so on.

Although the formal model as such may be perfectly consistent a paradox arises in the dynamic story that motivated the quest for consistency in the first place. It is imperative to realize that in equilibrium expected outputs are mutually confirmed for any conjectural variation model. The problem of inconsistent conjectural variations is a problem in the dynamic story, not in the static model.

We have thus in our motivating story become entangled in the classic paradox first attributed to the Cretan philosopher Epimenides (600 b.C.) who declared: "Cretans are always liars." If so, what about Epimenides himself? A somewhat younger but clearer version is the also ancient proposition: "This statement is a lie." If it is true it must be false but then it is true and then... ? In the same way local consistency in conjectures implies such conjectures to be the Cournot conjectures that outputs are independent. A reaction function as traditionally defined will however not be constant. Hence Cournot conjectures are inconsistent!

If by chance moved outside of equilibrium to some arbitrary output combination, the firms will know the optimal outputs of each other and so immediately return to equilibrium. Hence there is no converging adjustment process of the kind commonly used to rationalize reaction functions, nor are there any stability issues.

If a firm should respond to tests, rationality would require it to use the knowledge that it is being tested. Thus it must engage in guessing how the opponent will interpret different reactions. This becomes a very complex problem indeed, because if your rival knows that you know that he is testing, then he must of course be prepared to allow for attempts to mislead and deceive when he interprets test results. But then you can engage in trying to foresee how far he will foresee your shrewdness. And so on, and so on. Will such outguessing then result in consistent conjectures?

Rational agents trying to outguess each other will anyway only rarely find themselves in a locally consistent conjectures equilibrium. One reason is that it is a typical feature of the quantity models used to model local consistency that the equilibrium profits are lower in a consistent conjectures equilibrium than in the ordinary Cournot equilibrium.[4] Economic rationality hence cannot motivate such an equilibrium concept and it is very hard to think of any plausible adjustment or learning process in which agents arrive at such conjectures voluntarily and using all available information.

Andrew Daughety(1985) goes a long way to clarifying the exact source of the confusion. If I have to act on a belief about your action, given your belief about the action I would take, given my belief about what you believe I would do ...etc., then we get involved in an infinite regress. Daughety's contribution is to face this and explicitly model the infinite regress. He defines a rational pair of consistent conjectures as the equilibrium where the rival actually conjectures your optimal action correctly, given that your conjecture about his action is correct. That cuts the regress down to a more manageable two stage process. With complete information on demand and both cost functions and an assumption of common knowledge about optimization behaviour this results in a Cournot equilibrium as the only possible equilibrium. However, contrary to the assertions in the local consistency literature, this is consistent in the sense that the notion of conjectural variation refers to the imagined reactions at each step in the infinite regress modelled to take place in the mind of the rival. These are shown to be the same as the slopes of the ordinary Cournot reaction functions. Perhaps not very unexpected since this infinite regress essentially is the same adaptive process that Cournot initially had in mind, though he thought of it as taking place in reality while the infinite regress model presumes it to take place in the minds of the two rivals.

It should be noticed, however, that complete information assumptions are not in the spirit of the conjectural approach. In general incomplete information can be expected to reintroduce a difference between the Cournot equilibrium and the conjectural variation equilibrium modelled as an infinite regress where Daughety's consistency condition holds.[5]

2.3 Local consistency – accidental and paradoxical

The various attempts to achieve intertemporal consistency and at the same time restrict the number of equilibria have been rather unsuccessful as indicated by the short and incomplete survey above. In most cases no useful limitation of possible equilibria have been achieved, and the resolution of the "right for the wrong reasons" issue is far from convincing. The local consistency

[4] In price models the contrary is the case however, cf. Holt(1985) and in public goods models the consistent conjectural equilibrium raises social welfare, cf. Scafuri(1988).

[5] It need not even be close to the Cournot equilibrium. Rubinstein(1989) shows that almost common knowledge results are not necessarily close to results derived under the assumption of complete common knowledge.

concept is however somewhat of an exception. In a linear demand cost structure Bresnahan(1981) demonstrates that there exists a constant conjectural variation that is locally consistent and unique within a fairly wide class of conjectural variation functions. This section will show why this equilibrium concept must be regarded as accidental in the formal model and paradoxical in the economic interpretation.

The decision making firm will be called X and its rival Y. Each firm is taken to know the market demand function for its product. This demand function is assumed to have an inverse that is the price function.

(3.1) $p(x, y)$ for firm X and $q(x, y)$ for firm Y

where x and y denotes the output quantities of firm X and firm Y. The cost functions are the simplest possible denoted by

(3.2) $c(x)$ and $d(y)$ respectively.

The profit functions then are defined by

(3.3) $V(x, y) = p(x, y) \cdot x - c(x)$ and $U(x, y) = q(x, y) \cdot y - d(y)$

and all functions are assumed to be continuously differentiable to any required degree.

The behavioural assumptions of Cournot(1838) and Bertrand(1883) implied that each firm maximized profits while regarding the competitors decision variable as fixed, be it quantities as assumed by Cournot or prices as preferred by Bertrand. That is to say, the decision variables of the two firms are formally independent of each other. In a static model where decisions are taken once for all this would seem to be a perfectly reasonable approach but for one small detail. In calculating the price (or market demand) some value has to be supplied for the other firm's quantity (or price) if any definite decision is to be reached. The optimizing decision thus becomes dependent on the competitor's decision. We will use an asterisk as superindex on the decision variable to denote this *reaction function* of the firm, e.g. $x^*(y)$ which in this case determines the profit maximizing quantity for X to produce and put on the market for each quantity the rival Y sees fit to output.

DEFINITION 1. A stable model equilibrium satisfies the following conditions

(i) The reaction functions have a common point in the non-negative orthant of the $x-y$-plane, i.e. a fixed point exists such that $(x,y) = (x^*(y),y^*(x))$ and all quantities are non-negative.

(ii) The profit for both firms are non-negative at the equilibrium quantities.

(iii) The equilibrium is stable, i.e. given a temporary disturbance of cost or demand parameters the two firms by observing the other's output and reacting according to their reaction functions will make a sequence of quantity decisions converging back toward the original equilibrium quantities.

Conditions on the demand and cost functions ensuring this can be found in e.g. Friedman(1977). We will not discuss stability or existence further but just note the potential for destabilizing reactions and non-existence of equilibrium. In the following stable equilibria will be assumed to exist.

The term reaction function implies there is some notion of dynamic background for this type of models. We may think of it as some kind of tatonnement process, perhaps in the form of an auction that precedes the final once for all equilibrium decision. We might also picture it as Cournot originally did as a real time adaptive process where the firms alternately make new choices in each period based on the decisions of the former period and learn by adapting their output until ex ante profits equal ex post profits. The Cournot assumption of independence indicates a conjecture of zero reaction, i.e. the firm conjectures that no reaction will take place.

It should in fairness be noted here that there really is no good story to motivate an interpretation in terms of reaction expectations because if decisions are taken once for all simultaneously there will be no opportunity for reactions. If decisions are taken in turns we have to allow for a dynamic convergence process and then the agent could as well wait and see what reaction actually will take place before he makes a counter move. This has been taken to indicate that the whole idea of conjectural variation models is logically flawed. This is true of the special interpretation used here and in the consistent conjecture literature. However, when Frisch(1933) coined the term "conjectural variation" he explicitly had in mind a parametrical representation of a dynamic adaptive system where the reaction functions actually are the loci of points of sign change in the direction of change in the respective decision variables of the phase diagram. The conjectural variation in this setting is interpreted as a parametrical guess on the drifting forces out of equilibrium. The logical difficulties associated with the reaction interpretation within a static framework has no bearing on this dynamic interpretation. This is, however, not the subject of this paper, which on the contrary aims to illuminate the difficulties with a strict reaction foresight approach.

To avoid excess formulae we will normally only state one of the symmetric conditions and equations for the firms X and Y. Let X be a profit maximizer trying to foresee Y's optimal reactions, that is trying to solve the problem

$$\max_{x} p(x,f(x)) \cdot x - c(x) \tag{3.4}$$

where $f(x)$ is the conjectured dependence of y on x.
The first order necessary maximizing condition is then

$$(p_x + p_y \cdot f'(x)) \cdot x + p(x,f(x)) - c'(x) = 0 \tag{3.5}$$

where the subindices indicate the partial derivatives of the price function. The conjectural variation of X, $f'(x) = \mathrm{CV}_x$, i.e. the conjectured reaction of Y to a change in x.

DEFINITION 2. The conjectural variation CV_x is *consistent* if

(3.6) $f'(x) \equiv y^{*\prime}(x)$ for some $\epsilon > 0$ and all $x \in [x - \epsilon, x + \epsilon]$

meaning that X will correctly foresee the optimal reaction to changes in y at least within some local neighbourhood of the equilibrium.

DEFINITION 3. If (3.6) and the corresponding condition for the other firm holds in addition to the other equilibrium conditions then there is said to be a *consistent conjectural equilibrium.*

The crucial point (made already by Fellner(1949) in a footnote on p. 119) is that *a conjectured reaction can be correctly predicted only if it takes place along the rival's reaction function.* The conjectured action–reaction pairs of outputs must thus for each competitor be restricted to the rival's reaction function if the conjectural variation is to be correct. The action can then be optimal only if the reaction functions coincide in the same point in the *x*–*y*–plane. If we have a unique equilibrium this will be a unique point.

When defining reaction functions the implicit assumption is made that we can replace $f(x)$ with y in the first order conditions (3.5) above. This assumption is however consistent with correct knowledge of the optimal choices of the rival *only* in equilibrium. Outside the equilibrium $f(x)$ will *not* be identical to the optimal choice $y^*(x)$. If it was, every point on the reaction function $x^*(y)$ would be an equilibrium. This is a very important observation. The reaction function as it is defined above and in most of the literature should reflect the optimal decisions of the agent given information on the rival output. But presuming complete information about the rival's reaction function would imply that the rival's reaction function should be substituted for past information on his output in the own reaction function. Hence the optimal reaction function becomes effectively independent of the competitor's real decision, just as a Stackelberg leader frees himself from dependence on observations of the followers. The reaction function thus becomes a constant, and the intuition behind Daughety's result (cf. section 2.2) is clear, because consistent conjectures then are of course the Cournot conjectures that no reaction will take place. The reaction function as commonly defined presumes that rival output varies independently of the own output.

To get around this, it is necessary to assume the level of output to be unpredictable outside equilibrium even though the derivative of the reaction function is completely known within the local neighbourhood. To assume unpredictable levels of output in an ordinary conjectural variation model is quite natural as the agent make conjectures precisely because he has no means of knowing the true reaction. In the case with consistency imposed it is of course a rather strange information assumption. But nevertheless let us assume our duopolists to behave as if they were subject to such information restrictions, and pretend that it is reasonable to still assume that they should learn reactions somehow.

If the derivative of the rival's reaction function is known as a function of the own output in some neighbourhood of the equilibrium, then under certain regularity conditions, e.g. if this function is Lipschitz-continuous in the own output, then this reaction function is uniquely determined up to a constant within this neighbourhood.[6]

When the equilibrium is reached and recognized as such, this may be used as an initial condition determining the constant, too, and hence the reaction function of the rival *will* be completely known, and $f(x) \equiv y^*(x)$ in the local neighbourhood. This shows that the assumption of unpredictable levels in general will be incompatible with both knowledge of reactions and optimal, rational use of that information. Unless, of course, we suppose our agents to be unaware of elementary calculus results.

To be consistent with the degree of sophistication we have earlier endowed our agents with we ought to accept that the level of output too must be correctly predicted. But then we get into logical trouble. Defining the reaction function for X then will be ambiguous since we cannot simply replace $y^*(x)$ by y in the first order conditions. That is so because these conditions will include the derivative $y^{*\prime}(x)$ which is related to Y's output in a way that directly depends on $x^*(y)$. The definition hence is circular and has to be resolved by some further condition. One of the easiest ways is by assuming the CCV (short for consistent conjectural variation) to be a constant and so avoid the troublesome dependence. Such an assumption is however not consistent with general demand and cost functions. This is stated formally as:

Proposition 1.

a) If the first order conditions for profit maximization are locally linear in x and y then a locally consistent CV, if it exists must be constant.
b) If a locally consistent constant CV exists then the first order conditions must be locally linear.

Proof.

a) Let α, β and A be constants, $\alpha \neq 0$, then

$$A + \alpha x^* + \beta y = 0 \text{ imply } \frac{dx^*}{dy} = -\frac{\beta}{\alpha} \text{ is a constant.}$$

[6] The conjectural variation is $f'(x)$ so Lipschitz-continuous means that $|f'(x_1) - f'(x_2)| \leq L\,|x_1 - x_2|$ for some constant L and all x_1, x_2 in the neighbourhood of equilibrium in Definition 3. This is not a very stringent condition under the standard regularity assumptions of the conjectural variation models. If this assumption holds then the conclusion follows from elementary theorems on ordinary differential equations.

b) If $\frac{dx^*}{dy} = \gamma$ is locally constant and consistent then by integration the reaction function necessarily is $x^*(y) = B + \gamma y$ where B,γ constants and hence the first order conditions must be locally linear. **QED.**

This is all very obvious and is stated as a formal proposition only to emphasize that the assumption of constant locally consistent conjectural variations is not compatible with arbitrary demand and cost assumptions. Provided the profit function is regular enough we can easily construct constant CVs *approximating* the true derivative of the rival's reaction function arbitrarily close in a small enough neighbourhood, but *that is not the issue here*. The claim of the local consistency approach is to have a functional identity in the neighbourhood and thereby achieve essential restrictions on the number of possible equilibria.[7]

Bresnahan(1981) proves a theorem showing that constant CVs are the only consistent CVs in the class of polynomial conjectural variations in the special case when the profit function is quadratic in the own quantity and linear in the other. Robson(1983) extends this result to uniqueness in the class of analytic functions. (See the appendix for some further comments on these results.) Robson also proves the non-existence of such constant CCVs when the inverse demand function is changed to incorporate the square of the own quantity. Just like proposition 1, only less general and more complicated, this shows that the method to avoid the self-referential problem by letting the CV be constant cannot be extended beyond linear demand and linear marginal costs. Note that, because the CV is part of the first order conditions, these may be non-linear if the CV is non-constant, so proposition 1 does not imply the uniqueness results of Bresnahan and Robson.

In a reply to Robson, Bresnahan makes a reformulation of the consistency condition in order to amend for this non-existence result. The produced quantities are defined as functions of some unspecified strategic variables in a one period game with the usual Nash equilibrium concept. Formally $x = x(a, b)$ and $y = y(a, b)$ and we have first order conditions

$$\frac{\delta V(x(a,b),\ y(a,b))}{\delta a} = 0 \text{ and } \frac{\delta U(x(a,b),\ y(a,b)}{\delta b} = 0 \tag{3.7}$$

respectively contingent on the rival's strategy. Letting $x = a$ and $y = b$ we obtain the traditional Cournot equilibrium. Letting a and b be the respective prices and x and y the demand functions the Bertrand solution of the price model will result. In general the procedure amounts to a change of strategic

[7] Boyer and Moreaux(1983a and b) employ a concept of pointwise consistency to construct such approximations. Kamien and Schwartz(1983) use a different variation on these theme. In both cases it is shown that approximative pointwise consistency does not in any essential way restrict the number of possible equilibria in conjectural variation models.

variables in the problem. Consistency in this extended meaning then is achieved by choosing such a change of variables that the reaction functions $a^*(b)$ and $b^*(a)$ are constant. This will according to Bresnahan (1983) be equivalent to the conditions

$$\frac{\delta^2 V}{\delta a \delta b} = 0 \text{ and } \frac{\delta^2 U}{\delta b \delta a} = 0 \tag{3.8}$$

We are thus choosing a strategy space in order to fulfill the consistency condition by the right transformation of the variables. Bresnahan's original consistency concept is more restrictive in that it implies $\delta x/\delta a = 1$ and conversely $\delta y/\delta b = 1$. This formulation emphasizes two problems and clarifies the sense in which consistency is achieved.[8] Firstly there will, if a solution exists, often be a multiplicity of solutions from which we can choose only by providing further conditions. This follows because (3.7) in general is a non-linear system of second order partial differential equations in the functions we seek for a consistent change of variables, $x(a,b)$ and $y(a,b)$. Secondly it emphasizes the arbitrariness of this consistency concept. The strategy spaces of the players are made dependent, not on any behavioural assumptions, but on some special transformation of the geometry of the cost and demand structure. The strategy variable for the firm will in general not correspond to any natural economic decision variable but to something in between. To economists it must be crucial to recognize that optimality properties are not an invariant with respect to changes of variables. In oligopoly theory that is obviated by the well known difference in outcomes between Bertrand and Cournot models.

Even if we accept the restriction to linear models and the rather strange interpretation associated with the change of variables, the common result in quantity models, as remarked earlier, is that firms entertaining non-zero constant local CCVs always receive a lower profit than they could have got by sticking to the primitive Cournot assumption of zero conjectural variations. We must obviously ask ourselves what economic motive could possibly make

[8] That is, apart from purely technical problems. This change of variables approach can rule out some cases of non-existence for consistent conjectures of the more restrictive type. The conditions (3.8) above are also recognized as hyperbolic partial differential equations solved by $V(a, b)$ and $U(a, b)$. It would be extremely surprising if these two conditions were consistent for the same change of variables unless $V(x, y)$ and $U(x, y)$ has some special kind of symmetry. It seems unreasonable to let such symmetry be a necessary implicit assumption of a duopoly equilibrium concept. In any case there can be no a priori guarantee that the Nash equilibrium is among the admissible restrictions needed to solve for the change of variables from the partial differential equations defined by the above consistency conditions. The theory for partial differential equations suggests that the two kinds of conditions may very well be inconsistent with each other (cf. Fritz John(1982)). Even if uniqueness of the consistent equilibrium can be achieved in some special cases the change of variables may very well destroy concavity in the profit function. So even if there were a conceptual appeal in this formulation there would still remain a host of technically non-trivial problems to solve. It would seem this concept of consistency raises a good deal more problems than it is solving.

firms adopt such clearly suboptimal conjectures. Bresnahan does provide an answer:

"The spirit of this enterprise is therefore not one of giving firms discretion, but of removing their discretion by imposing correctness." (Bresnahan(1981), p. 937)

What is then the force "imposing correctness"? Certainly not the rationality of economic agents. It seems to be fundamental in economic theory to assume that agents act consistently rational in given environments to the best of their ability. It may well be that rationality is bounded by information restrictions or computation capabilities. That is, however, a radically different issue. If we are to require that economic agents choose their environments in order to look as if they were acting consistently, that may perhaps be an enlightening thought experiment. It cannot, however, in any conventional sense be a model of real economic behaviour.

To emphasize the conceptual confusion inherent in these consistency concepts we will reproduce the Epimenides paradox referred to in section 2.1, by proving that Cournot conjectures may well be regarded as locally consistent, although the reaction functions certainly will not imply zero reactions. As Daughety(1985) has shown, consistency can be motivated rigorously under perfect foresight and common knowledge assumptions by explicit modelling of the infinite outguessing regress of optimizing agents. This is not the aim here. We shall only point out the treacherous change of perspective that takes place in the conjectural variation model. A change of perspective that all too easily escapes unnoticed.

As was explained above the concept of local consistency hinges critically on the assumption that the level of output from the rival is impossible to predict although the reactions from the rival are known with certainty. Bresnahan(1983) points out that this is equivalent to some special choice of decision variable in order to transform the profit functions into a form consistent with the condition (3.8). This means that the reaction functions in terms of those variables will be constant and hence Cournot conjectures regarding the transformed decision variable are the only consistent conjectures. That far everything is quite clear. *But even regarding the original decision variables Cournot conjectures will satisfy the consistency condition (3.8).*

The condition (3.8) may be interpreted in this way: that a change in the rival's decision variable will not change the marginal profit with respect to the own decision variable. Hence marginal profit is independent of the rival's decision variable locally. It has been argued above that once in equilibrium firms with locally consistent conjectural variations can easily compute the autonomous part of the rival's output. Hence X will be maximizing $V(x, f(x))$, i.e. finding optimum on the three-dimensional profit surface above the path in two-dimensional output space described by the rival's reaction function. Or equivalently, maximizing the one-dimensional projection of the profit function restricted to this path. This fact is exploited in the following proposition.

Proposition 2.

Cournot conjectures are consistent according to (3.8) when we let $x = a$ and $y = b$ be the identity transformation

Proof.

The perceived profit function will be maximized when marginal profit is zero given conjectures $y = f(x)$ hence first order conditions imply

$$V_x(x,f(x)) + V_y(x,f(x)) \cdot f'(x) = 0 \tag{3.9}$$

Let $x(y)$ be an arbitrary parameterization. If $x(y)$ is an optimal reaction path of X then marginal profit along this path is kept constant at the zero level. By differentiation of the marginal profit along $x(y)$ with respect to y we then will obtain

$$\frac{dx}{dy}[V_{xx} + V_{xy}f'(x) + V_{yx}f'(x) + V_{yy}(f'(x))^2 + V_y f''(x)] = 0 \tag{3.10}$$

as a consistency condition. This will of course be satisfied if the optimal reaction, $\frac{dx}{dy}$, equals zero. Cournot conjectures then satisfy (3.8) for arbitrary profit functions. **QED.**

Note that (3.10) will of course hold also if the expression within brackets is zero. This is the ordinarily used expression to compute the CCV by substituting y for $f(x)$ and solving for $f'(x)$, $f''(x)$ is of course zero when $f'(x)$ is constant. However, we can easily see that although (3.8) is satisfied if there are Cournot conjectures, the verbal interpretation of this condition will not hold. Let $p(x,y) = K - ax - by$ and $c(x) = c{\cdot}x$ where K, a, b, and c are nonzero constants. Then $V(x,y) = (K - ax - by)x - cx$ is X's profit function and from the first order maximizing condition we can then derive

$$x^*(y) = \frac{K - c - by}{2a} \tag{3.11}$$

and consequently

$$x^{*\prime}(y) = \frac{-b}{2a} \neq \mathrm{CV}_y = 0$$

Let us review how this come about. By regarding the rival's output as a function of the own output, like a Stackelberg leader, the perceived profit function is reduced to depend only on the own decision. Independency of course imply that the perceived optimal path $x(y)$ is constant and do not vary with y, hence Cournot conjectures are consistent. The expression (3.11), on the other hand is derived under the assumption that perceived profit is, first, in the derivation of the first order conditions exclusively a function of the own output and then, shifting perspective, we derive a reaction function under the assumption that y now is independent and is substituted for $f(x)$ in the first order conditions. The derivative of this function is easily seen to be non-zero, and we conclude that the firm is "right for the wrong reasons".

Somewhat confusingly we can then compute the ordinarily used *consistent* conjectural variation by the bracketed expression in (3.10) where we in the derivation revert back to the assumption that y depends on x. Then we once again restore y to be independent, which in the linear CV case will not cause any technical problems. We can easily calculate it, provided linear marginal returns and costs. Otherwise we get non-existence results according to proposition 1. Now, clearly this is nonsense that we have derived by ignoring the implicit assumptions we use in setting up the formal model. Reaction functions are parametrical ex ante constructs, whether they include CVs or not and should not be interpreted as being ex post optimal anywhere out of equilibrium.

Proposition 2 is based on the paradox involved in the concept of consistent conjectures and should not be taken seriously as more than an illustration of the confused state of mind that the concept of "local consistency" entails. On the one hand, if conjectures correctly predict optimal reactions then marginal profit becomes independent of the rival's output as we then can determine this output by varying our own. On the other hand the concept of a reaction function becomes void as no reactions will ever take place. The perceived profit function will match the actual one only as the levels of the decision variables are correctly predicted. If the equilibrium is unique that takes place only in one point. That point is the only point where both parties can be right about the others output at the same time.

Consistency then amounts to nothing more than a complicated way to achieve consensus regarding what equilibrium outputs should be in a way that bypasses economic rationality. Formally this may sometimes be hidden by insufficient specification of the semantics of the formal model and/or by overlooking trivial possibilities. In the appendix it is shown how both Robson and Bresnahan achieve more complicated results by overlooking or assuming away the trivial Cournot solution.

The route most often taken to explain the concept of local consistency is by recourse to some story about how the rival's output consists of two parts: $y = a + f(x)$ where a is some autonomous part, that, to make sense, of course cannot be a constant of the function $f(x)$. In that case clearly a reaction function must include a. The conjectural variation can be said to be consistent if it equals $f'(x)$ and the level $f(x)$ is unknown.

On some reflection the approach of autonomous output seems to be rather empty. If a is truly exogenous to the model it should be constant in the model and hence a parameter that in general is computable to any firm in equilibrium with correct knowledge of the reaction. If on the other hand, *a* does vary in unpredictable ways or, to be more exact, in ways not predicted by $f(x)$, then it seems unreasonable to require consistent conjectures about something the agents will have no opportunity to observe, as they then by assumption have no way to tell whether a certain change in rival output depends on the unpredictable autonomous variation or is a reaction on their own output changes.

2.4 Conclusions

To sum up, consistency requirements in the Fellner sense are logically troublesome unless we weaken the concept so much as to make firms "right for not quite so wrong reasons". Unique consistent conjectural equilibria can only be achieved in a very special case of linear first order conditions and very peculiar informational assumptions. If the latter are dropped we immediately arrive at the paradox of "consistent" Cournot conjectures.

Fellner's phrase is certainly very catchy but my conclusion is that there really is nothing inconsistent about a model where agents entertain faulty conjectures as they slip out of equilibrium. On the contrary consistency requires that mistakes are made and suboptimal behaviour takes place in order to make it plausible that agents ever deviate from an established equilibrium.

The consistency debate then points to the conclusion that the traditional conjectural variation model is as useful as any of the more sophisticated attempts to make static oligopoly models dynamically consistent. It seems warranted though to point out that the conjectural variation should be viewed as a parameter for expectations of dynamic effects not explicitly modelled or as a measure of the deviation from some common standard like the Cournot behaviour and not as an explicit expectation of optimal reactions under perfect foresight assumptions. Used in the latter way conjectural variation models could be useful as empirical tools. The conjectural variation can easily be related to traditional indices of market concentration and market power like the Herfindahl index and the Lerner index (cf. Kamien and Schwartz(1983), and Waterson(1984), and for examples of empirical studies see Iwata(1974) and Gollop and Roberts(1979) and more recently Conrad(1989)).

As for the enrichment of economic theory the consistency debate could contribute to the final disposal of the reaction interpretation of the conjectural variation models. To regard the derivative of the rival's decision variable as your own expectation of reactions may seem a very natural and innocent proposition. The essence of strategic interaction captured in a very simple way, the process of convergence to equilibrium in a diagram is easily sketched on a blackboard. A closer look however reveals the logical trap thus set up. The assumption really is that you try to maximize your own profit by fixing both your own and your rival's output. As he no doubt will try the same the attempt will often result in an indeterminate situation where the problem of how to arrive at a mutually acceptable equilibrium still remains.

The content of the preceding paragraph is certainly not any new discovery. Pareto(1911) remarked on it as a problem of overdetermination in a note on the Cournot model and Stackelberg(1934) regarded the same problem as an inescapable instability inherent in oligopoly markets. One reason the reaction interpretation has had great intuitive appeal is no doubt that it corresponds closely to our feeling for real world economics. But the time structure of real world economics is not even close to that of conjectural variation models. The latter presumes decisions are taken in turns or coordinated in the same moment, a situation typical of many types of board games. In the real world economics the perceived costs and revenues of decision making will determine the timing of decisions and the timing of other peoples' decisions is but one factor in that complex. The relevant game analogies are closer to football or wrestling where time has a continuous structure which is not possible to flatten out into a static model because of both physical and economic restrictions on the possible speed of actions. To put it simply, conjectural variation models involve no time costs.

Though conjectural variations conventionally has been taken to signify some kind of expectations of reactions they might just as well be interpreted as market signals. Signals may be inviting rivals to collusion or trying to persuade them that a threatened action is plausible. Holt(1985) has performed some experimental gaming tests of the consistent conjectures hypothesis. The reactions of the participants seem in some respects very much favour a signalling hypothesis in contrast to a hypothesis of forestalling reactions. We have no firm theoretical basis to distinguish a priori between behaviour signalling intentions and behaviour trying to anticipate reactions. The market signals hence become ambiguous to interpret for the agents.

Finally the consistency debate focusses our attention on the fact that assumptions about full information, perfect foresight or other very demanding information requirements may introduce logical problems in the models. As informational assumptions often are implicit or made in passing this is a reminder that some care must be taken to assure that such assumptions are consistent with the model and the interpretations we wish to make. In the consistent conjectural variation models the agent is supposed to be able to outguess the other until finally equilibrium is reached when there is no point in trying to outwit the other any more. The embarrassing thing then is that the equilibria found to be consistent in the literature more often than not are such that it would pay off for the agent to discard this outguessing altogether and be a simplistic Cournot agent instead. By making the information set of each agent include that of all others it will by symmetry include itself and we end up making the decision to be taken contingent on that same decision.

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Appendix: Some comments on Robson and Bresnahan

Robson extends Bresnahan's uniqueness result for constant CCVs in the class of polynomials to the class of analytic functions. In the process he also proves that a model with a quadratic term in the price function has no constant CCV, in fact not even an analytic one. In reply to this Bresnahan makes the reformulation of the consistency condition in terms of a prescribed change of variables (described in section 2.3).

Now there are some peculiarities in both these short notes which I wish to point out without too much elaboration.

(*i*) In the notation previously used in this paper Robson arrives at a first order maximizing condition for the firm

(A1) $$a - (2 + b + \mathrm{CV}_x)x - cx^2 - y = 0$$

which implicitly defines $x^*(y)$. Now Robson rewrites the equation

(A2) $$x \cdot \mathrm{CV}_x = a - (2 + b)x - cx^2 - (x^*)^{-1}(x)$$

and finally by consistency $\mathrm{CV}_x = y^{*\prime}(x)$ and imposing symmetry, $z(\cdot) = x^*(\cdot) = y^*(\cdot)$ and $t = x = y$, he arrives at the expression

(A3) $$tz'(t) = a - (2 + b)t - ct^2 - z^{-1}(t)$$

which is the central expression used to prove the results.

The validity of (A2), specifically $(x^*)^{-1}(x) = y$, in a neighbourhood of equilibrium is based on implicit differentiation of (A1) with respect to y to yield a contradiction if $x^{*\prime}(y) = 0$. Let $x = x^*(y)$ in (A1) and differentiate with respect to y. If $x^{*\prime}(y) = 0$ for some y in the neighbourhood and by symmetry $CV_x = 0 = y^{*\prime}(x)$ then we get the contradiction $-1 = 0$.

This depends on the assumption that firm X regards the output quantity of the rival as independent when evaluating his first order conditions but dependent when he derives the expression for those conditions. If it was not so then no contradiction can be derived because the last term on the right hand side of (A1) would then be $f'(x)\frac{dx}{dy}$ and not -1 as the contradiction result presumes.

(*ii*) In trying to amend for Robsons non-existence proof Bresnahan provides a change of variables which yields consistent conjectures in the above treated extended meaning in Robson's model with a quadratic term in the price function. The change of variables is given as

$$x(a,b) = f(a) + g(b) \text{ and } y(a,b) = f(b) + g(a)$$

where g and f are functions satisfying

$$\text{(A4)} \qquad cf^2(t) - \lambda f(t) = K_1 - g(t) \ \forall t$$
$$cg^2(t) + (\lambda + 2 + b)g(t) = K_2 - f(t) \ \forall t$$

where λ, K_1 and K_2 are arbitrary constants. In the case of linear demand ($c = 0$) this will be satisfied by the consistent conjectures derived from Bresnahan's earlier approach and also in Robson's approach. Now (A4) can be satisfied in the linear case also by constant functions implying zero conjectural variation to be consistent in this change of variables meaning. By letting λ take on the special value of Bresnahan's CCV in this case and letting $K_1 = -\lambda K_2$ any functions will satisfy (A4) that satisfy $g'(t)/f'(t) = \lambda$. But nevertheless the trivial Cournot solution will do equally well.

Chapter 3. Lessons from learning to have Rational Expectations

3.1 Introduction

When will economic agents learn enough about their economic environment to end up in a rational expectations equilibrium(REE)? That question has been the focus of much theoretical work since the end of the 70s. It is still a rapidly evolving research field that is hard to summarize and unify. Lacking the competence to do so, it may still be worthwhile to attempt a presentation of some important papers and results and how they seem to fit in with one another. The selection presented here is not exhaustive and the significance of the results is still a matter of controversy. Therefore the aim is not so much to evaluate but to point out common trends and divergences in the research. Before doing so, some general comments on rational expectations and the learning issue may serve to give a broader perspective in which to fit the models of RE learning.

The concept of rational expectations has become familiar to all economists over the last two decades. No doubt there is considerable intuitive appeal in the idea that systematic deviations in expectations from outcomes ought to be corrected. Any rational economic agent would be expected to at least try learning from observations in order to correct mistakes in forecasts. And it could well be argued that those who fail to do so will be disadvantaged and perish in the economic competition. There are, however, several difficulties with these arguments.

One of these is the main theme of this paper, viz. when and how aggregate information can be used to learn the environment of economic action good enough to support expectations on the future that do not deviate systematically from outcomes. To delimit the scope of this theme it may be useful to start by noting three difficulties that the literature about RE learning mostly do not consider.

One is that it is hard to imagine actual economic agents really forming the rational expectations belonging to a model they have never heard of. Interestingly John Muth(1961) in his original paper actually motivates RE by the empirical observation that agents often seem to anticipate changes in key variables better or as good as predictions from economic models. As Arrow(1978) remarks, that might very well be because agents have access to more relevant information than the economist, which is a rather less than convincing argument to assume that their expectations are consistent with economic models based on considerably less or at least different information. Muth's argument then is that the modeller should anyway assume predictions to be the best possible within the model framework.[1] But then, of course, the Lucas critique apply, since even rational expectations in that case is a parametrically reduced model of the underlying structural expectations and information structure. This is hardly mentioned in the RE learning literature.

The survival argument for rational expectations suffers from another difficulty, as pointed out by Richard Day(1990), viz. that the adaptive success of an economic agent is not equivalent to economic success, since the optimal satisfaction of an agents desires given his means is not the same thing as optimizing the survival in a given environment.[2] Although exit and entry of learning agents clearly are interesting features of learning models, this issue has not yet been treated in the context of RE learning.

A third difficulty is the costs associated with expectations formation. As Radner(1982) remarks, such information costs would be likely to introduce non-convexities in choice sets, due to fixed set-up costs and dependence of the production set on the informational structure. Such costs are mostly neglected in RE learning models[3] but there are some recent papers that indicate that this difficulty may receive more attention in future research.

The above problems as well as others not mentioned here may well give rise to scepticism about the realism in the RE hypothesis. We discontinue the list here, although it would certainly be possible to go on. But, whatever the arguments are to question the empirical relevance of the rational expectations hypothesis, it might still be a useful theoretical device when we want to compress exceedingly complex real individual behaviour into theoretical representatives. It is then a long tradition in economics that a more or less reasonable adjustment process should be assumed and use the stability of that process as a correspondence principle. The conditions that are necessary for a learning process to converge to an REE serve to weed out models where the equilibrium is unstable in the sense that a perturbation of the equilibrium will lead agents to revise their expectations in such a way that the REE cannot be reestablished. Such stability also provides a criterion by which the number of REEs can be cut down when there are several. Models of the learning of REs are, I think, generally intended as a Samuelsonian correspondence principle rather than as attempts to describe how economic agents really learn. For the

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[1] A historical remark here is that the optimal properties of some rational predictors used by Muth actually was investigated already by Herman Wold in his doctoral thesis of 1938, cf. H. Lang(1989).

[2] Mark E. Schaffer(1989) shows that explicit modelling of firm competition by evolutionary games may lead to the fittest survivors being not, as economists would expect, the profit maximizers. That is except in the case when firms lack all market power, i.e. do not influence the profit of each other. Not even optimizing relative profits needs be a viable survival strategy.

[3] Cf. Kirman(1983), Evans and Ramey(1988) and Bala and Kiefer(1990) for some exceptions.

latter purpose other models of learning based on psychological research and purely adaptive algorithms are no doubt a better choice.[4]

In the last few years a unified approach of analyzing the learning process in RE models based on methods from the theory of adaptive control (Ljung(1977), Ljung and Söderström(1983)) have been developed. These methods are considerably more adapted to the problem than the martingale theorems used initially to establish convergence results. Formidable difficulties in the technical tractability of the learning problem have been overcome and substantial progress made in the understanding of how and when such processes converge to REEs. That is in itself an important feat regardless of the still remaining difficulties in interpreting the diverse results of such learning models.

The literature on RE learning is often classified according to whether the agents are fully rational or only boundedly rational, following an article in Journal of Economic Theory, 1982, by Blume, Bray and Easley. Fully rational learning takes place when agents know the model specification well enough to learn by estimating the parameter values consistently. Essentially the whole model specification is known, excepting only a few parameter values. Since this begs the question how the model specification came to be known – not only its fundamental equilibrium form but also including the updating procedures used by other agents – Bray and Kreps(1987) has evaluated this approach as a "sterile benchmark". The concept of bounded rationality has therefore become widely accepted as a more fruitful approach for this kind of models. Most of the papers, although not all, referred to in section 3.2 below concern learning based on some form of bounded rationality. The agents are assumed to have some reasonable initial belief about the model but lack information needed to guarantee consistent estimation. It seems that most researchers try to narrow down their discussion to assumptions restricting agents to use commonly accepted econometric estimation methods in their learning. But there are many variations and an important recent paper prefer the computability concept from computer theory as a criterion for learnability instead of the convergence of estimation procedures. More general search models incorporating experimental learning strategies have also been used lately to investigate the RE learning issue.

From the research on RE learning it is clear that assumptions on initial information sets and the procedures of learning that agents use is very important. That is only to be expected, but not only are convergence properties dependent on these assumptions, the REE outcome itself is contingent on the

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[4] Cf. e.g. Day(1975,1990), Cross(1983), Baumol and Quandt(1964), Cyert and March(1963) among many others for discussion and references.

informational assumptions.[5] It is remarkable how very simple, not to say primitive, learning rules often converge under fairly reasonable conditions to REEs conditional on limited amounts of information which sometimes may even be irrelevant. Although it has to be admitted that there are plenty of non-convergence results as well. On the bright side it should be noted that the use of learning stability as a selection criterion to weed out sunspots and bubble equilibria from commonly used macro models has been at least partly successful. One problematic aspect is, that it remains an open and hardly researched question how to find any generally applicable selection criteria among the multitude of information assumptions associated with distinct REEs.

Published results so far seem to support the boundedly rational learning as a reasonable correspondence principle for maintaining the RE hypothesis in a limited set of model types, in general stationary models where information assumptions are highly stylized. Since that set includes many commonly used macroeconomic models and hence, if the methodology based on the correspondence principle is accepted, means that the attempts to give credibility to the RE hypothesis by learning arguments have met with at least some success. On the other hand the identifying restrictions provided by rational expectations is derived from the informational structure imposed on the models rather than from any rationality *per se*. This is a feature of RE models that in general does not directly hit the eye, but which becomes very obvious as it is emphasized by learning models. Thereby the *ad hoc* character of these assumptions comes into focus.

How should rational agents choose their learning procedures when they lack the information and capabilities necessary to make a fully rational choice? Learning necessarily means committing errors and correcting them. Hence the optimal procedure will depend on how costly errors are and how easily they can be corrected. That, however, is information only available in a precise form after learning has taken place. Agents then have to form conjectures based on insufficient information. This opens the possibility that they may get stuck on non-rational equilibria in the learning process.[6]

In applying the RE hypothesis to real developing economies where non-stationarity and insufficient or even false information is common and totally unexpected economic events take place, learning is considerably more

[5] If the REE is fully revealing in Radner's(1982) sense that all private information can be nferred from market outcomes, it would seem the REE is independent of informational assumpions, and in some sense that is so, since all sources of information postulated in the model have oeen exhausted. However, it still is the case that the REE is dependent on assumptions about what the set of total private information is and the scope for active information production.

[6] Alan Kirman(1983) provides an example of how this may lead to indeterminacy even in very simple duopoly models. Frank Hahn(1977, 1978) has explored the issue of more general conjectural equilibrium models. Two-armed bandit models provide further examples, cf. section 3.2.5.

complex. In "experimentally organized economies" (Eliasson(1989a)), the learning process itself becomes more important than any (temporary) convergence point. In such a setting, where information is scarce and localized and behaviour is experimental and testing rather than optimizing, the path followed by the economy will depend more on the dynamics of the learning process than on any characteristics of a long run REE of the economy.

In a modern economy where information and information handling is of primary importance[7] this indicates that the conclusions derived from the rational expectations hypothesis may be very sensitive to implicit or explicit information assumptions. While adaptive learning in general is centered directly on goal achievement and treats the environment essentially as a black box, the rational expectations learning in contrast aims at specifying the framework within which to optimize. However, treating the parameters of the relevant framework as the facts to be learned, this is an adaptive learning process and subject to all the problems of such processes when the underlying structure changes while the learning is still going on. Being cautious about the applicability and interpretation of RE models in real economic contexts should however not prevent us from learning the lessons of theoretical research on RE learning. There can be little doubt that this research has provided a richer and deeper insight in the workings of expectations in economic modelling.

Of course, space limits as well as subtle shifts of meaning buried in different usages of terminology prevent any really deep probing of the problems of RE learning in the context of a short overview like this. The next section, which also is the main part of this paper, will describe and organize some important parts of the literature on learning about rational expectations as well as try to substantiate some of the assertions made above. As will be seen the adaptive – or even adoptive, in the sense of Alchian(1950) – character of these learning processes is a prominent feature. In the third and concluding section some tentative connections are made between the literature surveyed and more general adaptive models as well as game theoretical concepts. I have chosen to avoid formalization on the whole, with one trivial exception, in order to avoid squeezing slightly disparate equilibrium and learning definitions into any common framework, which still awaits general agreement.

[7] According to G. Eliasson(1989b) some 60 percent or more of total labour input in Swedish firms are devoted to some kind of information handling.

3.2 Learning rational expectations

This main section will be subdivided into seven subsections. The first will discuss the distinction between fully rational and boundedly rational learning. The second will review some results concerning econometric learning algorithms. The third subsection reviews papers concentrated on the issue of econometric learning stability as a selection criterion among multiple REEs. The fourth subsection treats work on general learning stability in a temporary equilibrium framework. The fifth cursorily reviews a more diverse collection of papers. These diverge from the papers in the earlier subsections in assumptions about state spaces and learning algorithms in ways that is hard to generalize under any common heading. The sixth subsection treats some computability results that differ considerably in spirit from the main trend within the RE learning literature. The seventh and final subsection contains a summary and some tentative and partial conclusions.

3.2.1 Fully rational versus boundedly rational learning

The distinction between fully and boundedly rational learning commonly used in the literature is based on Blume, Bray and Easley(1982). It will be used here in a not quite equivalent form.

Fully rational learning means that agents know the model specification well enough to learn by estimating the parameter values consistently. More exactly, when they estimate they use likelihood functions that are correct specifications, conditional on available information, for data generated by stochastic processes where agents do use these likelihood functions. Townsend(1978) introduces an early model of fully rational learning by a Nash equilibrium concept.The points of convergence of the learning process may be considered as Nash equilibria in learning strategies, where each agent's market model specification, including parameter values, is the correct one for the market information generated *ex post*, if he and everybody else use this specification. The main point in fully rational learning is that agents learn this correct specification by using correctly specified estimation models.

Under rather mild regularity assumptions these models will converge to an REE where the expectations of the agents will be verified by the outcomes, apart from some residual stochastic noise, from which the learning methods of the agents can extract no further information. Bayesian learning will in general converge given some coordinating common knowledge assumption. Mark Feldman(1987a) shows this for the case with homogeneous beliefs where the distribution of equilibrium outcomes is assumed to be a continuous function of forecasts and homogeneity of beliefs is common knowledge and

Feldman(1987b) proves it for heterogeneous beliefs in a partial equilibrium model based on Townsend(1978), who conjectured this. Margaret Bray and David Kreps(1987) show similar results for somewhat more general information assumptions, but they emphasize that problems of convergence to the REE can still persist even if beliefs converge, if there are multiple market equilibria of the basic model. The problem of coordinating beliefs to one specific equilibrium is by no means trivial, cf. Crawford and Haller(1990). Blume and Easley(1984) investigates Bayesian learning in a model where some agents update beliefs over a common finite set of probability measures and some agents are fully informed. The updating is recursive, i.e. current observations do not enter in current beliefs. The structural model can be learned if the estimated parameters are sufficient to identify the structure. It bears stressing that all of these models rely on a non-trivial common choice of prior beliefs. Bray and Kreps characterize this as learning *within* a grand rational expectations equilibrium.

Townsend(1983b), as many others, argues that rational learning must incorporate some assumption of common knowledge on some level. It may be with regard to forecast functions used, or updating procedures or on some deeper level. But it is needed to truncate the infinite regress involved in the structure of models where agents try to forecast the forecasts of others. I.e. on some level consensus has to be imposed on behaviour to make it determinate. Blume and Easley(1984) compare this to Harsanyi's(1967) Bayesian games where it is presumed that the space of player types is common knowledge. However, assuming agents to agree on essential features of the model seems no good starting point for an answer to the question how they came to learn this model. The strong dependence on common knowledge assumptions is not unique to this brand of modelling. It becomes very explicit though since it is common knowledge of other agents learning behaviour that is assumed. The question naturally arises: How was that common knowledge established? It could hardly be inferred from market signals *before* the parameters were learned. Michael Bacharach(1989) argues emphatically that such learning should not even be called rational, because with that much information available, optimizing agents do not act consistently if they converge to the REE fixed point of the model. They ought instead to take advantage of their knowledge to act strategically when they know that parameters depend on their actions. Of course, this is less of a problem when the set of agents is large, but then again: assumptions of common knowledge also become rather less attractive as the number of independent agents with incomplete information increases.

But fully rational learning can be useful in other ways than as approximations to real markets. Xavier Vives(1990) uses a signalling model of fully rational Bayesian learning to characterize the speed of learning when information is asymmetrically distributed. Convergence speeds turn out to depend crucially on the precision of private information. These results can then be extended to somewhat less rational learning. Using fully rational learning as a modelling tool in order to get a handle on questions in more general models may be a more fruitful way of exploiting this particular line of research than as a correspondence principle for RE models.

The unattractive prior information assumptions of fully rational models have made the concept of bounded rationality more widely accepted as a starting point for models of RE learning. Most of the papers referred to below concerns learning based on some form of bounded rationality. Bounded rationality here can mean just about anything that is not fully rational. The common denominator is only that there is something in the model that prevents agents from being fully rational in a well defined sense. It may be lack of information on the model that necessitates the use of more or less misspecified learning techniques, e.g. Bray(1982). It may be that the set of models to choose from is too restricted, e.g. Blume and Easley(1982), or it could be that only local information is available for some variables, e.g. Frydman(1982). Heterogeneity in information sets and instrumental variables regression is another example where learning must be considered boundedly rational, like in e.g. Fourgeaud, Gourieroux and Pradel (1986). Restrictions on the calculating abilities of agents is another possibility, e.g. Spear(1989). In fact it is one of the main difficulties of a theory of bounded rationality that it can take so many different forms. The classification used in this paper is somewhat broader than the one used in e.g. Blume, Bray and Easley(1982) where Frydman(1982) i.a. have been classified as a fully rational model. I have not considered knowledge of the "true" model as a sufficient condition for fully rational learning if the ability of agents to make full use of that knowledge is restricted by e.g. in Frydman's case limited information on the realizations of the model.

The boundaries between full and bounded rationality models in the literature are necessarily somewhat fuzzy, due to subtle differences in definitions and approaches. For example, J.S. Jordan(1985) defines the REE somewhat unconventionally as the outcome of a kind of informational tâtonnement process. Thereby the learning process can be dealt with recursively, avoiding the problematic simultaneity in expectations and price determination of the conventional formulation. The expectations of the agents therefore need not be conditioned on the expectations of other agents. Within this REE definition learning could then be considered fully rational. However, the definition as such prevent agents from trying to predict how other agents change their expectations, thereby placing a bound on their rationality. The main feature of boundedly rational learning models in general is that agents are supposed to use in some sense misspecified estimation models due to lack of information.

3.2.2 Econometrically based learning

The mainstream of the RE learning literature concerns learning of parameters in linear models by means of least squares regression or, occasionally, Bayesian estimation. The early contributions in this area – e.g. DeCanio(1979), Bray(1982), Bray and Savin(1986) and Frydman(1982) – used particular models, like cobweb and asset trading models, and proved convergence of learning within some limited range of structural parameter values. Techniques of proof were often complicated and specially tailored to the specific market at hand.

The stability of learning processes with misspecified models often depends critically on parameter values, and one of the recent developments in the area is the application of a general method from control theory to determine such stable parameter values in relatively more simple ways. This method was developed by L. Ljung(1977) and L. Ljung and T. Söderström(1983) for use in recursive estimation for adaptive control. The first published application, to my knowledge, of this theory to the RE learning problem is a short note by D. Margaritis(1987) analyzing the Bray(1982) model. But it was two papers by Albert Marcet and Thomas J. Sargent(1989a,1989b) that were widely circulated before publishing that adapted the technique to the RE learning problem in general terms and introduced it into the mainstream of research. Marcet and Sargent(1989a) applies the Ljung theorems to the learning problem of self-referential REE models (in the sense that the actual law of motion depends on the perceived law of motion), exemplifying the approach on the models of Bray(1983) and Bray and Savin(1986) as well as a model used by Fourgeaud, Gourieroux and Pradel(1986).

In the Marcet and Sargent(1989a) paper the essentials are outlined for learning processes where information is symmetrically distributed and encompasses all state variables. The behaviour of the system of the stochastic difference equations arising from a linear learning rule may under certain general, but rather messy, regularity conditions be inferred from the behaviour of associated ordinary differential equations. The perceived behaviour (summarized by a parameter vector, β) induces the real behaviour of the system by the mapping $T(\beta)$ back into the parameter space. Local stability of the stationary point of the associated differential equation system

$$d\beta/dt = T(\beta) - \beta \quad (2.1)$$

then implies local convergence with probability one of the corresponding least squares learning process based on β. Global convergence properties can be inferred from analysis of a larger differential equations system that incorporates changes in the updating procedure for β. More general updating procedures than ordinary least squares can thus be analyzed and fit into this framework. In order to guarantee almost sure convergence some rather complicated boundedness conditions must be fulfilled. A drawback of the technique is that these may sometimes be difficult to verify. To prevent the updating procedure from going outside the verifiable attraction areas a projection facility is often needed that prevents outliers from throwing the estimation out of bounds. Essentially it is an assumption that learning agents throw away certain outlier observations but the projection facility may be a little more sophisticated in order to extract at least some information from the discarded observation.

In Marcet and Sargent(1989b) the approach is extended to models with hidden state variables and asymmetric information like those in e.g. Bray(1982) and Frydman(1982). In models with these characteristics it may be very complicated to compute the actual REE state. Either numerical solution of the associated differential equation governing convergence or simulation of the least squares learning model then offers alternative ways of computing REE values. Marcet and Sargent(1988) summarizes the arguments for this.

When there are hidden state variables and/or private information the approach described above must be modified. The associated differential equation governing local convergence is changed to

(2.2) $$d\beta/dt = S(\beta) - \beta$$

where $S(\beta)$ is a composition of the mapping $T(\beta)$ in (2.1) with certain partitions of the covariance matrix of the system, the partitions depending on which variables are hidden or private. Given that $T(\cdot)$ has its eigenvalues in the unit disc, and the system otherwise is well defined, the mapping $S(\cdot)$ too will be well defined. Under regularity assumptions similar to the above it can then be used much in the same way as $T(\cdot)$ when there are no hidden variables. Apart from the above papers Sargent(1991) applies the apparatus to a more general class of models proposed by Townsend(1983a).

This approach gives a unifying and more general framework for analyzing and comparing convergence properties of a great variety, though not all, of the different learning processes in the literature. The differential equation approach parallells and confirms stability results by Evans and Honkapohja in a series of papers described in the next subsection. The rest of this subsection will be devoted to a more detailed, but still sketchy, verbal description of some important and often cited papers within the mainstream of econometric RE learning literature. Papers focussed on stability issues are deferred to the next subsection.

Margaret Bray (1982) uses an asset market model (based on an infinitely repeated version of the model used in the Grossman and Stiglitz(1980) paper on efficient markets) with two classes of traders, one informed and one uninformed. The informed traders act on rational expectations[8] all along while the uninformed forecast asset return from current price on the basis of OLS-regressions on past observations of the relation. The underlying stochastic process consists of the variables: information private to the informed traders, the asset returns and supply; that form a sequence of independent identically distributed multivariate normal random variables.

Two different learning rules are investigated. In the first uninformed traders regress asset returns on price, while they are using an initial conjecture as forecast. Trade then takes place at market-clearing prices. When the estimates have converged to a probability limit all uninformed traders simultaneously shift their forecasts to this limit estimate. Continuing in that way by periodic revisions they will eventually arrive at rational expectations, provided the ratio of informed to uninformed demand is high enough to dominate price effects on asset returns. The degree of coordination in the switch required by agents in such two-stage learning processes is rather formidable.

The second and considerably more complex but also more realistic case is when agents are assumed to update their forecasts every time a new data item is reached. By the assumption that uninformed traders know the means of prices and returns, it can be shown that expectations also in this case will converge to an REE under a similar but less stringent condition as the one in the first case. This recourse to an assumption effectively meaning that average forecasts are known at the time of forecasting seems rather artificial and circular. But Marcet and Sargent(1987), using the differential equations approach, confirm the result without this assumption.

Margaret Bray and N.E. Savin (1986) use a cobweb-model with a continuum of agents learning by Bayesian methods, of which OLS is a special case. The basic stochastic process is a sequence of independent, identically distributed random variables, one of which is unobservable at decision time while the others are assumed observable. This learning process converges to a stable REE with probability one provided some economically reasonable conditions, essentially to ensure that the supply curve cuts the demand curve from below. Maybe the more significant result is that it can be shown that the probability is zero for convergence to any non-rational equilibria. This is a property the authors feel should hold for every reasonable learning process,[9] since estimation methods give consistent estimates if the data are generated by a stationary model. The cobweb model is stationary when expectations have converged and hence non-rational equilibrium expectations ought to be ruled out. However, the authors claim that convergence can be proved for models

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[8] In the sense of correct conditional expectations given their private information.

[9] Cf. section 3.2.4 and the Blume and Easley(1982) model for examples where this does not hold.

with non-stationary stochastic processes, too, (p. 1137) although they then need stronger conditions on permissible parameter values. A. Marcet and T. J. Sargent(1989a) also show the assumption of independently identically distributed variables to be unnecessarily restrictive in this case.

Just as in the preceding paper agents estimate a standard linear model that obviously is misspecified during the learning phase. Therefore the main theme of this paper is simulation experiments trying to determine how fast convergence of the learning process must be to prevent Bayesian agents from spotting the misspecification of the learning model in relation to the data generating process by standard statistical tests. The closer parameter values are to unstable regions, and the more initial confidence agents have in an incorrect prior conjecture, the more probable such spotting of misspecification becomes, because learning will take place at a slower pace. I.e. if the slopes of demand and supply schedules are sufficiently separated and the agents open-minded about their initial guesses, especially if they use ordinary least squares estimation, learning will take place at a rate that makes it very difficult to obtain a significant misspecification test. What will happen if the misspecification nevertheless is spotted by a test is not clear because there is no precise econometric rules available when it comes to correcting the model specification. I.e. there is no formal rule available to determine rational action in this case.

Roman Frydman (1982) explores another aspect of bounded rationality in information handling. The model is a product market where the agents, on the supply side, are equipped with correct model specifications of the demand and supply structure but lack information on parameters. Cost information is only locally available. A trader will know the market price and the stochastic realizations of his own cost function but not the realizations of other traders' cost functions. Then, to be able to form optimal forecasts in the minimum mean square error sense, they will need information on the "average opinion", i.e. they need to know the average of other agents' forecasts up to a white noise disturbance. If some institution external to the market provides such information, rational learning estimation of parameters may converge to REE. Frydman distinguishes two cases. One where forecasts are modified by the information before supplies are finally determined. The information thus relates to a preliminary average opinion. In this case the probability is strictly positive that convergence of forecasts *does not* take place even if a very large number of updating rounds are allowed to take place and divergence of opinions take place with probability one if demand is sufficiently inelastic. In the other case supplies are determined before information on average opinion is received. Then model parameters can be consistently estimated with *ex post* information on average opinion. If all firms forecast price in the next period by using the rule that the current parameter estimates are substituted into the relation that holds between parameters and price in the REE, then prices will converge to the REE price. Frydman stresses that such a consensus-rule would in general be suboptimal for the individual to use.

Marcet and Sargent(1987) analyze this model using a learning procedure that disregard the forecasts of other agents and obtain a strong global convergence result. This is corroborating the conclusion of Evans and Honkapohja(1990b) and Grandmont and Laroque(1990) in another context that "oversophisticated" agents that try to be more rational than data allow them to be, may disturb the stability of RE learning.

Fourgeaud, Gourieroux and Pradel(1986) emphasize that no prior knowledge of the model ought to be assumed. Therefore they model the forecast procedure as a regression on a predetermined set of instrumental variables that may or may not be included among the exogenous variables of the structural model. They show that such an automatic forecast procedure converges to REE in a cobweb model under assumptions of stationary regression coefficients and some mild regularity in the asymptotic behaviour of the instrumental variables chosen for prediction. The same holds for a Cagan model of hyperinflation and hence for a model including expectations on future values of the endogenous variable. The surprising feature is that convergence holds independently of how strong the correlation is between instruments and exogenous variables. Hence an essentially *ad hoc* regression will yield rational expectations in the long run. However, the rate of convergence will depend on the choice of instruments as well as how close parameters are to the stability boundaries of the model. Furthermore the REE will depend on this choice of information set. Different choices of instrumental variables will in general result in different REEs, as the conditional expectations will be dependent on the information sets.

In a conference volume from 1983 (edited by Frydman and Phelps) several contributions are centered around the average opinion problem and how to handle expectations on expectations. Edmund Phelps and J. C. Di Tata discuss short run effects arising from assuming that the individual agent does not believe that the average opinion is the same as his own expectation. George Evans shows how different sets of initial conjectures about the average opinion by learning from experience may lead to different individual rational expectations and outcomes. The REEs of this model may be interpreted as Nash equilibria in strategies dependent on these initial conjectures and the learning rules used. Expectations of others' expectations necessarily entails strategic considerations. Then knowledge of the fundamental model parameters is not enough to guarantee the stability of an REE. It is also necessary that collective expectations of expectations converge when they are updated out of equilibrium. Evans shows how such processes may be unstable in a Goodwin business cycle model as well as in a simple macro model where static expectations imply stationarity. Frydman derives conclusions resembling those of his 1982 paper in an island model of Lucas type.

Only a selection of results, that I find representative of the literature, have been mentioned here. In summary, econometric learning on basis of misspecified models converges probabilistically to a unique REE within certain ranges of structural parameter values in most of the studied models. When agents recognize that outcomes depend on the expectations of other agents problems arise, unless they are short-circuited by some common knowledge assumption. Those problems are very similar to the corresponding interaction in oligopolistic market models. Once agents recognize their strategic interdependence we are in a game situation where a much more sophisticated and detailed modelling is required giving considerably less general results. Whenever more than one fixed point of the mapping $T(\beta)$ exists the problem of choosing among the possible REEs arise. That is the theme of the next subsection.

3.2.3 Learning stability as selection criterion

R. Lucas(1986) in a wellknown article proposed that stability of learning processes should be used as a criterion to decide which of multiple REEs to choose as the fundamental one. This subsection reviews some central papers dealing with this issue.

Michael Woodford(1990) investigates if learning can be used as a selection criterion among multiple REEs, i.e. which, if any, equilibrium will a learning process converge to. His framework is an overlapping generations model with fiat money as the only asset where stationary sunspot equilibria can be shown to exist. Unlike most other authors his learning scheme is not based on least squares estimation. He uses a non-parametric adaptive learning rule, stochastic approximation, that is analyzed with the same technique from control theory that Marcet and Sargent use.

Though the results are somewhat complicated to describe, they clearly indicate that the REE that would obtain in the absence of sunspot beliefs may not be the one that learning processes converge to. If agents are willing to consider sunspot variables, uncorrelated with the predicted variable, as nevertheless influencing the outcome, then adaptive learning may lead to sunspot equilibria and the REE that most economists would regard as the fundamental one may even be unstable with respect to the dynamics induced by the learning rule. If sunspot equilibria exist there will generally be multiple locally stable equilibria, but it is not in general possible to determine by initial conditions which of these a particular realization of a stochastic learning process will tend to because the stochastic element means that domains of attraction need not be disjoint.

Woodford also points out that if agents have different choices of sunspot variables or, more realistically, weakly correlated exogenous variables, the situation becomes increasingly complex. Not only does the set of REEs multiply but stability results may be reversed for a former stable equilibrium point by the introduction of another sunspot variable believed to be possibly relevant by some significant fraction of the agent population. It seems clear then that learning processes per se cannot be relied on to single out a reasonable REE even if they do converge to some REE. However, as will be seen below, less ambitious targets may be accomplished by the use of learning processes.

George W. Evans(1985) uses a learning process similar to Bray(1982) as a "natural revision rule" for analyzing expectational stability (E-stability) of REEs in a general model where the current state of the model depends on last periods prediction of both the current and next periods state variables. In these models so called bubble equilibria may appear, i.e. REEs that are deemed as less fundamental than another in some sense. Evans mostly use the definition of bubbles advanced by Bennett McCallum(1983), viz. REEs that do not satisfy the minimum state variable criterion for selection of the fundamental REE. See Evans(1986) for a detailed exposition on the relation between the minimum state variable criterion and E-stability. Evans interest in the adjustment process is, like Woodfords, not primarily the learning aspect but the use of stability of learning as a selection criterion among multiple REEs. He finds the bubble equilibria to be robust with respect to small perturbations in the parameters of the expectation function used by agents for forecasting. Evans refers to this as weak E-stability. However, the bubbles can be shown to be unstable in a strong sense if the learning process admits the use of irrelevant lags of the state variables, i.e. lags which are not included in the bubble RE solution in question.

In Evans(1989) this is followed up to include analysis of stability against inclusion of sunspot variables, and it is shown that Woodfords(1990) stability results on sunspot variables, for one class of the utility functions involved, are not E-stable in this strong sense. Evans initially conjectured that rational bubbles in general should not be strongly E-stable i.e. locally stable to overparametrization. This conjecture, however, seems to be refuted by Evans and Honkapohja(1990c) in a paper analyzing solutions of a general linear model including expectations on future values, where they find that for some parameter values isolated bubble equilibria indeed are strongly E-stable with respect to inclusion of irrelevant lags in the expectation function. These parameter values are shown to be within reasonable economic bounds in a macro model with real balance effects used in the literature.

In a recent paper, Evans and Honkapohja(1990a), a general class of linear models is analyzed. The class is characterized by one endogenous lag and expectations extending to three future periods. Within this class it can be shown that continua of REEs are at best weakly E-stable and if current period information is used to form expectations, not even weakly E-stable. But the instability with respect to overparametrization is one-sided so there might be convergence for some initial conditions. A close connection is established between E-stability and convergence of adaptive learning algorithms in the differential equation approach used by Marcet and Sargent.

Strong E-stability hence shows some promise as a correspondence principle for RE models in the specific sense that there are at least in some cases adaptive learning algorithms that will converge to a unique strongly E-stable REE. Evans and Honkapohja are confident that the results can be extended to more general classes of models.

However, Woodford(1990), points out that his stability concept is related to but not equivalent to E-stability and in some cases yield different conclusions. He claims that even if no sunspot equilibria are strongly E-stable there are reasonable learning processes which will not converge to the solution commonly regarded as fundamental. This is of importance since it means that scope is left for a multiplicity of "fundamental" equilibria depending on assumptions about the learning procedure.

Evans and Honkapohja(1990b) extends E-stability results to some simple classes of non-linear models with and without stochastic disturbances. These models exhibit periodic solutions and have been studied in a more general deterministic context by i.a. Grandmont and Laroque, see below. The precise way that stochastic disturbances enter the model is shown to affect stability conditions. For isolated equilibria of the model it is proved that these equilibria alternate between E-stable and E-unstable solutions. Hence when there are several such equilibria E-stability would partition them into one stable and one unstable set of about equal size, give or take the odd one. The convergence results of DeCanio(1979) and Bray and Savin(1986) on the cobweb model are extended to the case where demand and supply may be non-linear. It turns out that the equilibrium can be rendered unstable for some parameter ranges if agents are considering periodic solutions to be possible. Hence, the earlier remark above about "oversophisticated" agents contributing to instability.

To conclude this section a short note by N. Gottfries(1985) could be mentioned. He uses a deterministic overlapping generation model with asymmetric information and a tâtonnement process of revisions of demand and supply before trade takes place. By the revision process private information can be disclosed and learned by others. It is found that only the unique stationary perfect foresight equilibrium may be stable, though it need not be.

Typically learning processes may be used to rule out equilibria by instability, but it seems more doubtful whether they really lend support to any specific equilibrium. Alternative reasonable learning processes may very well reverse stability results and thus call into question how "fundamental" a chosen equilibrium really is. Without a criterion that singles out some learning process as more reasonable than others multiplicity will remain a problem.

3.2.4 Temporary equilibrium stability

Most models described so far have started out by assuming some specific kind of learning process, Bayesian, least squares or (Woodford(1990)) stochastic approximation, and then trying to determine conditions when a more or less general model will converge to an REE. J.-M. Grandmont(1985), Grandmont and Laroque(1986,1988,1990) takes another route by trying to characterize the set of learning processes that are compatible with a stable temporary equilibrium in the neighbourhood of a perfect foresight equilibrium. This work is closely related to earlier contributions by Fuchs(1976,1977,1979a and 1979b).

In Grandmont(1985) the general dynamics of a non-stochastic overlapping generations model is discussed with only a small part discussing learning as a fixed function of a finite sample of past prices. He finds that stability of backward perfect foresight equilibria implies forward learning stability, but not the converse in general. This, at first sight, surprising connection between forward and backward dynamics has a natural explanation since the learning process uses backdated variables to predict the future. Grandmont and Laroque(1986) extend these results in a one-dimensional non-linear model and also give a class of expectation functions for which the converse also holds. Equilibria in these models may be periodic cycles and hence the expectation function itself must be able to "detect" the cycles, in fact forecasts must be able to detect cycles with period $2k$ to generate stability of a k cycle equilibrium. Grandmont and Laroque(1988) further extends this to a multidimensional framework where the temporary equilibrium may depend on lagged variables.

Grandmont and Laroque(1990) criticize the use of projection facilities in the multiple equilibria context. They deem it contrary to the spirit of enquiry, since it presumes that agents have some consensus on which domain of attraction to project estimates into. But how could such consensus arise before any learning have taken place? On the other hand it could be argued that in analyzing local stability one should presume that there is some previous experience providing coordination to the neighbourhood of the equilibrium.

Anyhow, Grandmont and Laroque use the same temporary equilibrium framework as in earlier papers finding the temporary equilibrium locally unstable for almost all initial conditions when the forecasting function is continuous and agents attach positive prior probability to the possibility of divergence. When the forecasting function is allowed to be discontinuous there are open sets of initial conditions that may result in convergence.

It remains somewhat obscure how this approach ties in with the mainstream of the literature. The assumption of a fixed memory bound makes comparisons with econometric learning models difficult, since it may considerably change equilibrium properties, as shown by Fourgeaud, Gourieroux and Pradel(1985). Evans and Honkapohja(1990b) make some connections to their own work, where the condition on the expectation function to detect higher order cycles is viewed as a condition for strong E-stability against overparametrization in the sense of allowing for longer period cycles.

3.2.5 Some other approaches to learning

Lawrence Blume and David Easley(1982) develop a learning model where each agent considers a finite set of possible models of the economy, and learns by updating a prior distribution over these models. The true joint signal becomes known *ex post* while the agents *ex ante* had knowledge only of their own contribution to the signal. They then learn according to the simple rule: increase the weight of the model if its prediction is better than average and vice versa. Blume and Easley conclude that an REE will be locally stable with this kind of learning. However, there are also non-rational expectations equilibria and even cycles that are locally stable. None of the admissible models include predictions of other agents' predictions and the set of economic models the agents choose from thus is too small to describe the full behaviour of the economy. Therefore some sets of data will induce model choices that are non-rational and still locally stable. Hence learning in this way may, but does not necessarily, lead to REE. Curiously, an extremely simplistic learning procedure actually does guarantee convergence to REE. If the agents use one point distributions and update by choosing randomly a new point distribution whenever a prediction fails, they almost surely converge to an REE. The authors reject this result, partly because it depends heavily on the finiteness of the model space, and partly because such behaviour has "no trace of rationality attached to it"(ibid. p. 350).

J. E. Foster and M. Frierman(1990) use the Blume and Easley(1982) model to investigate conditions of global stability of the RE learning process. They employ a graphical representation that makes the model considerably more transparent to intuition and shows that gross substitutability is a sufficient condition for the revealing REE to be globally stable under Bayesian learning. Gross substitutability in this context is conditioned on the state of the world and includes the effect on total demand that a price change induces by updating of beliefs. It is interesting that sufficient conditions for a unique REE stable under learning are analogous to the common Walrasian conditions for a static equilibrium. Essentially the gross substitutability condition requires that the adjustment of beliefs affect decisions relatively slowly in the sense that these effects do not dominate the ordinary income and substitution effects. Foster and Frierman points out that the stability conditions in the Bray and Savin(1986) cobweb model (described above) also are analogue to commonly stated stability conditions for the static model.

Another approach to the learning issue takes its departure in the wellknown "two-armed bandit" problem, where a gambler, choosing between two slot machines, one with known and the other with unknown pay-off probabilities, may with positive probability end up playing the machine with the lower pay-off probability for ever. Michael Rothschild(1974) has applied this to the price setting problem when demand is unknown and found the choice of final price to be undetermined. Nicholas M. Kiefer(1989) has worked out a variation on this to the case of a monopolist trying to establish which of two possible demand curves is the true one. In this case, too, the monopolist may get stuck on the wrong conclusion.

The key mechanism behind these results are that agents are supposed to optimize their learning behaviour, actively generating information. If an initial sequence of experiments leads to beliefs about expected payoff from continued experiments that are too low the agent will discontinue active learning. The idea is that learning entails a cost or at least a possible cost in terms of sacrificing short term profits in order to learn about the environment. Other papers in the same vein of thought are Easley and Kiefer(1988), using a more general model Kiefer and Nyarko(1989), analyze a similar setup restricted to linear models. Kiefer(1989) provides a summary and introduction to this area of learning models. Bala and Kiefer(1990) introduce investment in calculation abilities, e.g. computers, in the same type of models.

Conceptually similar but technically rather different is a paper by Evans and Ramey(1988) where explicit calculation costs and myopic agents induce non-rational equilibria for some parameter values and REEs for others. While the Kiefer *et alia* papers posit an agent actively seeking to generate information, Evans and Ramey keep to the mainstream paradigm of agents passively receiving market generated information but achieve much the same effect by making learning costly so that information may not be used even if it is available.

The papers treated above and in the preceding subsections are based on some adaptive learning mechanism and are only a sample from a thriving branch of the economic literature. There are several other papers in a similar vein. To mention a few other results and views in short: S. J. DeCanio(1979) concludes, in a simple cobweb model, that the existence of a rational forecasting function is no guarantee that agents will ever discover it. In a deterministic overlapping generations model G. Tillman(1985) concludes that self-fulfilling expectations equilibria exist and are stable only under homothetic preferences and small elasticities of substitution, when the model is rigorously derived from utility-maximizing behaviour. In a similar overlapping generation model J.-P. Benassy and M. C. Blad(1989) show that rational expectations will almost never be learned. Their learning process is, however, extremely simple. It uses only the second last observation at every updating. The argument for such simplistic behaviour is the great complexity arising because of non-linearities in the system governing dynamical behaviour of optimizing individuals.

It seems an open question whether the instability results by Benassy and Blad(1989) and Grandmont and Laroque(1990) may have something to do with the fixed memory length they use. Evans and Honkapohja(1990b) make a remark in that direction. A somewhat peripheral paper that may be relevant on this specific question is Gates, Rickard and Wilson(1977) which analyzes the adjustment process on oligopoly markets and finds that updating processes placing high weights on the most recent observations increases the risk for instability. Fixed memory learning as compared to accumulating memory learning, e.g. ordinary least squares, in the long run weights recent observations relatively more. However, Fuchs(1976) in a deterministic context finds that too high weighting of observations in the past decreases stability when the expectations function is fixed. Results on memory length and weighting schemes thus are rather context dependent and more general results on this seem to be lacking.

3.2.6 Computability and decidability

There are a few papers concerned not with convergence of adaptive learning but with the question: Is market information sufficient for agents to make the necessary calculations for a consistent updating? In this subsection two different approaches will be described.

A recent contribution by Stephen Spear(1989) imposes the constraint that any forecast function used must be computable by a finite algorithm. Using results from computer science he shows that with perfect information about the state space (which is finite) the rational expectations forecast function can be recursively identified in a two-stage learning process. Two-stage means that agents first collects observations of the outcome using a fixed forecast function and then switch to the identified function conditional on the old. Then they repeat the process until they eventually arrive at the fixed point. The process resembles that in Bray(1982). At least that is what happens if the functional mapping from forecast functions to price functions as well as the price functions themselves are primitive recursive, a not very restrictive requirement in practice. Lacking perfect information, however, the rational expectations function cannot be learned by inferring the functional mapping from forecast functions to price functions in the two-stage process because that would require knowledge of correspondences, i.e. multi-valued functions, that cannot be recursively calculated. The same obstacle arises if agents only try to determine whether they are using a fixed point function or not. This problem is in general undecidable because of an analogue in recursion theory to the Gödel theorem. When agents update the forecast function in every period Spear finds that even if it is assumed that they arrive at an equilibrium, where the forecast function is consistent with the forecast function selected by the updating procedure, it still cannot be determined whether this is an REE in the sense that it is the same as the true price function. More concretely, agents may receive information signals such that their updating of the forecast function cease to change it, but this forecast function may still differ from the price function of the economy. The point is that the agents are unable to tell the difference because they cannot calculate which updating procedures that converge to non-rational equilibria and which converge to REEs.

At first glance these results seem to contradict e.g. the Fourgeaud, Gourieroux and Pradel(1986) results where no knowledge of the fundamental state variables is presumed. But it should be noticed that these strong computability results really relate to the possibility for agents to completely specify how the economy transforms forecast functions into price functions within a fairly wide class of computable functions. Ordinarily it is only required that information signals from the model does not controvert the models used by agents for forecasting. That does not in general imply that the models used are identical with the theoretical model of the economy where agents use such forecasting models. Furthermore most of the models used in the learning literature have a linear structure as well as the learning rules used. This considerably limits the possibilities among which to learn. It remains to be seen what significance these computability results really have for the question whether learning agents end up in REE. The learning impossibility results in Spear's sense is actually a statement to the effect that there is no way for the agent to *decide* whether a model equilibrium is REE or not. From the standpoint of economic theory that seems less relevant than asking if the model used by the agent is consistent with the equilibrium information the economy will provide him with.

Jonathan Thomas(1989) provides a very simple and concrete, though rather non-economic example, of an economy with infinitely many REEs none of which are computable.

Mordecai Kurz(1989) takes a quite different angle on whether agents really are capable of computing REE processes. He assumes agents with no restrictions whatsoever on calculating abilities. He also assumes away the feature that to most researchers have seemed the main difficulty in learning, namely that actions of the agent depend on beliefs about the beliefs of other agents, and suggests a non-participant learner as e.g. an economist. Giving a rigorous definition of complexity of stochastic processes he shows that these cannot be learned generically by Bayesian methods. Kurz argues that real economic processes typically are of a kind satisfying his definition of complexity, for example dependent on a large number of parameters, and the set of these parameters continually changing with time. Rather than Spear's dependence on intrinsic logical limits to inference Kurz points to non-reducible complexity as a reason why agents should not be expected to learn completely the parameters of the processes they need to forecast.

3.2.7 Summary

In all the above models some market clearing mechanism is assumed. Hence the information received from the market prices is not confounded by quantity constraints, though that would not seem to be any really critical feature. It seems reasonable to assume that the inclusion of such constraints should not in any essential way change the results on learning. More crucial is that the definition of rational expectations is contingent on whatever information sets agents are endowed with by the modeller. Especially learning in asymmetric information models seems then to be rather ad hoc. In e.g. Bray(1982) it remains obscure how the informed agents came to learn the correct specification. In Fourgeaud, Gourieroux and Pradel(1986) it remains unclear why a certain choice of instrumental variable is made, by all agents nota bene. Some preliminary results of R. Frydman(1987) points out the possibility that diversity of opinion as to the correct model specification may in some cases actually enhance convergence to REE.[10] On the other hand there are results like Brusco(1988) in a similar model concluding that there will be no convergence with heterogeneous information when no group of agents is perfectly informed.

Anyway the REE will in general be dependent on the specifications of information sets used. This may lead, in the case of heterogeneity in initial beliefs, to rapid multiplication of possible REEs contingent on information assumptions. Such dependence is of course very troublesome for the predictive value of the rational expectations hypothesis since the information sets actually used by agents are only rarely observable.

[10] Diversity of opinion is then defined as negative correlations among the sets of variables used for forecasting by different groups of agents.

The picture emerging is somewhat complex. On the one hand stable REE often emerge from simple learning rules in linear models, at least within some parameter ranges. On the other hand, those rules could generally be improved upon by an optimizing individual agent. But attempts to such improvements would often render the REE unstable. In the case of only locally available information we may have to assume the existence of some external information dissemination and even individually sub-optimal consensus rules may be necessary for its almost certain convergence to REE. Moreover the actual REE achieved will be sensitive not only to assumptions about initial information sets, but also the length of memory and how learning rules weight past observations, as well as the confidence agents have in their beliefs about the appropriate models and learning rules. Extensions to more general model spaces and elaborate updating rules seem to undermine convergence results for simpler, fundamentally linear models. When the cost of information processing is taken into consideration it may further modify conclusions. If learning is too slow to prevent agents from discovering that their models are misspecified, it is far from clear what will happen, but many results point in the direction that if they try to be *too* clever the REEs will loose stability.

To me it seems to be at least two related sets of questions that need be answered before the relevance of RE learning to economic theory becomes reasonably clear.

1. Which one of several competing model specifications should a rational agent use when even economists disagree? In what sense might learning based on consensus rules be a rational economic choice? Is there an optimal choice of model specification given incomplete information on the form of the model? When and how should the basic model choices and learning rules be revised?

2. If agents learn by misspecified models such that they end up in REE, then there ought to be a pay-off to detecting such misspecification or even in some cases a pay-off to maintaining uncertainty by misleading signals or by experimenting to find out more about the system. How speedy need convergence be to prevent detection of misspecification? What happens to an economy where agents deliberately take sub-optimal decisions either to learn or to deceive?[11] To what extent should other rational agents take such possibilities into consideration?

No doubt there are more questions and perhaps more relevant, these are just two areas that strike me as important. In part this is due to my own work

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[11] S. J. Grossman, R. E. Kihlstrom, and L. J. Mirman(1977) deals with a similar problem in the learning by doing context. G. Eliasson(1989a) also tries to give some answers to this. The optimal control aspect without strategic interaction has been dealt with by Kiefer et alia, see above.

regarding the intertemporal consistency of conjectural variations in oligopoly models, i.e. the correctness of the conjectured dependence between the decision variables of oligopolists (cf. Lindh(1991) and section 3.3.). The problems encountered in this branch of modelling lead me to suspect that especially the second set of questions might be very tough to answer in any general way. Experiments could be conclusive only if you knew to what extent other agents engaged in experimenting. Hence all information is contingent on other agents' behaviour, which in turn depends on the information these agents possess and their informed guesses as well as the extent of their knowledge of each others' knowledge, etc. Questions of strategic interaction in learning introduce a potential circularity in the definition of information sets that may prevent learning from being even boundedly rational in any reasonable sense. A. Kirman(1983) provides a simple duopoly illustration of how such circularity makes the outcomes of learning procedures dependent on initial conditions and hence generally indeterminate without an argument for the specification of initial conditions. Townsend(1983a and 1983b) is clearly more optimistic in this regard, a view that seems based on belief in the reasonableness of common knowledge assumptions. In the game theory literature similar problems have been extensively investigated in connection with common knowledge assumptions, cf. e.g. Ken Binmore and Adam Brandenburger (1988) who go so far as to assert that Bayesian learning can be no more than a tiny part of genuine learning behaviour because it leaves the choice of priors unexplained. In a much earlier paper J. Marschak(1963) cautions us to observe that the optimal updating procedure cannot be chosen independently of the actions to be taken.

The hints above about "oversophisticated" agents disturbing stability also adds to this picture of complex interaction between beliefs, information and optimality at all levels. The information generated by the economy will depend on optimization dependent on beliefs. Beliefs that in turn are modified by updating procedures using the new information. These procedures may be dependent on information and beliefs, etc. The potential circularity is obvious, how to handle it is considerably less obvious. Good answers to the second set of questions therefore require good answers to the first set. The self-referential character of guessing about other agents' guesses demands some restrictions to be answerable. Such restrictions can be given by answers pointing out how rational model choices should be made in the absence of certain knowledge and to what extent other rational people can be expected to abide by the rules of the model and refrain from experiments and deceptions that create circularity in the learning process. But surely we still know very little about this in economics. The next section attempts to widen the perspective from RE learning to related problems in other areas in order to provide a wider perspective on these issues.

3.3. An attempt at perspective

In this section the problems of RE learning will be at least superficially related to more general adaptive learning in economics and to strategic interaction and game theory. Some interesting connections and references are pointed out without any ambition to discuss the deep issues involved. First some remarks are made on adaptive learning, then some issues of oligopolistic competition naturally leading to game theoretic aspects is considered. The section concludes by some brief comments on common knowledge assumptions.

In the models treated in section 3.2. "learning" means "learning the model" in order to optimize. A different approach to learning is the behavioural models in the spirit of R. Cyert and J. March(1963). The distinctive mark of this literature is that agents require much less information than is commonly assumed in rational, even boundedly rational learning models. That does in no way imply that outcomes differ, though of course they may. But "learning" in these models means "learning to be successful" in terms of whatever goal one wants to achieve.

Such learning models often have very simple rule mechanisms by which agents learn. The simplicity of the rules, however, does not necessarily impede their effectiveness. In e.g. Richard H. Day(1967) and R. H. Day and E. H. Tinney(1968) all it takes to converge to optimal solutions is some regularity, essentially convexity properties, in the postulated environment and some restrictions on how agents interaction takes place. The adaptive mechanism is extremely simple, just repeat successful behaviour and avoid unsuccessful and moderate responses according to the short history of the last two decisions made. If response moderation is avoiding extremes, convergence ordinarily results.[12]

Optimization over very large information sets can sometimes be replaced by very simple rules of thumb in stable enough environments. Rational economic agents should in many cases prefer simple rules of thumb to optimization even if perfect information sets were available at reasonable costs (cf. Baumol and Quandt (1964) or Winston(1989)). Uncertainties are associated with all real world information, e.g. measurement errors and transmission losses. The models used to process the information is subject to considerable uncertainty regarding their relevance. That is especially true of many economic models.

However, some coordination in behaviour as well as sufficiently informative feed-back is necessary, R. B. Archibald and C. S. Elliott(1989) show in a

[12] Crain, Shughart and Tollison(1984) have attempted an empirical test of Day's satisficing model and found that it seemed to fit data nicely with the exception that expansive responses did not seem to be moderated by past failures.

learning model formally equivalent with expected utility models that individuals may easily "learn" false hypotheses, i.e. commit Type II errors to use statistical terminology, if their sampling of the environment is biased or incomplete. This is closely related to the two-armed bandit problem in section 3.2.4 above. There is always the possibility of getting stuck at inefficient or non-rational equilibria or confounding signals generated by the structure of the model with signals generated by erratic or strategic behaviour of other agents. Sidney G. Winter(1970, 1975) emphasizes that, although the equilibrium of the optimizing model may be obtained as a special long run equilibrium of an evolutionary adaptive model, this requires rather special assumptions on the adaptive mechanism. Day, Morley and Smith(1974) show how very small changes in the environment can radically change outcomes. Day(1975) cautions that the potential complexity of adaptive models essentially is limitless and that in real life there will always remain scope for error and misjudgement.

In Marcet and Sargent(1988) the processes of boundedly rational learning are formulated in a way that clarifies their adaptive character. The difference is essentially in the state space. While adaptive processes in general imply movements in a space of available actions, the RE learning processes move in a space of conditional expectations or more generally model specifications. We may see the similarity by considering how to find, given a model specification including expectations, the forecasting rule making a certain action the optimal choice for an agent using this forecasting rule in this model. Delimiting the allowable set of forecasting rules should provide restrictions on the set of possibly optimal actions, but in general one would conjecture that variation in information assumptions and learning procedures would allow a fairly wide choice of model equilibria to be optimal. The results described in the preceding section also indicate this.

The concept of a learning process intrinsically includes some element of misperception on some level, because if agents had no misperceptions whatsoever they would have nothing to learn. Every learning process is in some sense an adaptive process where the outcome by definition cannot be precisely known a priori. Agents faced with the problem of finding out just what mistakes, beliefs and learning strategies others use can easily render economic processes unstable by trying to be overly rational[13] or by attaching too great confidence in faulty prior beliefs, and even if equilibrium is attained it may be a sunspot or a bubble resting on irrelevant common beliefs. This of course adds a very high degree of complexity to economic models unless we are

[13] An interesting paper in this context is Crawford(1985) using a coordinated updating procedure to show that mixed-strategy Nash equilibria are unstable for a wide class of learning mechanisms. Randomizing agents could be considered "overly" rational.

prepared to resort to some restrictions on what may and may not be allowable learning procedures and common knowledge. The question then is what should be presumed in order to keep things tractable.

In a rudimentary form that problem was considered already by Augustin Cournot (1838). Although more or less completely neglected at the time,[14] his solution to a simple duopoly model, being a special case of the general Nash equilibrium concept of non-cooperative games, has become famous. It has been the basis for the well known reaction function approaches to oligopoly problems of which the traditional conjectural variation models are an early example. It is interesting that Cournot takes the impossibility to exclude mistakes and deception as an argument in favour of optimizing as if the rival's action was independent of your own. The resulting equilibrium of his own simple adaptive model of duopoly was for a long time regarded as inferior to the cartel optimizing solution, see i.a. Fellner(1949).

Based on Fellner's "right for the wrong reasons" argument – that agents ought to perceive that their conjectures about the reactions of other agents are wrong – these models quite recently gave rise to a strand of literature exploring a "consistent" conjectural variation concept (Bresnahan(1981) and Perry(1982) among many others). Consistency in this context referred to the property that conjectural variations should be consistent with the optimal reactions of the oligopolists. The learning character of strategic interaction here becomes quite explicit through the motivation that agents ought to learn by experience how their rivals will react to changes.[15]

The Fellner critique obviously is very close in spirit to the common motivation for rational expectations, that agents learn by experience to avoid all systematic mistakes in their forecasts. But as the literature on RE learning shows, systematic misperceptions in the learning process itself does not necessarily prevent convergence to REEs. Likewise conjectural variation models often exhibit stability if agents do not take discrepancies in actual and expected values as a reason to revise their a priori beliefs before equilibrium is reached.

The problems of these failed attempts to rationalize conjectural variations by consistency are closely related to the problems in defining stable equilibrium concepts in non-cooperative game theory. Ken Binmore and Partha Dasgupta(1986) regard the above models as well as the somewhat related

[14] It was only forty five years after publication and six years after his own death that Cournot's "Recherches sur les principes Mathémathiques de la Théorie des Richesses" was reviewed by the statistician Joseph Bertrand(1883).

[15] The existence of such conjectures, however, hinges critically on assumptions of linear conjectures and often results in a negative pay-off to learning. That is to say, the agent would do better by not trying to learn about the reactions of other agents. There are also logical problems with the interpretation of "consistency" in this context that leads to semantic paradoxes (cf. Lindh(1991)).

conjectural equilibrium (F. Hahn (1977,1978)) as premature. They argue that game theory has not yet developed concepts precise enough to describe rationality or consistency in this setting without ambiguity.[16] Maybe, but it remains to be seen whether such precision can be obtained. As the Gödel theorem warns us, the techniques of formal proofs do not necessarily generate all true statements.

One may view strategic game solutions as mimicking the outcome of learning processes by the selection of strategies that will prove stable in a certain environment of game rules and actions of other players. Striking similarities can be seen between economic equilibrium concepts and concepts arrived at by biologists modelling evolutionary games. In evolutionary games the player does not choose among different strategies, receiving a pay-off which he tries to maximize. Instead the set of strategies are seen as a population of single-minded players that reproduce themselves into the next stage of the game according to success. Though the dynamic processes are very different, the concepts of equilibria are often quite similar in games where rational players choose optimal strategies and in evolutionary games where the strategies survive. The results are in general very sensitive to assumptions on how new entrants choose their strategies, and also to the exact characteristics of the pay-off structure.[17]

The problem of strategic games is more ordinarily perceived as finding an acceptable solution to two or several conflicting optimization problems (O. Morgenstern and J. von Neumann (1947)) or making rational choices in situations where the outcome depends on the actions of other agents or players. That point of view leads to another aspect of the general problem of economic learning. K. Arrow(1986) stresses that rationality, although often presented as a property of the individual alone, in fact is mainly dependent on the social context of the individual. One can easily agree with Arrow that the comprehensive common knowledge and sophisticated rational calculations of fully rational learning goes contrary to the spirit of viewing market processes as efficient informational institutions.[18]

However, bounded rationality can take many different forms and yield many different results. If such a route should be followed economic theory cannot establish those bounds on rationality on an *ad hoc* basis. It seems ines-

[16] Cf. Bernheim(1986) for a discussion of how different rationality concepts affect the choice of relevant equilibrium concept. An enlightening discussion on behaviour out of equilibrium in a game theoretic context can be found in Kreps(1989).

[17] For some short notes on this with further references, see Dasgupta(1989) and Hammerstein(1989).

[18] Common knowledge or consensus rules are central to any definition of rational, or boundedly rational, behaviour of the individual. For a thorough but easily accessible discussion on common knowledge, game theory and Bayesian learning, see K. Binmore and A. Brandenburger(1988).

capable that boundedly rational learning requires explicit modelling of the institutional and informational environment of economic agents. F. Hahn(1989) argues that the definition of economic equilibrium should explicitly recognize that learning implies that the historical path of the economy and the information specific to this path is decisive. The hypothesis of individual rationality then cannot be sufficient to resolve the issue of what agents will learn in a market system. In order to draw general conclusions on how to improve real economies we not only must consider their specific histories but also must prescribe how institutions and information arrangements should change. Thus there seems to be much need for a theory of *collective* rationality governing this choice of institutions and information arrangements to provide stabilizing common knowledge to learning agents.[19]

The lessons of RE learning research may be potentially revolutionary in economics by bringing path dependence and institutional information arrangements into focus. The demonstration that the RE hypothesis relies so heavily on implicit assumptions in this respect must surely have consequences for how economists think about economic equilibria in the future.

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[19] In fact there is a recent paper by M. Cripps(1991) investigating the optimal monetary policy in a game with agents learning about inflation concluding that the optimal policy in fact delays learning.

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Chapter 4: Productivity Deceleration when Technical Change Accelerates

4.1 Introduction

For a long time economists have been concerned about the slowdown that standard measures of productivity growth indicate in the late 60's or somewhat later. The historically high rate of productivity growth in the first two decades after World War II in the industrialized countries seems to have been replaced by a considerably slower growth rate in almost all these countries. Many explanations have been offered, most often maybe the rise in energy prices in 1973, e.g. see Griliches(1988) and Jorgenson(1988), but declining knowledge production, slower rates of increase in labour skill, declining investment rates, shifts towards service production, and increasing government sectors and distorting taxes have also been appointed as causes, cf. S. Fischer(1988). For countries other than USA the exhaustion of technological gaps may also play a role, cf. Abramowitz(1990). The possibility that the whole slowdown really is due to measurement errors has also been pointed out, for example cf. Baily and Gordon(1988) for a general treatment of mismeasurement in the US, E. Berndt and Wood(1987) for some energy price related measurement issues, and B. Carlsson(1989) for micro-based measurement considerations. But the productivity slowdown still remains largely a puzzle and none of the possible causes has been generally accepted to explain more than minor parts of the slowdown.

One reason why economists continue to put forward new explanations for the productivity slowdown is, I believe, the feeling that productivity should have a close connection with the rate of technological change, in the sense of knowledge accumulation and technological advance in both the production process itself as well as new and better products. One puzzle posed by the productivity slowdown then is that it coincides with a period of technological progress that many intuitively feel must be rather fast and showing little sign of slowdown. Not only has there been a massive computerization in production but new and better materials, e.g. ceramics substituting for metals (Scientific American Oct 1986) as well as great changes in management and organization. The speed and sheer volume in the changes that have taken place in production techniques, in the quality of goods and services and the extension of choice we have experienced during the slowdown period makes it hard to accept that the 50's and 60's just were exceptions in the secular growth trend. Therefore the quest for an explanation to the productivity puzzle continues.

This paper considers one of several possible ways of reconciling an actual acceleration in technical change with a deceleration in the productivity growth rate. The basic idea is that the productivity structure of the inherited capital stock may be such that adjustment to new techniques becomes increasingly costly. The general idea that progress in production possibilities may at least temporarily have negative feed-back effects in the economy has old roots in economic theory. Hicks(1973) have formalized such effects in a neo-austrian framework and refers to Ricardo as the originator of a theory where mechanization causes unemployment and temporary decline in production. Some of Marx's (varying) explanations why the tendency towards a falling rate of profit does not materialize have a similar flavour, see especially ch. 15 sec. 3 in the third volume of "Das Kapital'(1894). The Schumpeterian business cycle theory incorporates similar ideas in the process of "creative destruction" (cf. Schumpeter(1911). Keynes' concept of the "marginal efficiency of capital" seems intended to capture analogue considerations, see ch. 11 in "The General Theory..."(1936). Such effects of course may be captured in many different formal mechanisms but the key idea is in all cases that the existing capital structure somehow prevents or distorts immediate adaptation to new production techniques.

It is a fairly simple exercise to show that decelerating labour productivity may be compatible with acceleration in technical change in a simple standard Solovian growth model given certain parameter settings).[1] In the Solow type of model technical change is however by definition identical to total factor productivity and it is therefore not possible to explain any slowdown in total factor productivity by this mechanism. Of course, the mechanism of increasing adjustment costs due to the capital structure has no place here either, although an assumption of increased depreciation rates or a decreased savings ratio may very well cause labour productivity to fall in the simple Solow model.

Even though it is an old idea, it has turned out to be difficult to model the influence from an irregular capital structure on productivity growth since it is

[1] I owe this observation to Stefan Lundgren, at IIE in Stockholm. Let labour productivity $\pi(\varkappa,t)$ be a function of capital intensity and time. The proportional rate of growth is $\hat{\pi} = (1-\alpha)\hat{\varkappa} + \hat{\theta}$, α labour elasticity of production and $\hat{\theta}$ is the rate of technical change. The time derivative $\dot{\hat{\pi}} = (1-\alpha)\dot{\hat{\varkappa}} - \dot{\alpha}\hat{\varkappa} + \dot{\hat{\theta}}$, implies $\dot{\hat{\varkappa}} < 0$ and $\dot{\alpha} > 0$ is sufficient for some acceleration of technical change to actually result in labour productivity deceleration. Assume the savings ratio s and depreciation rate δ are constant. $\hat{\varkappa} = s\pi/\varkappa - \delta$ implies $\dot{\hat{\varkappa}} = s\frac{\pi}{\varkappa}(\hat{\theta} - \alpha\hat{\varkappa})$. An elasticity of substitution less than unity imply $\dot{\alpha} > 0$. Choose parameter values $s = 0.2$, $\delta = 0.08$ and assume $\alpha = 0.6$ and the capital/output ratio $\varkappa/\pi = 2$, then $\dot{\hat{\theta}} > 0.012$ will be sufficient. These values are empirically reasonable so it is certainly a possibility.

tied to the transitory paths of non-steady state growth. When the importance of induced capital obsolescence mechanisms have been considered in the slowdown debate, as e.g. by Hulten, Robertson and Wykoff(1987), and by Baily(1981), it has been within a framework of parametrization of the capital vintage structures, making it possible to work with quality adjusted capital stocks and measures of capital utilization, instead of an explicitly non-balanced vintage structure.

This study is conceptually based on a simple one good macro model of the vintage type, pioneered by Leif Johansen(1959), Robert Solow(1960,1962) and W. Salter(1960,1965) and extensively studied in a variety of forms in the 60's by i.a. Phelps(1963), Bliss(1968) – putty-clay and perfect foresight– , Sheshinski(1967) – putty-clay and static wage expectations, Solow-Tobin-Yaari-Weizsäcker(1966) – clay-clay and perfect foresight–, Kaldor and Mirrlees(1962) – technological progress function–, etc. just to mention a few representative papers. M. N. Baily(1981) used a putty-putty variant in an early study pointing to obsolescence due to rising energy prices as an important cause of the slowdown which has been followed up by several others seemingly with inconclusive results. An interesting recent paper by J. Benhabib and A. Rustichini(1989) use a variant based on a utility maximizing representative agent with fixed capital objects distributed as a time-indexed sequence. Their model includes an echo effect in investment activity such that earlier investment slumps will cause another slump in the future. This is related to the main idea of this paper. They also estimate the model on aggregated U.S. investment data and find vintage effects supported by the data. A fixed lifelength for capital equipment is assumed, making it difficult to compare with the model in this paper.

The formal production model chosen is a version of the short run macro production function of Leif Johansen(1972) with one variable factor, based on aggregation of capacity distributed over fixed input coefficients without explicit reference to vintages, but still closely related to vintage models in general. In this context the concept of labour productivity is more natural than total factor productivity since the marginal rate of substitution between different capital inputs cannot in general be made independent of the labour input thereby preventing aggregation of capital equipment into a homogeneous stock that is separable from labour in the production function. Without imposing regularity conditions on the structure which we want to avoid the concept of total factor productivity becomes ambiguous.

In a vintage type model productivity varies due to technical change but also by changes in the capital structure. Irregularities of the capital structure will thus provide for variation in the transmission from technical change to productivity growth. The putty-clay context also forces a more explicit recognition of the crucial importance of price expectations for long term investment since every investment is a unique sunk cost. Thereby this model emphasizes the dependence of present economic performance on both the past history and expectations on the future.

The basic idea about increasing costs for implementing new technique is a two-way mechanism. One way is by an increase in the wage raise[2] required to transfer a certain amount of labour from obsolete equipment at the extensive margin to new best-practice equipment on the intensive margin of production. The other way is by increases in capital costs due to changes in expectations on future returns to capital. The former mechanism starts to function when a given rate of increase in wages tends to free less and less labour per time unit thus raising operating costs in new investment. The latter effect is less well defined since it depends on expectations and how these change, but typically periods of extensive scrapping may induce both an increase in the interest rate due to increased investment demand and a shortening of expected economic lives of capital equipment. Both these effects tend to increase capital costs and thus slows down investment. This is important both for a slowdown in labour productivity growth and total factor productivity growth as measured by deducting factor share-weighted rates of factor growth from the total growth in production.

When the short run supply schedule is concave on the margin of total capacity it can be shown both that a given transfer of labour requires a higher wage raise than in preceding periods and that a given reduction of production costs requires a relatively more extensive scrapping. A concave supply schedule will then be a sufficient condition for productivity slowdown when technical change is constant given that expectations react in the above indicated way. It is far from necessary however, although it is intuitively easier to understand the mechanisms in this case. Such a short run supply schedule is essentially the distribution of input coefficients ranked from the lowest to the highest over accumulated output. One needs only multiply by the ruling wage to get supply as a function of price. It is often referred to as a Salter curve, cf. Salter(1960).

A Salter curve that is concave in the upper portion may be the result of a similar investment slump at the time of installation of the equipment that is now on the margin of obsolescence. Hence we have here an echo mechanism by which earlier slowdowns and speedups tend to show up in the future, too. Not in a perfectly cyclical fashion since lifelengths will change and so there will be no exact periodicity. It is interesting to note here that simulation studies of vintage models calibrated with real data (Bentzel(1978), Melén(1990)) turn up very substantial reductions of the endogenously determined life length of

[2] Lawrence Lau has pointed out to me that the same kind of effects could be achieved by replacing the assumption of full employment by a fixed rate of increase in wages. This would work by slowing down scrapping and thereby providing less room for new investment.

capital equipment in the late 60's, indicating dramatically increased scrapping rates. But the concave portion may also be due to localized changes in the specific techniques used in older vintages which may even have rearranged the order of vintages within the input coefficient ranking.

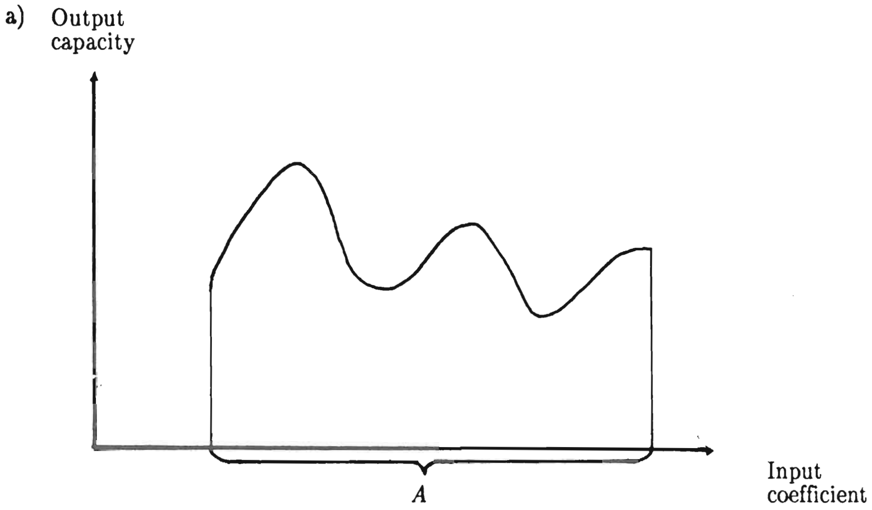
Expectations are hard to model in this long run context. Assumptions of static expectations about wages and interest rate are obviously unsatisfactory on a growth path with changing growth rates. Perfect foresight is hard to solve for on such a path and also hard to motivate, since there would be no previous experience to learn such expectations from. To avoid these problems at an early stage of the study the choice here is to treat expectations as a black box concept, i.e. as exogenous to the formal model and only discuss their relations to endogenous changes heuristically. Since the determination of interest rates will depend heavily on expectations of future developments of production, so will consumption demand too, being residual to savings. Therefore only the production side of the model economy is explicitly modelled.

The paper is organized as follows. Section 4.2 attempts to give an intuitive non-formal description of the essential mechanisms of the model. Section 4.3 develops the formal production model of a putty-clay structure with a representative firm taking decisions on scrapping and investment. A law of motion of the production structure is derived that shows changes in aggregate production to be the product of new capacity flow and the relative difference in input coefficients of the structure. Section 4.4 derives a basic bench-mark condition for simultaneous deceleration in aggregate productivity growth and acceleration in technical change under simplifying assumptions; fixed discount rates, expected wage rates and life time of capital, fixed labour supply and a Cobb-Douglas type production function characterizing the best-practice technique. In section 4.5 the restrictions on expectations are relaxed and conditions on how they must move in order to slow down productivity is derived. The plausibility that endogenously determined expectations would move in that direction is discussed informally. In section 4.6 assumptions of fixed labour supply and unit elasticity of substitution are relaxed and the relation labour productivity to total factor productivity is discussed. Section 4.7 finally holds some concluding comments. The appendix contains a list of variables for easy reference and some proofs of assertions made in the text.

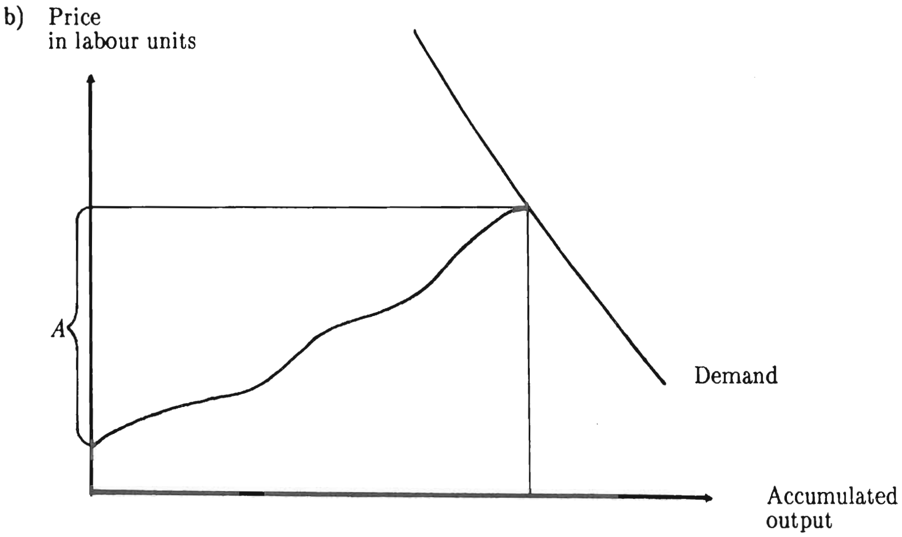
4.2 Intuition

Before engaging in formal modelling it may be helpful to gain some intuitive insight in the mechanisms of how productivity interacts with technical change in a vintage context. First consider a simplified vintage model. One and only

Fig 1



The distribution of capacity ranked by input coefficients



Supply and demand schedule. Supply given by the inverse of the accumulated capacity distribution. I.e. the labour cost of production in each unit. *A* represents the same span of input coefficients in both diagrams.

one good is produced by two factors, homogeneous labour and heterogeneous capital. Capital taking the form of once-and-for-all fixed pieces of equipment which once installed have fixed input/output coefficients, i.e. a conventional putty-clay structure. For simplicity we keep capital costs fixed.

Let the vintage structure of capital be described by a distribution of output capacity over fixed labour input coefficients. In fig. 1a) an arbitrary distribution of output capacity for each input coefficients is depicted and in 1b) the resulting distribution of average labour costs per unit (or input coefficients, with a suitable scale) over accumulated output. The latter we can regard as a short run supply schedule, since equipment will not be used when labour costs exceed returns. Assuming a fixed demand schedule and free competition the price of the good will equal average variable cost at the extensive margin and the present value cost per unit at the intensive margin.

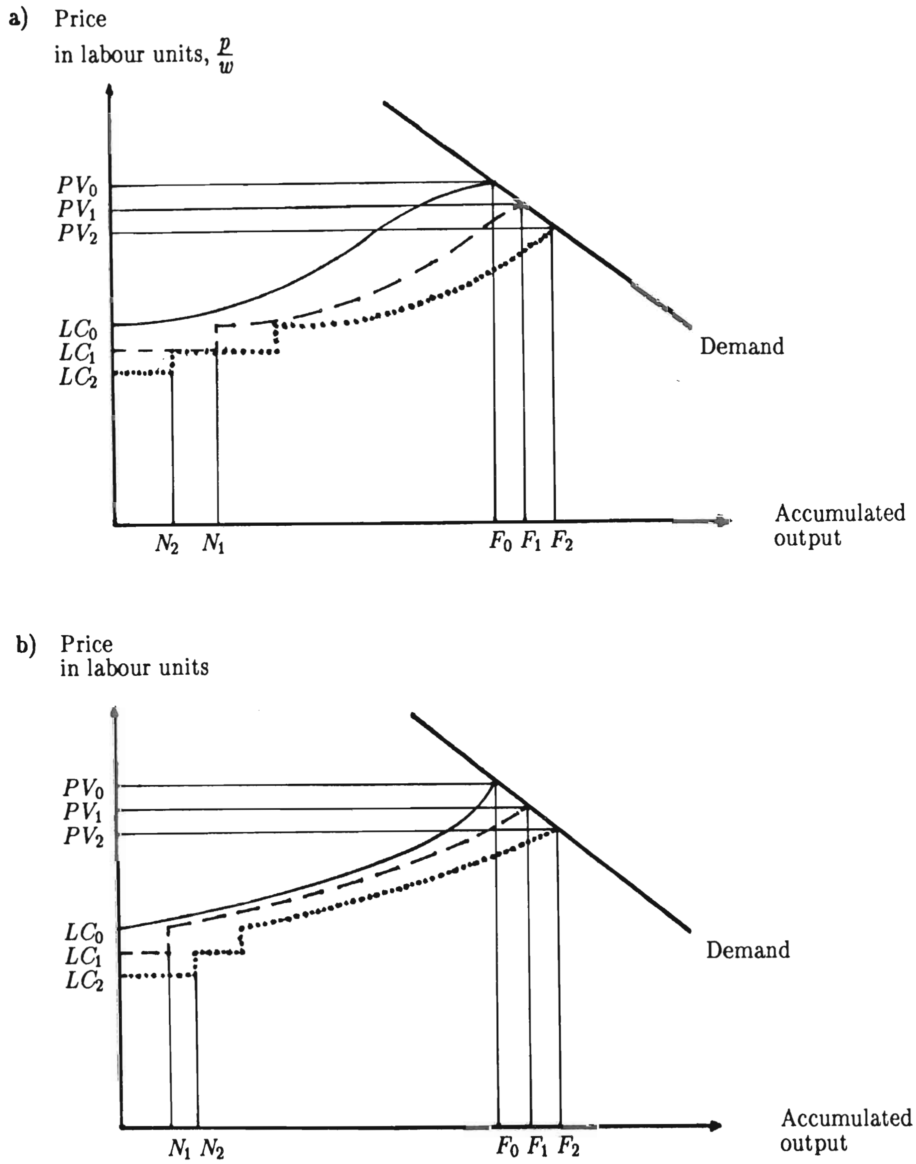
In fig. 2 we have a diagram of two extreme examples of supply schedules together with the fixed demand. Fig. 2a) will be referred to as a backflat supply and 2b) as a backsteep supply. It is assumed that in both cases we have market equilibrium at the same relative price in terms of labour units. We also assume that in both cases the minimal input coefficient is the same. Under these assumptions the economy in 2a) would require more labour than the one in 2b) to carry out the same production and hence would have a much lower aggregate labour productivity.

Given this setup we assume a sudden jump to occur in production possibilities which the economy adapts immediately to. To facilitate comparison we depict the sudden change in production opportunities as an equal reduction in both cases of the minimal input coefficients.

The story behind the diagrams can be taken in steps. At given prices the reduction in the minimal input coefficient will reduce the present value cost per produced unit in best-practice equipment thus making new investment profitable. When the production from new investment arrives in the market real wages will be raised. This partly leads to scrapping of now obsolete equipment and partly offset the reduction in present value cost per unit in front production, thus equilibrating the market. It is then clear that a given change in present value cost will tend to transfer considerably more labour to the front production from the scrapped equipment in the backflat case than in the backsteep case. Hence in the backflat case many production units with low labour productivity are replaced by units using the best available technique, while in the backsteep case relatively few such low productive units are replaced.

Thus, the impact of a given change in technique will have a proportionally greater influence on aggregate productivity in the backflat case. Whether the jump in the rate of technical change will be associated with deceleration in aggregate productivity growth of course depends on the rates of change preceding the jump in the front. By introducing another jump of equal size

Fig 2



In a) a backflat and in b) a backsteep supply schedule. *PV* is present value cost per produced unit, *LC* is labour cost per produced unit, *N* is added new capacity. Subindex 0 denotes initial values, subindex 1 values after adaptation to the first change, subindex 2 values after the second change in best practice input coefficient. Note how the added capacity diminishes in a) in step 2 while it increases in b).

in the input coefficients of the best available technique it is evident that the introduction of this second reduction in average labour cost must imply a transfer of relatively less labour than the preceding reduction in case 2a) where the right end of the distribution is concave, while exactly the converse happens in case 2b). Not only are fewer units replaced in the second transition in the backflat case but the share of each unit in total production has also decreased, thus further reducing the contribution to aggregate productivity growth. Aggregate production per capita would hence increase at a decreasing rate in the backflat case. Now it is obvious that a slightly greater reduction of average cost in the second jump might still be associated with a slowdown in the rate of aggregate productivity growth. Of course a slowdown is all the more probable if technical change also decelerates, but the point to be made here is that a slowdown in productivity does not necessarily signal a slowdown in technical change. It should also be noted that if the reduction in average cost is sufficiently large productivity will increase nevertheless. But there is some positive degree of acceleration in technical change that can occur simultaneously with decelerating productivity growth.

The above reasoning is of course much too simplified to be relied on for any definite conclusions. If we allow capital cost adjustments and substitution to work freely there may be both dampening and reinforcing feed-backs from the markets. Note that in case 2a) a considerably larger investment must be made to take advantage of the cost reduction. When capital costs are flexible these would increase and thus dampen investment relatively more in case 2a) than 2b). Hence productivity growth would be further damped. Growth in the labour supply, factor substitution, deterioration and disembodied technical change occurring simultaneously would naturally influence conclusions like many other modifications.

Obviously assumptions on demand and expectations are very important in this context. Since there is no difference in price between investment goods and consumption goods with only one aggregate good the price of capital goods does not enter into capital costs. In a more disaggregated model the movement of relative prices would also be important. Ruling out balanced growth – only example 2b) could possibly be a steady state supply schedule – the direction of change in conclusions will depend on the details of interest determination and expectation formation as well as the elasticity of substitution in the front production. Any factor-saving bias in the evolution of front production possibilities would also be important. So even though the diagrams and the reasoning above make the countervariation in technical change and productivity growth intuitively plausible when the supply schedule is concave at the extensive margin, it is by no means evidence that a formally specified model economy, where indirect effects and price responses are taken into account, would confirm this intuition. In the following we will therefore in a very simple formal model specify more rigorously the conditions for second order changes in best practice technique and productivity to be in opposite directions. With these basics clear we can then make some of the extensions mentioned above.

4.3 The representative firm and its production structure

This section contains the formal production model which will be used for a more rigorous analysis of the mechanisms discussed in the preceding section. The model must be simple enough to be tractable yet maintain the essential complexity of a heterogeneous capital structure. The general background should be thought of as an economy in temporary market-clearing equilibrium in continuous time. Formally, however, we will not explicitly model the demand side, but be content to discuss it informally.

Our general modelling strategy aims only at showing the influence from a historically given capital structure and expectations of future price movements on the transmission from changes in the technical potential to changes in economic productivity. After having laid down a formal production framework here we will in the next section derive a basic condition for productivity slowdown. Given that, we will then in section 4.5 discuss how parametric changes in price expectations will modify the picture. In section 4.6 we relax two other important simplifying assumptions to be made below, viz. fixed labour supply and unit elasticity of substitution in the best practice technique.

The production model is a conventional putty-clay type where one homogeneous good is produced by one primary factor, labour, using heterogeneous capital equipment. The good can either be immediately consumed or frozen into capital equipment which is impossible to recover for consumption. The good is chosen as numéraire for real prices in the model.

The "one good" assumption abstracts from several real world features, among which the most important probably is the very different characteristics of investment and consumption goods. But one essential difference between these goods is preserved by the freezing assumption, viz. the durability and commitment to the future that investment as a sunk cost carries with it. The investment cost cannot be recovered should outcomes deviate from expectations.

Not only is the equipment as such fixed but we also assume that producers expect that once installed it can only be operated at a fixed labour/output ratio. They also disregard the possibility of disembodied technical change and deterioration of equipment so output from a specific equipment will also be believed to be fixed. We will further adopt the assumption that this belief is true in the current short run.

But there are no restrictions, apart from analytically convenient continuity and differentiability assumptions, on what kind of historic path that actually has generated the structure. The assumptions we will make on the current production possibilities, current technical change etc. refer only to current time and its immediate vicinity. That makes the model more generally valid as an approximation for empirical problems, but of course prevents theoretical conclusions about the long and medium run, limiting us to statements only about the immediate future. Ideally we would of course like to loosen these assumptions, but this must be a later task.

Before proceeding to the formal model we must be specific about what we mean by aggregate productivity growth. As remarked in the introduction the standard definition in terms of total factor productivity growth is not well suited to our heterogeneous capital structure. Productivity here therefore means output per labour unit. In section 4.6 we will discuss this further and show how the conventional measure of total factor productivity moves in the same direction as labour productivity in our model if the "stylized fact" of a constant capital/output ratio is assumed. From here on "productivity" always refers to labour productivity unless otherwise stated.

Let $\Pi = F/V$ stand for aggregate productivity, where F is aggregate production and V total labour supply. It will prove convenient to work with proportional growth rates, and also more natural since we rarely talk about changes in other terms. To simplify notation we let a circumflex over a variable denote a total logarithmic time differentiation. If we differentiate the proportional growth rate $\hat{\Pi}$ w.r.t. to t we get

$$\frac{\mathrm{d}}{\mathrm{d}t}(\hat{\Pi}) = \frac{\mathrm{d}}{\mathrm{d}t}(\hat{F} - \hat{V}) \tag{3.1}$$

which is the change over time in aggregate productivity growth if V varies over time. With V fixed, aggregate productivity will decelerate if growth in aggregate production slows down

$$\frac{\mathrm{d}}{\mathrm{d}t}\left[\frac{F_t}{F}\right] = \frac{F_{tt}}{F} - \frac{F_t^2}{F2} < 0 \text{ or } \frac{F_{tt}}{F_t} < \frac{F_t}{F} \text{ if } F_t > 0 \tag{3.2}$$

where subindices denote partial derivatives. Obviously this is substantially easier to analyze so we will keep labour supply V fixed here, and discuss the effects of relaxing this assumption only in section 4.6. Our aim is to show how exogenous technical change, the labour elasticity of best-practice technique and the capital structure will determine F_{tt}/F_t.

Having assumed a description on the basis of a putty-clay approach, there are however alternative formal ways to describe such a structure. A description in terms of capacity ranked by labour/output ratios in the manner of Leif Johansen's(1972) short run macro production function is the more convenient for our modelling strategy. Since the existing structure is given arbitrarily by history, we can dispense with the explicit vintage distribution and use only a description in terms of input coefficients, because we need not derive the structure analytically from any given history of investment.

Let ξ denote labour/output ratios, and assume there is a distribution of capacity over these input coefficients at current time t,

$$\psi(t) = \int_{X(t)}^{\infty} \psi(\xi,t)\mathrm{d}\xi \tag{3.3}$$

$X(t)$ being the minimal input coefficient at that time. Assuming that new investment is made in equipment with the minimal input coefficient, the density function can be simplified to $\psi(\xi,t) \equiv \psi(\xi)$ since we assumed installed equipment to have both fixed input coefficients and fixed output. We further assume the density to be continuously differentiable without any point masses and the support $\{\xi : \psi(\xi) > 0\}$ to be a connected set, i.e. there is a positive capacity for each $\xi \geqslant X(t)$ up to some maximal input coefficient. $X(t)$ is also assumed to be continuously differentiable. That $X(t)$ is monotonically decreasing is a simple consequence of the above assumptions. Keep in mind that this refers to the vicinity of current time and need not have been true over the whole history of the structure.

Assume one uniform wage rate for all uses of labour. It is then obvious that an optimizing representative firm will use all capacity at lower fixed input coefficients before transferring any labour to less productive equipment. Since the capacity density is continuous there will be no unit used at less than full capacity. Labour market equilibrium with an exogenously given labour supply holds when

$$V(t) = \int_{X(t)}^{R(V,t)} \xi\psi(\xi)\mathrm{d}\xi \tag{3.4}$$

where $R(V,t)$ is the maximal input coefficient for capacity actually used. Assume full employment. Then the aggregate production function can be written

$$F(V,t) = \int_{X(t)}^{R(V,t)} \psi(\xi)\mathrm{d}\xi \tag{3.5}$$

The function arguments will be suppressed from here on whenever it is not needed for clarity.

We can differentiate (3.4) and (3.5) w.r.t. V, thus obtaining the standard result 1

(3.6) $$F_V = \psi(R)R_V \text{ and } 1 = R\psi(R)R_V \implies F_V = \frac{1}{R}$$

meaning that the aggregate marginal productivity equals the average productivity in the least productive equipment.

Differentiating (3.4) w.r.t. t, using a dot over the variable to denote this, and recalling that V is fixed, we get

(3.7) $$R\psi(R)R_t = X\psi(X)\dot{X}$$

with the obvious interpretation that labour released from existing equipment equals that employed in new equipment. Likewise differentiating (3.5) yields

(3.8) $$F_t = -\psi(X)\dot{X} + \psi(R)R_t$$

The change in production is the added capacity minus the abandoned. These two expressions hold even if V is not fixed but the interpretation then requires the addition of equal terms on both sides for capacity changes due to changes in total labour supply. We can then define the flow of new capacity as

(3.9) $$\varphi(t) \equiv -\psi(X)\dot{X}$$

and by using (3.7) and (3.6) to substitute, rewrite (3.8) as

(3.10) $$F_t(V,t) = \varphi(t)(1 - F_V(V,t) \cdot X(t))$$

where arguments are written out to emphasize dependencies[3] or equivalently, skipping the arguments,

(3.11) $$F_t = \varphi(1-\frac{X}{R})$$

we can express the parenthesis in terms of the relative gap in input coefficients. These equations serve as the law of motion for the production system. They quite generally describe the change in production over time and will be our main vehicle in deriving the relation between changes in productivity growth and technical change. But first we need to determine how the capacity flow and minimal input coefficient will move.

We then assume a representative firm in the model that takes all production decisions. The firm first has to make a decision whether to go on using already existing equipment or not. We will call this the scrapping decision since the equipment has no alternative use. But we keep open the possibility that scrapped equipment may be reinstated in production without incurring any

[3] This partial differential equation actually can be shown to have the vintage description as solution under certain conditions, cf. Pomansky and Trofimov (1990), hence confirming the equivalence of the two descriptions. Such a solution, however, is of little use to us, being only a translation from one formalism to another which could be derived independently.

extra costs. This will ordinarily not happen but we want to avoid paying specific attention to the restriction imposed by actually truncating the capacity distribution.

The scrapping decision is a very simple one. Because there is no alternative use for the equipment it only depends on whether production can cover operating costs. With a continuous capacity distribution wage costs will be equal to production in the least efficient production unit used. Using w for real wages

$$w = F_V = \frac{1}{R} \tag{3.12}$$

is the scrapping condition, so the usual marginal productivity condition holds for the aggregate.

The firm must also make an investment decision. We assume the firm chooses its investment, k, and the labour supply flow, v, necessary to operate the new equipment in order to maximize the present value of its future profit flow from the investment. The front production function, $f(k,v,t)$, is the maximum capacity given k and v, with standard properties as differentiability and quasi-concavity. It changes over time due to technical change. We will also assume f to be linearly homogeneous in k and v and $f(k,0,t) = 0$. The firm maximizes the present value $P(k,v)$ of expected profits at each point in time. Let r stand for the interest rate.

$$\max_{k,v} P(k,v,t) = \int_t^{\infty} (f(k,v,t) - \tilde{w}(z,t)v(t))\, e^{-\int_t^z \tilde{r}(x,t)dx}\, dz - k \tag{3.13}$$

$$\text{subject to } f(k,v,t) \geq \tilde{w}(z,t)v(t)\ \forall z$$

where $\tilde{r}(\cdot)$ and $\tilde{w}(\cdot)$ are the currently expected time paths of interest rates and wages. Let $\bar{l}$ stand for the life time the investment is expected to last, i.e. yielding non-negative quasi-rents, and assume the firm not to expect any further use of the equipment. Expectations of monotonically rising wages will then imply a finite $\bar{l}$ and also a connected period of usage. This also guarantees P to be bounded for any finite choice of k and v. To clean up notation somewhat we leave out the argument for current time in expectations and define $\tilde{v}(z)w(t) = \tilde{w}(z)$ so we can move the current wage out of the integral in (3.13) and define the shorthand expectational variables.

$$D = \int_t^{t+\bar{l}} e^{-\int_t^z \tilde{r}(x,t)dx}\, dz \text{ and } W = \int_t^{t+\bar{l}} \tilde{v}(z) e^{-\int_t^z \tilde{r}(x,t)dx}\, dz \tag{3.14}$$

The first order conditions for maximization can then be written conveniently as

$$f_k = \frac{1}{D} \text{ and } f_v = \frac{wW}{D} \tag{3.15}$$

Note that although only w is an endogenously determined factor price in the model, capital costs generally depend on w too, since $\bar{l}$ depends on the level of current wages. The present value function may be convex due to the dependency of $\bar{l}$ on the capital/labour ratio chosen, so a unique maximum cannot be guaranteed by first order conditions in general.[4] Here we simply assume f and expectations to have the properties needed for uniqueness. Assume that the economy is in a state of perfect competition so the present value of expected quasi-rents exactly cover investment costs

$$\varphi D - wvW - k = 0 \tag{3.16}$$

using φ to emphasize that this holds for actually installed capacity.

As stated above we will not write down a formal model for the consumer side of the economy but take that as exogenously given. But a short discussion is appropriate to clarify why it is not needed. First of all we assume labour supply to be exogenous, so only the savings in the economy remain to determine. Since both investment and consumption demand is for the same good all we would really need the consumer side for is the determination of the current interest rate. The savings decision of consumers given market interest rates will in the standard Ramsey model depend mainly on their time preferences and intertemporal elasticities of substitution. Investment demand on the other hand is mainly determined by expectations.

Incorporating e.g. a Ramsey model of the consumer side would determine the ruling interest rate given expectations on future production flow and thereby equalizing saving and investment. The interest rate would be heavily dependent on expectations which we prefer to keep exogenous any way so we loose very little by ignoring the consumer side and take interest as exogenous.

Since we also abstain from formally modelling the capital market it leaves us with the level of investment and production indeterminate, but as will be clear in the next section we will only need their rates of change to make our point.

It will prove convenient later on to work with the cost shares or factor elasticities of the ex ante production function and we therefore define the labour elasticity, the equalities holding in equilibrium

$$\alpha \equiv \frac{wWv}{D\varphi} = \frac{wW}{D}X = \frac{XW}{RD} \tag{3.17}$$

which allows us to rewrite the law of motion for production, (3.10) and (3.11), in yet another way

---

[4] Cf. Bliss(1968), who gives a condition guaranteeing $P(k,v)$ to be locally concave on steady state paths if the elasticity of substitution is sufficiently high. Essentially this is a condition that the ex ante production function must not be too inelastic with respect to changes in capital intensity. In Appendix 3 this is further elaborated.

$$(3.18) \qquad F_t = \varphi(1 - \alpha\frac{D}{W})$$

We will find both the above formulation and the others useful further on. While (3.10) emphasizes that the movement of production over time is determined by capacity flow times the current quasi-rent earned on new investment, (3.11) expresses this in terms of the relative gap in productivity between the intensive and extensive margin. This last formulation then emphasizes the elasticity of the *ex ante* production function and the decisive role of expectations, especially the role of wage increase expectations.

Returning to (3.2) we can then reformulate the condition for deceleration as

$$(3.19) \qquad \frac{F_{tt}}{F_t} < \frac{\varphi}{F}(1 - \alpha\frac{D}{W}) \text{ or } \frac{F_{tt}}{F_t} < \frac{\varphi}{F}(1 - \frac{X}{R})$$

Since $D < W$ and $\alpha < 1$ the RHS of the above inequality is always positive and for each given φ/F the more so the larger is the productivity gap between the intensive and extensive margin. We will not elaborate more on this side of the inequality. In the next section we will instead focus on translating the LHS of the condition into rates of technical change and elasticities of the capacity distribution and thereby demonstrate how the capital structure of an economy can dampen the translation of technical change into productivity growth.

4.4 The basic condition for deceleration in aggregate productivity

When will the rate of aggregate productivity growth slow down even if the rate of technical efficiency growth increases in the best available techniques? We will derive an answer to that question in our simple model by introducing some further simplifications. Though not as general as could be desired this condition will nevertheless throw light on the interaction between technical change and a heterogeneous capital structure. We shall proceed by relating the rates of change in aggregate productivity growth to the elasticity of a predetermined arbitrary capacity distribution and the rates of change in a parameter of technical change in the ex ante production function.

4.4.1 Derivation of the basic condition

Given the *ex ante* production function $f(k,v,t)$ we define the rate of technical change as

(4.1) $$\hat{\theta} = \frac{f_t}{f}$$

that is the proportional increase with time in production possibilities with k and v fixed. This is a standard definition but note that here it refers to the *ex ante* production function and not to the aggregate production function and therefore is not identical to total factor productivity growth.

The role of expectations in the model is of course very important but in this section we only aim to achieve a bench-mark condition for deceleration in aggregate productivity growth. Recall that W and D will depend on expected life lengths, in turn implying they are both dependent on current wages and thus would change with wages even if interest is constant and the expected paths of changes invariant. To simplify the derivation of the basic condition we provisionally assume not only that the expectations of changes in interest and wage rates but also the expectational variables W and D are held fixed throughout adjustments and postpone treatment of changes in W and D to the next section. In section 4.5 we show that under the assumptions behind the basic condition, this will in general be equivalent to fixed expectations functions.

The basic condition for deceleration in this section will be derived under the assumption that the *ex ante* production function is of Cobb-Douglas type. In the first steps here we will retain a more general front production function in order to avoid duplication in section 4.6.

Using the formulations (3.11) and (3.18) of the law of motion of the production and differentiating logarithmically, recalling that V is fixed, we then have

(4.2) $$\frac{F_{tt}}{F_t} = \hat{\varphi} - \frac{\alpha D/W}{1 - \alpha D/W}\hat{\alpha} \quad \text{or} \quad \frac{F_{tt}}{F_t} = \hat{\varphi} - \frac{1}{R/X - 1}(\hat{X} - \hat{R})$$

This we want to relate to the rate of technical change and the capacity distribution. Rearranging (3.7), equating labour flow in new investment to that released from abandoned equipment, using the definitions (3.9) of capacity flow and (3.17) of the labour share, we have

(4.3) $$\varphi\alpha\frac{D}{W} = -\,\psi(R)R_t \quad \Longrightarrow \quad \hat{\varphi} + \hat{\alpha} = \frac{\psi'(R)R_t}{\psi(R)} + \frac{R_{tt}}{R_t}$$

which relates capacity flow at the intensive margin to the changes in the capacity distribution at the extensive margin. Define the elasticity of the capacity density function as

(4.4) $$e(\xi) = \psi'(\xi)\xi/\psi(\xi)$$

With the understanding that $R_t/R = \hat{R}$ only when V is fixed we can rewrite the RHS of the implication (4.3)

(4.5) $$\hat{\varphi} + \hat{a} = e(R)\hat{R} + \hat{\hat{R}} + \hat{R} = -(1 + e(R))\hat{w} + \hat{\hat{w}}$$

where we have used that $\hat{\hat{R}} = \frac{R_{tt}}{R_t} - \hat{R}$ and $w = 1/R$. Note how the second equality here confirms the intuition from section 4.2 that wage increases will dampen productivity growth when the capacity distribution is of a certain form. This will be still clearer further on. We now want to relate wage changes to the exogenous rate of technical change so we differentiate the *ex ante* production function logarithmically to get

(4.6) $$\hat{\varphi} = (1 - a)\hat{k} - a\hat{v} + \hat{\theta} = \hat{k} - a\sigma\hat{w} + \hat{\theta}$$

where σ is the elasticity of substitution and the second equality is a simple consequence of the definition of σ implying $(\hat{k} - \hat{v})/\hat{w} = \sigma$ in this particular case when W is fixed. To simplify at this point we now assume a Cobb-Douglas specification of the *ex ante* production function

(4.7) $$f(k,v,t) = \theta k^{1-a}v^{a}$$

which implies $\sigma = 1$, constant and, since D fixed, $\hat{\varphi} = \hat{k}$. Then follows

(4.8) $$a\hat{w} = \hat{\theta} \text{ and } \hat{\hat{w}} = \hat{\hat{\theta}}$$

and the second term on the RHS of (4.2) vanishes and we get by (4.5)

(4.9) $$\frac{F_{tt}}{F_t} = \hat{\varphi} = -(1 + e(R))\hat{\theta}/a + \hat{\hat{\theta}}$$

We can then state the main result in this section, namely that productivity will decelerate if

(4.10) $$-(1 + e(R))\hat{\theta}/a + \hat{\hat{\theta}} < (1 - aD/W)\frac{\varphi}{F}$$

giving us a simple bench-mark form of the deceleration condition. The RHS of this inequality is always positive. Clearly if the elasticity of the capacity distribution is positive a proportional acceleration in the rate of technical change may be perfectly compatible with deceleration in aggregate productivity growth as long as $\hat{\hat{\theta}}$ is not substantially greater than $\hat{\theta}$. The condition may still hold for a range where $e(R)$ is not too negative and $\hat{\hat{\theta}}$ remains positive.

Note that $e(R) > -1$ will make higher rates of technical change contribute to deceleration. In fact, as long as technical change is positive this will be a sufficient condition to guarantee that productivity deceleration can take place at the same time as some positive degree of acceleration in technical change. For any given value of $e(R)$ above -1 it will be the case that the higher $\hat{\theta}$ is, the higher must $\hat{\hat{\theta}}$ be to reverse deceleration in productivity.

We can also note that our intuitive reasoning in section 4.2 that a concave supply schedule would promote productivity deceleration is borne out since that is exactly equivalent to a positive elasticity in the capacity distribution.

Using ξ for an independently varying maximal input coefficient and $\bar{F}(\xi)$ for production as a function of this we have

$$\text{(4.11)} \qquad \bar{F}'(\xi) = \psi(\xi) \text{ and } \bar{F}''(\xi) = \psi'(\xi)$$

Hence, inverting this to obtain the supply schedule in terms of input coefficients as a function of total production and discarding the bars

$$\text{(4.12)} \qquad \xi'(F) = \frac{1}{F'} \text{ and } \xi''(F) = -\frac{1}{F'^3}F'' = -\frac{e(\xi)}{\xi\psi(\xi)^2}$$

proving this assertion for all open sets where $\bar{F}'(\xi) \neq 0$. Due to our assumption of a connected support of ψ it therefore holds for the whole supply schedule. Hence our intuition is confirmed but we can also conclude that concavity of the supply schedule is a stronger condition than needed.

The elasticity $e(R)$ is positive whenever $\psi(R)$ is increasing, e.g. if past investment has been insufficient to raise capacity flow at the same rate as input coefficients decreased. This is not what we would regard as a normal case in a growing economy, but it does seem probable that it may happen in the real world for different reasons. Recessions, wars and shocks to the economy like the oil crisis of 1973, may very well cause such downturns or at the very least dampen the capacity flow in relation to technical change. Some decades afterwards these economic downturns would then echo through deceleration in aggregate productivity growth. And furthermore they would continue to echo. The elasticity of the density would be exactly reproduced from the extensive margin to the intensive under our bench-mark assumptions. Under more general assumptions it would be modified in different directions but still influence the evolution of productivity growth.

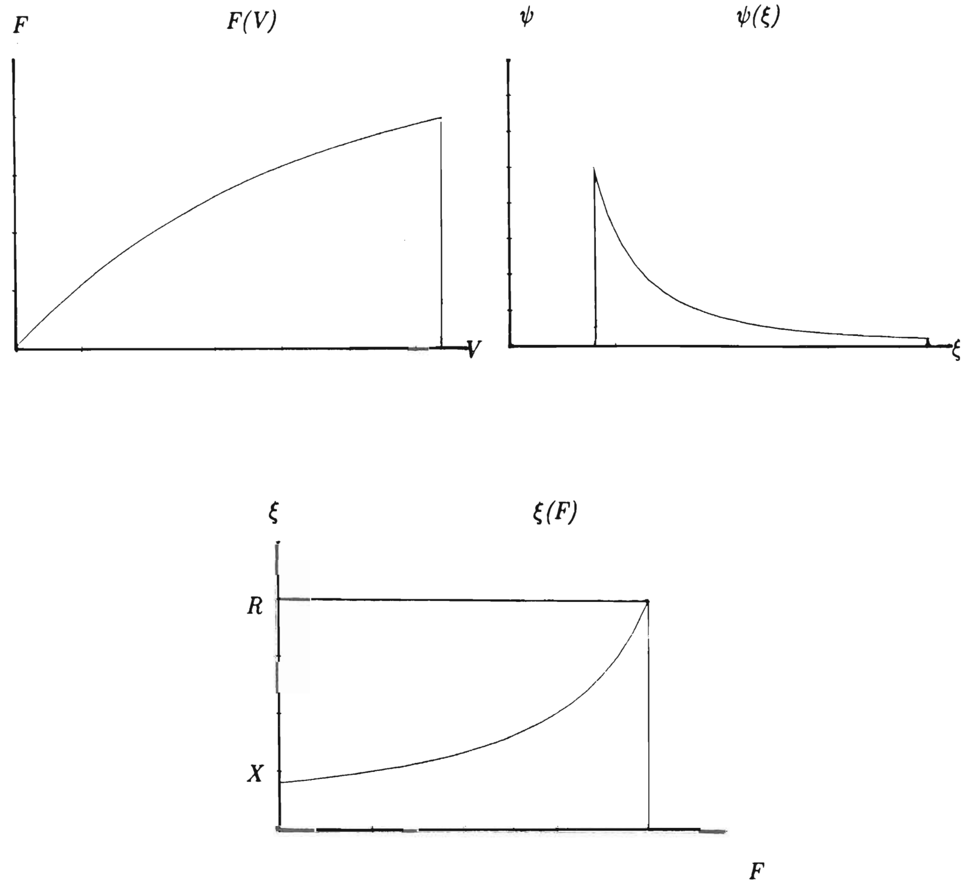
There are other possibilities, too, if we allow for localized disembodied technical change or deterioration to have shaped humps in the capacity distribution. Similar possibilities have been considered by e.g. Atkinson and Stiglitz(1969) in a short but very suggestive paper. If disembodied technical change, as could be expected, affect different techniques in a non-uniform way this may increase capacity for different input coefficients such that humps are created in the capacity distribution, cf. Pomansky and Trofimov(1990). On one side of the hump the capacity elasticity will of course be positive.

Under our assumptions here $e(R) = e(X)$ and the first part of (4.5) will hold when we substitute X for R everywhere. Under the Cobb–Douglas assumption the equation we then get will hold also if V is variable, cf. appendix 2.

4.4.2 The steady state example

In order to give some more feeling of the meaning of condition (4.10) we will take a look on how it works out in the special case when the vintage economy

Fig 3



The steady state production function $F(V) = \frac{\varphi}{\gamma}(1 - e^{-(\gamma/v_0)V})$, and the corresponding capacity distribution, $\psi(\xi) = \frac{v_0}{\xi^2\gamma}$, and supply schedule, $\xi(F) = \frac{v_0}{\varphi - \gamma F}$. The graphs are generated with Mathematica, using the value 32 for total labour supply, V, $v_0 = 1$, $\gamma = 0.05$, $\varphi = 0.25$

actually has evolved along a steady state path.[5] A steady state growth path exists only if technical change in the *ex ante* production function is Harrod-neutral, i.e. purely labour-augmenting. In the Cobb–Douglas case this of course is of no consequence since this specification is consistent both with capital-augmenting and labour-augmenting technical change but to take notational advantage of this we let $\theta = e^{\alpha\gamma t}$

$$f(k, ve^{\gamma t}) = k^{1-\alpha}(ve^{\gamma t})^{\alpha} \tag{4.13}$$

where γ is the fixed rate of change in labour efficiency. With zero labour supply growth both φ and F will grow at the exponential rate γ. Furthermore the lifelength l will be fixed as will $v(t) = v_0$. It follows that

$$\frac{X}{R} = a\frac{D}{W} = e^{-\gamma l} \tag{4.14}$$

Thus the RHS of the basic condition (4.10) becomes

$$\frac{\varphi(1 - aD/W)}{\varphi(1 - e^{-\gamma l})}\gamma = \gamma \tag{4.15}$$

by calculating the integral $F = \int_{t-l}^{t} \varphi(t)e^{-\gamma(t-\tau)}d\tau$. Since clearly $l = V/v_0$ we can write the production function as

$$F(V,t) = \frac{\varphi_0}{\gamma}e^{\gamma t}(1 - e^{-\gamma V/v_0}) \tag{4.16}$$

where we use φ_0 to denote capacity flow at an initial time zero. Then follows

$$F_V(V,t) = \frac{\varphi_0}{v_0}e^{\gamma t}e^{-\gamma V/v_0} = \frac{1}{Xe^{\gamma V/v_0}} = \frac{1}{R} \text{ and } R_v = \frac{\gamma R}{v_0}, \tag{4.17}$$

$$\psi(R) = \frac{1}{RR_V} = \frac{v_0}{R^2\gamma} \text{ so } \psi'(R) = -\frac{2v_0}{R^3\gamma} \text{ and } e(R) = -2$$

Since we also have

$$\hat{\theta} = \alpha\gamma \tag{4.18}$$

it is easily seen that (4.10) in a steady state structure cannot hold since

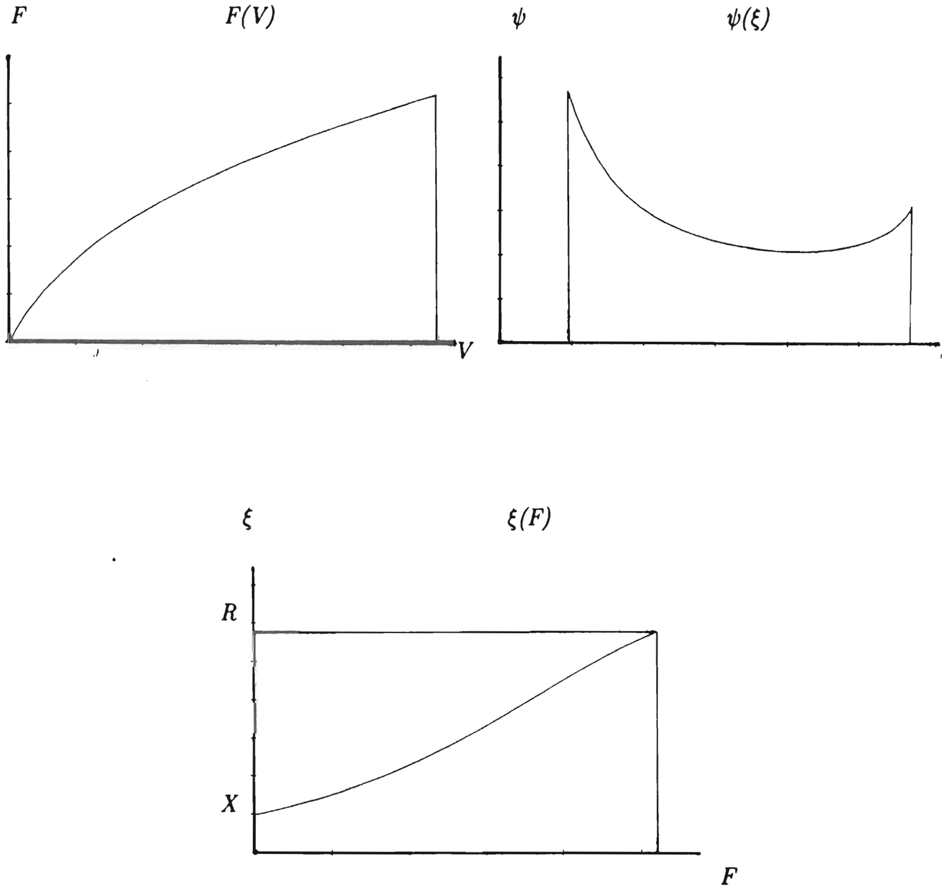
$$\frac{F_{tt}}{F_t} = \gamma = \frac{F_t}{F} \tag{4.19}$$

which confirms the obvious that since technical change is fixed by parameters there will be no change in aggregate productivity growth thereby corroborating our calculations.

If we introduce growth in γ, that is $\hat{\hat{\theta}} > 0$, from the current moment then it is easily verified that

[5] The properties of this type of putty-clay model in steady state are well known. Those interested in the details are referred to Bliss(1968) for a general perfect foresight treatment. A treatment with stationary wage expectations can be found in e.g. Sheshinski (1967).

Fig 4



Production function $F(V) = \frac{1}{c}[\ln(V+\frac{a}{c}) - \ln(a-b(V+\frac{a}{c})) + \ln(c-b)]$, and the corresponding capacity distribution density, $\psi(\xi) = \frac{a}{2bc\xi[(a/2b)^2 - (a/bc)\xi]^{0.5}}$, and supply schedule, $\xi(F) = \left[\frac{c}{b + (c-b)e^{-cF}}\right]\left[a - \frac{ab}{b + (c-b)e^{-cF}}\right]$.

The graphs are generated with Mathematica, using the value 32 for total labour supply, V, $a = 2$, $b = 0.02$, $c = 0.5$ This production function is arbitrarily chosen for being reasonably simple to calculate and yet provide an example of backflat supply. F in fact is the lower half of an inverted logistic curve and no economic significance should be attached to the parameters. Note that $\xi(F)$ is weakly concave to the right.

$$-\hat{R} = \gamma + \dot{\gamma}t = \frac{F_t}{F} \tag{4.20}$$

and $\hat{R} = \hat{\hat{\theta}} > 0$ so by using (4.5) we can conclude that we would have acceleration and not deceleration in aggregate productivity growth.

$$\frac{F_{tt}}{F_t} = -\hat{R} + \hat{\hat{\theta}} > \frac{F_t}{F} \tag{4.21}$$

Hence acceleration in the proportional rate of technical change in a steady state structure will always accelerate aggregate productivity as an immediate effect under our assumptions in this section. Fig 3 depicts the steady state production function *F(V)*, the capacity distribution $\psi(\xi)$, and the corresponding supply schedule $\xi(F)$ in terms of labour input. Note that the latter is strictly convex everywhere.

Just to provide a contrast, a concave production function with an underlying capacity density with positive elasticity in the back end have been depicted in Fig 4. The corresponding supply curve is slightly concave in its upper part, illustrating the intuition of section 4.2 that a back flat supply would be conducive to productivity slowdown. Note that the concavity is hardly discernible in spite of the rather obvious stretch of positive elasticity in the end of the capacity distribution.

From (4.21) we see that the steady state structure is a boundary case, since if *e(R)* is only slightly greater than -2 and the capacity distribution otherwise have the steady state elasticity except for a small neighbourhood to *R* we could have deceleration even for some small $\hat{\hat{\theta}} > 0$.

4.4.3 Summary

Summarizing this section it has been shown that locally on the time path a vintage economy follows it is possible that $\hat{\hat{\theta}} > 0$ at the same time as $\hat{\Pi} = \hat{\dot{F}} < 0$. This is derived under very simplified assumptions and the next two sections will consider what happens when some of these assumptions are relaxed. The most notable feature of the condition is that it clearly shows that the above state is most likely when the elasticity of capacity $e(R) > 0$ or when the supply curve (distribution of average labour costs) is concave in its upper end. But as the steady state example shows, it might very well happen even for elasticities close to the steady state value of -2. Note however that the boundary value of *e(R)* will be dependent on the predetermined structure and therefore in general will vary around -2, because the RHS of the basic condition also depends on the structure.

Of course, if *e(R)* is less than the boundary value we may have retarding technical change at the same time as accelerating productivity growth. The discussion here has been focussed on the reverse case, partly because the inspiration to this study comes from the productivity slowdown debate, partly because it would be very tedious to keep repeating all statements only reversing directions. It should be noted that the symmetry is around the boundary value of $e(R)$, which will be less than -1, and not around $e(R) = 0$. A convex supply schedule hence does not imply that retarding technical change can occur simultaneously with acceleration in productivity.

Relaxations of the assumptions in this section will modify conclusions, but those modifications have to be very substantial in order to eliminate the possibility of productivity slowdown at the same time as technical change accelerates. It therefore seems that the result as such will turn out to be fairly robust, decelerating productivity growth is a distinct possibility even if technical change accelerates. Counter-intuitive though it may seem such responses could be expected fairly often if this model is a not too distant approximation to reality.

More importantly, even if this case does not arise in the real world due to economic forces excluded here, the model still demonstrates the fundamental importance of the capital structure. It is clear that the shape of the capacity distribution even in cases when it does not cause productivity deceleration influences the transmission of technical change into production in very significant ways. When $e(R) > -1$ it even makes a high basic rate of technical change work against an accelerated productivity.

In the next two sections some of our restrictive assumptions will be relaxed. First we rather comprehensively will discuss the impact of changes in expectations in section 4.5 and then more cursorily labour supply growth, more general *ex ante* production functions and total factor productivity in section 4.6.

4.5 Expectations

On the steady state path expectations should be based on perfect foresight because then both life length of the equipment and the interest rate will be constants, and the growth rate of real wages will be the same as the constant rate of increase in labours technical efficiency so the firm should not be expected to systematically deviate in its expectations from these values. Any firm placed in the structurally stationary environment necessary for steady state growth should by simple adaptive rules be able to learn these constant parameters. Theoretical studies of the learning of rational expectations[6]

[6] I.a. Bray(1982), Marcet-Sargent(1989a and 1989b) and many others, for a more comprehensive list see Lindh(1990)).

clearly indicate that conditional on some prior coordination among agents such a stationary parametric environment ought to be learnable. Anyway agents should not be expected to remain on a steady state path where outcomes systematically deviates from the expected, so if we want to assume a steady state we should endow agents with perfect foresight.

In the non-balanced state we have a radically different situation. If we are to assume perfect foresight when the rate of technical change is allowed to vary in irregular ways we have to endow the economic agents with a degree of sophistication and precognitive abilities that takes on distinctly occult dimensions. Not only would they need parapsychological foresight to correctly predict the path of technical change for some decades ahead but they also would need a degree of scientific sophistication not available to any Nobel prize winner by being able to correctly trace out the complicated dynamic growth paths resulting from future irregular technical change, even within our simplifying assumptions. Allowing for uncertainty and risk behaviour would not really improve the situation much, since the rational expectations hypothesis suffers from essentially the same difficulties. The set of future technique states and its probability distribution would be just as hard, or harder, to predict as any specific path. The problem facing long term fixed investment is not to choose among some well defined possible pay-off states. It is a genuine uncertainty about what the state space even might look like, not to mention the complexities of the pay-off path, dependent as it is on the actions of other agents.

However, disregarding that lack of realism it could still be the case that perfect foresight was acceptable as a reasonable approximation to close the model. Because of the complexity of the dynamic path we would probably have to resort to numerical simulation with its lack of generality in order to analyze the model path when technical change becomes irregular.

We could then settle for some rule of thumb forming of expectations, but even such rules may exhibit very complex behaviour if they are allowed to adapt to exogenous influences of an irregular non-stationary character. If we are to have rational economic agents in the model their expectations should adapt when prediction fails. Therefore assumptions of static expectations, although easy to handle, are also clearly unsatisfactory. Agents exposed to a highly irregular history of price changes would be rather thick-headed if they assumed current prices – or rates of price change – to be constant over the life of a long term investment.

The choice here to avoid these difficulties has therefore been to treat expectations as black boxes that may change their output signal in response to changes in other variables but without trying to specify in detail how changes in current variables are interpreted and adapted to by the firms. Up to now we have therefore treated the expectations variables W and D as fixed. In this section we will try to say something about how changing expectations may influence productivity movements.

First we will establish how exogenously given changes in W and D will affect the growth rate of productivity and find a sufficient condition for the first order changes in W/D to slow down productivity growth. In the next step we use a parametrization of price expectations to establish the expected price changes this condition will hold for. Finally we informally discuss when endogenously determined price expectations are likely to fulfill the sufficient condition for slowing down productivity growth.

First we must modify the basic condition. Relaxing our previous assumptions of fixed present value expectations means that we must rewrite equation (4.2)

$$(5.1) \quad \frac{F_{tt}}{F_t} = \hat{\varphi} - \frac{\alpha D/W}{1 - \alpha D/W}(\hat{\alpha} + \hat{D} - \hat{W}) = \\ = \hat{\varphi} - \frac{\alpha D/W}{1 - \alpha/W}(\hat{X} - \hat{R}) \Longrightarrow \hat{R} - \hat{X} = \hat{\alpha} + \hat{W} - \hat{D}$$

introducing another source of difference between the rates of decrease in the front and rear end of the capacity distribution, i.e. the relative productivity gap becomes variable even if the elasticity of substitution is unity. The implied relation between changes in minimal and maximal input coefficients and expectations also follows directly from definition (3.17). The previous equation (4.5) becomes

$$(5.2) \quad \hat{\varphi} + \hat{\alpha} + \hat{D} - \hat{W} = e(R)\hat{R} + \hat{\hat{R}} + \hat{R} = -(1 + e(R))\hat{w} + \hat{\hat{w}}$$

and, since the wage/rental ratio is wW, (4.6) becomes

$$(5.3) \quad \hat{\varphi} = (1 - a)\hat{k} - \alpha\hat{v} + \hat{\theta} = \hat{k} - \alpha\sigma(\hat{w} + \hat{W}) + \hat{\theta}$$

by definition $\sigma = (\hat{k} - \hat{v})/(\hat{w} + \hat{W})$. Adopting the Cobb–Douglas assumption about the *ex ante* production function will no longer make the second term in (5.1) vanish so the condition for deceleration in aggregate productivity (4.2) will now read

$$(5.4) \quad \frac{F_{tt}}{F_t} = \hat{\varphi} + \frac{\alpha D/W}{1 - \alpha D/W}(\hat{R} - \hat{X}) < (1 - \alpha\frac{D}{W})\frac{\varphi}{F}$$

rearranging signs a little. By (5.1) and (5.2) then

$$(5.5) \quad \frac{F_{tt}}{F_t} = (1 + e(R))\hat{R} + \hat{\hat{R}} + \frac{1}{1 - \alpha D/W}(\hat{R} - \hat{W}) < (1 - \alpha\frac{D}{W})\frac{\varphi}{F}$$

After some algebraic manipulation, using (5.3) and noting that the Cobb-Douglas assumption now implies $\hat{\varphi} - \hat{k} = -\hat{D}$ and thus

$$(5.6) \quad -\hat{w} = \hat{W} - \hat{D}/\alpha - \hat{\theta}/\alpha$$

the LHS of the inequality can be written

(5.7)

$$-(1 + e(R))\hat{\theta}/\alpha + \hat{\hat{R}} + \left[\frac{1}{1 - \alpha D/W}(\hat{W} - \hat{D}) + (1 + e(R))(\hat{W} - \hat{D}/\alpha)\right]$$

To determine how changing expectations influence the condition of deceleration we have to determine the sign of the bracketed expression which we will refer to as A, as well as how $\hat{\hat{R}}$ relate to $\hat{\theta}$.

Rearranging A it can be shown that $A < 0$ if and only if

(5.8)

$$\hat{W}(1 + (1 - \alpha D/W)(1 + e(R))) < (1 + (1 - \alpha D/W)(1 + e(R))/\alpha)\hat{D}$$

Assuming $\hat{W} < \hat{D}$ turn out to be a sufficient condition for (5.8) to hold except when $-\alpha/(1 - \alpha D/W) < 1 + e(R) < 0$. Hence it will certainly hold for all $\alpha < 1$ and $e(R) > -1$, i.e. in the critical region of a distribution where the rate of technical change contributes to deceleration.

We have thus established a sufficient condition for A to be negative, although it clearly is not necessary. It then remains to see how $\hat{\hat{R}}$ relates to $\hat{\theta}$. Using (5.6) and noting that $\hat{R} = -\hat{w} < 0$ by assumption we can by some manipulation establish that $\hat{\hat{R}} < \hat{\hat{\theta}}$ either if

(5.9) $$\frac{\alpha\dot{\hat{W}} - \dot{\hat{D}}}{\alpha\hat{W} - \hat{D}} > \hat{\hat{\theta}} \text{ and } \alpha\hat{W} - \hat{D} > 0$$

or alternatively

(5.10) $$\frac{\alpha\dot{\hat{W}} - \dot{\hat{D}}}{\alpha\hat{W} - \hat{D}} < \hat{\hat{\theta}} \text{ and } \hat{W} - \hat{D} < 0$$

$\alpha\dot{\hat{W}} - \dot{\hat{D}} > 0$ is necessary for $\hat{\hat{R}} < \hat{\hat{\theta}}$ to hold in the case of (5.9) and sufficient in the case of (5.10). Hence if $\alpha\hat{W} - \hat{D}$ is growing fast enough the second order change will also contribute to deceleration in productivity growth. Note that it is a sufficient condition when $\alpha\hat{W} - D < 0$. We will return to the meaning of this later on when expectations have been further discussed.

Recall the definition of W and D from (3.14)

$$D = \int_t^{t+\bar{l}} e^{-\int_t^z \tilde{r}(x)dx} dz \text{ and } W = \int_t^{t+\bar{l}} e^{-\int_t^z \tilde{r}(x)dx} dz$$

where $\tilde{v}(z)w(t) = \tilde{w}(z)$. To simplify notation somewhat the tilde will be skipped hereafter in this section since it is clear that we only treat expectations of these paths here.

Without specifying how expectations are formed definite conclusions cannot be drawn. Note however that W and D will change with w, it is only v and r that are independent expectations functions, l must satisfy the consistency criterion that $w(t)v(t+l)v(t) = \varphi(t)$ so even if the expectations functions stay fixed W and D will change with the changes in l induced by changes in wages and front capacity flow. Furthermore, the forms of the functionals *W(v,r)* and *D(r)* are such that the ratio *W/D* cannot change quite arbitrarily. We can note that e.g. a decrease in life length will in itself always decrease *W* by more than it decreases *D*, since the integrand is everywhere greater in the former than in the latter and we hence cut off more mass in the former when we change the integration limits equally. Things are complicated, however, because the integrand of *W* may increase overall and hence add mass to *W*. Likewise an increase in interest rates can be expected to take away more from *W* than from *D*, since its effect is scaled up in the former. To conclude anything definite we want to take account of the proportional effects and hence scale down changes in *W* more than we do in *D*. *A priori* it is then not at all clear in what direction *W/D* will move.

To get some intuitive handle to judge the likelihood of a decreasing ratio *W/D*, we therefore parametrize the expectations functions, by assuming that a constant average rate of interest is used as well as a constant expected exponential rate of wage increases. I.e. we write

(5.11) $$D = \int_0^l \mathrm{e}^{-rz}\mathrm{d}z = \frac{1 - \mathrm{e}^{-rl}}{r} \quad \text{and}$$

$$W = \int_0^l \mathrm{e}^{(\beta - r)z}\mathrm{d}z = \frac{1 - \mathrm{e}^{(\beta - r)l}}{r - \beta}$$

where β is the proportional rate of wage increase expected, $t = 0$ has been chosen since the integrands in both cases will equal one at t, so we have no loss of generality. With this parametrized form we will use the partial derivatives to show how a decrease in *W/D* corresponds to movements in average expectations on wage changes and interest rates such that the latter is greater than the former. We have thereby produced a sufficient condition for changes in expectations to contribute to deceleration through a negative term *A* in condition (5.4).

First we see that the consistency critèrion now will read

(5.12) $$\mathrm{e}^{\beta l} = \frac{\varphi}{wv} = \frac{R}{X}$$

Keeping β and r fixed thus implies

(5.13) $$\beta \dot{l} = \hat{R} - \hat{X} = \hat{W} - \hat{D} = \left[\frac{\mathrm{e}^{(\beta - r)l}}{W} - \frac{\mathrm{e}^{-rl}}{D}\right]\dot{l}$$

By some manipulation it can be verified that l must stay constant if β and r are fixed since (5.13) is then an equation in l with unique solution, so we see that the assumption about fixed *W* and *D* made earlier actually is equivalent to fixed expectations in this case with parametrization and Cobb-Douglas technique. It is not difficult to see that this conclusion would hold generally for fixed expectations functions, except in very special cases like static wage expectations.

It is obvious that $\beta > 0$ guarantees $W/D > 1$. Differentiating W and D w.r.t. time and dividing through to get proportional rates we have

$$(5.14) \qquad \hat{W} - \hat{D} = \left[\frac{W_l}{W} - \frac{D_l}{D}\right]\dot{l} + \frac{W_\beta}{W}(\dot{\beta} - r) - \frac{D_r}{D}\dot{r}$$

By (5.12) we have

$$(5.15) \qquad \beta\dot{l} + \dot{\beta}l = \hat{W} - \hat{D}$$

so we can solve for $\dot{l}$ in terms of $\dot{\beta}$ and $\dot{r}$. It will prove convenient to proceed by expressing the proportional rate of change of the interest rate as a constant times the proportional rate of change of the wage increase parameter. I.e.

$$(5.16) \qquad \hat{r} = \eta\hat{\beta} \text{ so } \dot{r} = \eta\frac{r}{\beta}\dot{\beta}$$

so we have

$$(5.17) \qquad \hat{W} - \hat{D} = \left[\frac{W_l}{W} - \frac{D_l}{D}\right]\dot{l} + \left[\frac{W_\beta}{W}(\frac{\beta - \eta r}{\beta}) - \frac{\eta r D_r}{\beta D}\right]\dot{\beta}$$

Solving from (5.15) and (5.17) we then have

$$(5.18) \qquad \hat{W} - \hat{D} = \frac{al - b\beta}{a - \beta}\hat{\beta} \text{ if } a \neq \beta$$

It is proved in appendix 4 that the denominator is strictly negative when $\beta < r$, and the sign of the numerator will be the opposite to that of $1 - \eta$. Hence the numerator is positive when $\eta > 1$ and therefore we have

$$(5.19) \qquad \hat{W} - \hat{D} < 0 \text{ if } \hat{r} > \hat{\beta} \text{ and } \beta < r$$

From (5.15) and (5.17) we have

$$(5.20) \qquad \dot{l} = \frac{l - b}{a - \beta}\dot{\beta} \text{ and } a < \beta,\ al > b\beta$$

so life length decreases as expected wage change increases. It follows by differentiation that

$$(5.21) \qquad \dot{W} = e^{(\beta - r)l}\dot{l} + (\dot{\beta} - \dot{r})W_\beta \text{ and } \dot{D} = e^{-rl}\dot{l} + \dot{r}D_r$$

If (5.19) holds and $\dot{\beta} > 0$ then both W and D must decrease since it is clear that $W_\beta > 0$ and $D_r < 0$ and $\dot{\beta} < \dot{r}$. Therefore it is clear in this parametric case that when W/D is decreasing both W and D decreases separately, too.

We can then proceed to the question how reasonable price expectations should move in response to increases in the rate of technical change. The critical ratio W/D depends on expectations of wage increases and expectations on the path of interest rates, but also on the expected life time of capital equipment which is determined here by the equality of best practice labour productivity to the wage expected to prevail at the date of scrapping. It is reasonable to assume that acceleration in technical change should persuade the firms to adjust expectations of future wage increases upwards. It also seems reasonable that interest rates would tend to rise as well since we know that they would in general do so if we moved from one steady state to another with higher rate of technical change.

Recall that β and r here only are parametrical representations of the truly expected wage and interest paths. A decreasing ratio W/D therefore does not require that the interest rate change exceeds the rate of change in wage increases proportionally, $\hat{r} > \hat{\beta}$, at the current time nor over the whole life of the current investment, but only that this holds in some average sense. It is a textbook result from the Ramsey model of optimal growth that the impact of technical change on interest rates will be positive and dependent on the intertemporal substitution elasticity of consumption, cf. e.g. Blanchard and Fischer(1989), and also that a positive time preference imply an interest rate higher than the rate of technical change, which on the steady state path will equal the rate of wage increase. We cannot assert that this holds also for non-steady state paths, but it at least suggests that the above condition may hold.

It can be argued that the condition $\hat{r} > \hat{\beta}$ should hold in circumstances when labour is thought to be abundantly available in comparison to investment capital. To the extent that factor price expectations are based on recent experience the above view of the future would make sense in a situation when demand for new investment have been rising and the labour supply for new investment have been abundant. Referring back to fig. 2 in section 4.2 we see that in the backflat case extensive scrapping have made vast amounts of labour available in the near past at a relatively modest cost in terms of wage increases, while at the same time ample room for profitable investment have been provided. Without extending our model framework to specification of the determinants of consumption and saving it cannot be claimed with certainty that this means that interest rates have risen relatively more than the rate of wage increases, but it certainly seems plausible. At least if the memories of investors are not too short. If investors then base their expectations on extrapolation of the trends, a reinforcement of aggregate productivity deceleration by a negative term A would be very likely at least in the initial stages of a slowdown.

So far we have only established that the term A in condition (5.7) plausibly may reinforce deceleration. What about $\hat{\hat{R}}$ and conditions (5.9) and (5.10)? Observe that W/D is bounded downwards, more exactly our assumptions require $X/R < 1$ implying $W/D > 1/\alpha$, so the ratio cannot fall at an accelerated rate indefinitely, sooner or later its fall must be retarded. Given that it falls, i.e. that $\hat{W} - \hat{D}$ is negative, it seems not unreasonable to conjecture it should be getting less and less negative the closer we come to the bound, i.e. to guess that $\dot{\hat{W}} - \dot{\hat{D}} > 0$. Both W and D falls given our general assumptions on the direction of change in price expectations, $\hat{W}$ must then fall faster than $\hat{D}$ initially if we start from W/D constant, hence $\dot{\hat{W}} - \dot{\hat{D}} < 0$ initially. Both terms are negative so $\alpha\dot{\hat{W}} - \dot{\hat{D}}$ may well be positive, and as the ratio comes closer to its lower bound that becomes likelier. Of course this only justifies a loose conjecture that the magnitude of any positive contribution from $\hat{R}$ becomes smaller and smaller. So far I have not been able to conclude anything more specific than that.

It is interesting to note here the empirical investigations of vintage structures at the industry level by F. Førsund and L. Hjalmarsson(1987), more specifically the Swedish dairy, cement and pulp industries and the Norwegian aluminum industry. Just looking at their diagrams over changes in input coefficients confirm that a more or less pronounced flattening of the structures took place during the 70's. G. Eliasson and T. Lindberg(1988) show a similar flattening of the distribution of rates of return on capital at the micro level in Swedish industry. Both these findings would be consistent with a reduction in the ratio W/D in this model. It at least suggests that expectations may have been contributing to slow down productivity growth in this decade.

Summarizing this section, it clearly is possible that expectations may reinforce a deceleration if W/D is falling and also otherwise. However, such reinforcement may be counter-acted by second order changes in that fall, viz. if $\alpha\dot{\hat{W}} - \dot{\hat{D}}$. Since it seems plausible that the decreases in W and D slows down because W/D must be bounded from below, a fair guess may be that the magnitude of any counter-action is rather small and decreasing in comparison with the first order changes.

Although no definite assertion can be made, the arguments in the typical back-flat supply case points to the conjecture that expectations are likely to have a retarding effect on productivity growth, in exactly those circumstances when characteristics of the capacity distribution would promote a slowdown. That is to say that changes in expectations are likely to reinforce echo effects.

4.6 Extensions of the model

In this section we will discuss how relaxations of some of the simplifying assumptions will affect the results. First we examine the effect of a growing labour supply. Then we let the elasticity of substitution in the *ex ante* production function differ from unity. Finally the relation between labour productivity and total factor productivity is discussed.

4.6.1 Labour supply growth

Growth in labour supply would reduce the amount of scrapping that is necessary to free labour for any given addition of capacity in the front and thus reduce capacity at the extensive margin less for a given investment. On the other hand it would tend to lower capital intensity by keeping wages down and thus lower the labour productivity of new facilities. At the same time the average would be taken over a larger labour supply, tending to dampen productivity growth.

Starting from (3.1)

$$\frac{\mathrm{d}}{\mathrm{d}t}(\hat{\Pi}) = \frac{\mathrm{d}}{\mathrm{d}t}(\hat{F} - \hat{V})$$

we first assume $\hat{V}$ to be fixed so the above is equivalent to

$$\frac{\mathrm{d}}{\mathrm{d}t}(\frac{\dot{F}}{F}) = \frac{\ddot{F}}{F} - \hat{F}^2 \qquad (6.1)$$

Since, when F is twice continuously differentiable,

$$\ddot{F} = \frac{\mathrm{d}}{\mathrm{d}t}(F_t + F_V\dot{V}) = F_{tt} + (2F_{tV}\dot{V} + F_{VV}\dot{V} + F_V\ddot{V}) \qquad (6.2)$$

where the parenthesis, call it A, divided by F is the new contribution to changes in aggregate productivity aside from $\frac{F_{tt}}{F} - \hat{F}^2$ due to changes in labour supply. Using

$$\dot{\hat{V}} = \frac{\ddot{V}}{V} - \hat{V}^2 = 0 \iff F_{VV} = -\frac{R_V}{R^2} \qquad (6.3)$$

and the second order partial derivatives of F, the first from the law of motion (3.11) and the other two directly from the marginal productivity condition (3.6)

$$F_{tV} = \frac{vR_V}{R^2} \qquad F_{V_t} = -\frac{R_t}{R^2} \qquad F_{VV} = -\frac{R_V}{R^2} \qquad (6.4)$$

we can write

$$A = \frac{\dot{V}}{R}(2v - \dot{V})\frac{R_V}{R} + \hat{V} \qquad (6.5)$$

If labour supply is growing at a constant rate and the input coefficients in the front is decreasing strictly, all new labour will be used in the front provided investment is sufficiently high to absorb it. Under that assumption $v \geqslant \dot{V}$ and it is clear that A is a positive contribution to aggregate productivity growth. Even if that assumption does not hold, it is clear that investment must be very low indeed to make the contribution negative. That it may happen is however clear by considering the extreme case that no new investment is made and labour growth is absorbed by reinstating scrapped equipment with lower productivity. Then of course aggregate productivity would fall.

If $\hat{V}$ varies we will have a contribution to the LHS of the basic condition amounting to

$$B = \frac{A}{F} - \dot{\hat{V}} = \frac{\dot{V}R_V}{FR^2}(2v - \dot{V}) + \left[F_V - \frac{F}{V}\right]\frac{\ddot{V}}{F} + \hat{V}^2 \tag{6.6}$$

Since $F_V < F/V$ the second parenthesis will always be the opposite sign of $\ddot{V}$ while the first term is positive as is the third when labour supply grows and $2v > \dot{V}$. So B is possibly negative only if $\dot{V}$ is high relative to v and/or $\ddot{V}$ is strongly positive. I.e. high and accelerating labour supply growth may decelerate aggregate productivity growth if investment cannot be kept high enough to absorb the labour supply growth.

Finally it should be noted that labour supply growth will also affect $R(V,t)$ and thus, even if $F_{tt}/F_t < 0$, influence how that translates into conditions on the capacity distribution and its relation to changes in the technique factor. It is easily verified that (4.3) still holds for a varying V, but (4.5) now reads

$$\hat{\varphi} + \hat{\alpha} = e(R)\hat{R} + \hat{R}_t = -e(R)\hat{w} + \hat{R}_t \tag{6.7}$$

since $\hat{\hat{R}} \neq R_t/R$ now. First we observe that (6.4) implies that

$$-R_t = -vR_V \text{ so } \dot{R} = (1 - \frac{\dot{V}}{v})R_t \equiv yR_t \tag{6.8}$$

where y simply is the flow of labour from obsolescent equipment compared to total front labour flow. In order to compare (6.6) to our previous formulation we consider the difference

$$\hat{R}_t - \hat{\hat{R}} - \hat{R} = \frac{\dot{R}_t}{R_t} - \frac{y\dot{R}_t + \dot{y}R_t}{yR_t} = -\dot{y} \tag{6.9}$$

From this then follows that

$$\hat{\varphi} + \hat{\alpha} = e(R)\hat{R} + \hat{R} + \hat{\hat{R}} - \hat{y} = -(1 + e(R))\hat{w} + \hat{\hat{w}} - \hat{y} \tag{6.10}$$

Since

$$\hat{y} = \frac{\dot{v} - \ddot{V}}{v - V} - \hat{v} = \frac{\hat{v}\dot{V} - \ddot{V}}{v - V} = \frac{\hat{v} - \ddot{V}/\dot{V}}{v - \dot{V}}\dot{V} \tag{6.11}$$

where the denominator normally would be positive. We can conclude that, as long as front labour flow grows faster than total labour supply growth accelerates, labour supply growth will tend to diminish $\frac{F_{tt}}{F} - \hat{F}^2$ and thereby offsets the positive contributions from labour supply growth.

It cannot be ruled out that a labour supply growth that is high enough and accelerating strongly enough may tend to slow down productivity growth compared with the situation when labour supply is fixed. This is partly due to the possibility that not all new labour is absorbed in front production but some will actually be used for reinstatement of previously scrapped equipment. Although it cannot be disregarded without specifying the determinants of investment it seems safe to conclude that labour supply growth will in general work to accelerate labour productivity growth, but since there are also mechanisms working to the other direction the contribution to higher productivity growth ought to be relatively minor in normal circumstances when labour growth is less than growth in production.

4.6.2 A general *ex ante* production function

The assumption of unit elasticity of substitution and constant cost shares will be consistent with a wide range of measured substitution elasticities, since we have two margins here, one intensive ruled by the front production parameters and one extensive ruled by the capacity distribution characteristics. Recall that we make no assumption that the current structure is generated by a specific front production function with a form that remains stationary over time. We have only assumed the current production function to be of Cobb-Douglas type. Now we relax that assumption and allow the *ex ante* production function to have non-unit and even a changing elasticity of substitution. But we keep the constant returns to scale assumption.

We keep labour supply and W and D fixed like in section 4.4, and begin by establishing a relation between changes in the cost share and wages. By logarithmic differentiation of the ratio of factor shares in front production

(6.12) $$\hat{\alpha}(1 + \frac{\alpha}{1 - \alpha}) = \hat{v} - \hat{k} + \hat{w} = (1 - \sigma)\hat{w}$$

which implies that

(6.13) $$\hat{\alpha} = (1 - \sigma)\hat{w}(1 - \alpha)$$

Recall the following equations from the derivation of the basic condition in section 4.4.

(4.2) $$\frac{F_{tt}}{F_t} = \hat{\varphi} - \frac{\alpha D/W}{1 - \alpha D/W}$$

(4.5) $$\hat{\varphi} + \hat{\alpha} = e(R)\hat{R} + \hat{\hat{R}} + \hat{R} = -(1 + e(R))\hat{w} + \hat{\hat{w}}$$

(4.6) $$\hat{\varphi} = (1 - \alpha)\hat{k} - \alpha\hat{v} + \hat{\theta} = \hat{k} - \alpha\sigma\hat{w} + \hat{\theta}$$

Since wages in the model will increase monotonically with fixed labour supply the second term on the RHS of (4.2) will contribute to deceleration only if $\sigma < 1$. But the changing cost share might influence the relation between capacity flow and technical change, too.

Using the definition of the capital cost share, $k/(\varphi D) = 1 - \alpha$, we have

$$\hat{\varphi} - \hat{k} = \frac{\hat{\alpha}\alpha}{1 - \alpha} = \alpha(1 - \sigma)\hat{w} \tag{6.14}$$

implying that (4.6) can be rewritten as

$$\hat{w}(\alpha\sigma + (1 - \sigma)\alpha) = \alpha\hat{w} = \hat{\theta} \tag{6.15}$$

and we see that the relation (4.8) remains valid, so the basic condition in the form (4.10) will now translate to

$$-(1 + e(R))\frac{\hat{\theta}}{\alpha} + \hat{\hat{\theta}} - \hat{\alpha}(1 + \frac{\alpha D/W}{1 - \alpha D/W} < (1 - \alpha D/W)\frac{\varphi}{F} \tag{6.16}$$

since we now have a non-zero $\hat{\alpha}$ on the LHS of (4.5) or in still more fundamental terms

$$-(1 + e(R))\frac{\hat{\theta}}{\alpha} + \hat{\hat{\theta}} - \frac{(1 - \alpha)(1 - \sigma)}{\alpha(1 - \alpha D/W)}\hat{\theta} < (1 - \alpha D/W)\frac{\varphi}{F} \tag{6.17}$$

If $\sigma < 1$ we will then have a negative contribution to the LHS compared to the unit elasticity case. We can then conclude that $\sigma < 1$ will contribute to deceleration and vice versa.

4.6.3 Total factor productivity

The production function, $F(V,t)$, has no simple relation to the standard neoclassical one where production is a function of a capital stock and a labour flow. Although it is possible to define a measure of the vintage capital stock either in physical or value terms this measure will in general not be independent of the aggregate labour input, and therefore does not allow functional separation according to the Leontief(1947) aggregation theorems.[7] In general an aggregate production function hypothesis constructed on the basis of data from a vintage structure will tend to underestimate long run production possibilities in the economy that could be realized through investment since all aggregate observations will be well within the *ex ante* production possibility frontier. It will in general also distort the actual substitution and scale properties of the *ex ante* function, see Johansen(1972) for a more detailed discussion. Comparisons could easily be misleading and it is important to appreciate that the explicit aggregation in the above structural production functions makes the concept of technical change in the neo-classical production function very

[7] Cf. Nadiri(1970) for a short summary of other aggregation problems in connection to production functions.

different from the corresponding concept in the front production function.

Disregarding this aggregation problem we use the common growth accounting approach and write $\Phi(K, V, t) = F(V, t)$ as a linearly homogeneous function of some index of the capital stock and labour supply at a given time t. Assuming neutral technical change and profit maximization we can then calculate an accounting measure of total factor productivity growth, $\hat{\Theta}$. With λ as labours cost share we get

$$\hat{\Theta} = \hat{\Phi} - \lambda\hat{V} - (1 - \lambda)\hat{K} \tag{6.18}$$

Again assume zero growth in labour supply and add the assumption of a constant capital/output ratio, thus implying that a decrease in labour productivity growth here must be matched by an equal decrease in the rate of capital accumulation. Then (6.18) simplifies to

$$\hat{\Theta} = \lambda\hat{K} \tag{6.19}$$

implying that if the rate of capital accumulation is decreasing at a faster rate than increases in λ, total factor productivity will also slow down. Using the definition of $\lambda = \dfrac{wV}{F}$ we get

$$\hat{\lambda} = \hat{w} - \hat{F} \tag{6.20}$$

which is positive only if aggregate marginal labour productivity increases faster than average labour productivity. We can also write

$$\hat{\hat{\Theta}} = \hat{\lambda} + \hat{\hat{K}} = \hat{\hat{w}} - e(R)\hat{w} - 2\hat{F} \tag{6.21}$$

by (3.2) and (4.5) and the definition of λ. From this we can conclude that since $\hat{F} > 0$ and $\hat{w} > 0$ a positive elasticity of the capacity distribution will guarantee that total factor productivity growth will move in the same direction as labour productivity for some positive acceleration of technical change. Obviously this is only a sufficient and not necessary condition. Moreover, adding and subtracting $e(R)\hat{F}$ in the last member of (6.21) we easily see that $e(R) > 0$ actually will make growth in the labour share contribute to deceleration in Θ!

Note that a rising capital/output ratio,[8] $\hat{\Phi} - \hat{K} < 0$ would work in (6.18) to decrease the level of total factor productivity growth. If we assume that the capital ratio has been constant initially a rising capital/output ratio also must slow down capital accumulation less than the slowdown in production for some period of time. This gives a negative contribution to total factor productivity growth.

These calculations are only intended to show two things. First that deceleration in standard measures of total factor productivity growth may be explained by the same kind of mechanism as labour productivity deceleration in the

[8] Boskin and Lau(1991) for example finds a rising capital/output ratio when estimating a meta-production function over US, UK, West Germany, France and Japan.

model used here. Second that labour productivity in the context of heterogeneous capital structures is a natural productivity concept to use.

4.7 Conclusions

The results here show that in a simple model with heterogeneous capital, acceleration in the rate of technical change can take place at the same time as a deceleration in the growth rate of aggregate productivity. An elasticity of the capacity density that is not too negative, or equivalently a supply schedule that is not too convex will guarantee this when expectations are fixed. Moreover, the larger the gap between the highest and the lowest input coefficient or equivalently the higher the capital elasticity of the front production function, the more convex the supply schedule may be for given rates of change in technique, and still aggregate productivity growth would slow down. Changes in expectations may work in either direction, and results so far do not support any definite conclusion, although it is not too far-fetched to conjecture that shortening of expected life times will tend to reinforce deceleration tendencies.

Of course, the capital structure may also be such as to enhance productivity acceleration and compensate for retardation in technical change. This paper has concentrated on the slowdown aspect here since mechanisms like this may have contributed to the productivity slowdown in the 70's at least in the initial stages. If that is so, the other side of the coin would lead us to believe that the echoes from high investment activity in the 60's may reach us in the 90's. Until empirically verified this is of course purely speculative, but the possibility seems to justify some effort to be spent on empirical work about irregularities in capital structures.

The main importance of this theoretical exercise is, however, not these specific results, but the demonstration that the historically given capital structure of an economy may crucially determine the transmission from enlarged production possibilities to actual economic productivity growth measures. Expectations of future price movements can both reinforce and attenuate these echoes from the past. To the extent that such expectations are based on the recent past they will probably tend to reinforce slowdowns and speedups at least in the initial stages.

The growth depressing effect overtaking accelerated technical change reveals important economic effects from investment decisions taken one or two generations ago. It seems worth pointing out two things.

First. It may very well take a long time – on the order of centuries – before the final effects of technical changes are reached, presumably long after the equipment implementing it was scrapped, because price responses to imbalances in the capacity distribution will result in further imbalances in the structure. Thus any anomalies in the history of the economy will tend to be more or less reproduced later on when equipment installed during an anomaly is scrapped. Furthermore, by the time final effects are approached further changes have occurred imposing their own adjustment paths on the original one. The comparison of steady state paths should therefore be expected to yield very limited information about the short and medium term effects of any sizable change in the rate of technical efficiency growth.

Second. It bears stressing that the slowdown in the model is not due to any economic inefficiency. On the contrary, given price expectations, the representative firm acts optimally within the given framework and a social planner would face essentially the same mechanisms. If a faster transmission of technical change into productivity change is desirable for some reason, any policy aiming to increase the speed of that transmission must take account of the dependency on the existing structure and should not be expected to admit generalization to rules only dependent on current macro variables. E.g. if aggregate productivity is depressed because of a transition from backflat to backsteep regions of the supply schedule and for some reason a planner would like to speed it up, subsidizing capital costs would perhaps be improductive in the short run, since it would work against the transfer of labour. On the other hand it could prove highly efficient in order to boost productivity growth in the neighbourhood of the converse transition. Of course, subsidies affect the economy in many other ways so any policy recommendations would have to take many more mechanisms into account than this very imprecise sketch.

Further research would probably be well spent on disaggregation, at least into a capital and a consumption goods sector, since the relation between the prices of these two sectors may play a crucial role in determining aggregate productivity. The rate of aggregate productivity growth would then depend on which sector it is that have the fastest change in technical efficiency.

The results here only answer the question how, at a specific moment, the capital structure transmits technical change into productivity growth. It does not say anything specific about the duration of such a relation or the long run path of the economy. This is a very essential question, which may gain some illumination by simulation studies even if the dynamic model, as can be suspected, turns out to be analytically intractable.

Of course, since the productivity slowdown is the obvious inspiration for this work, empirical testing of the degree to which the hypothesis can explain real data must be high on the research agenda. Since the essential feature of the vintage model is its covariation in variables separated by long but varying periods of time, standard econometric techniques seem ill suited to the task of analyzing changes in this time structure. Spectral analysis and similar techniques may be a better choice.

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Appendices

1. List of variables used

For ease of reference the notation for economic variables and functions that are used in the main text is listed here.

Latin letters

D	present value of one investment unit over expected life time
e	elasticity of the capacity density in the input coefficient domain
f	*ex ante* production function
F	aggregate output as a function of aggregate labour
k	real investment
K	capital stock measure
l	life time of capital equipment
P	present value of expected profit
r	interest rate
R	maximal labour input coefficient
t	current time
v	labour flow in new investment
V	total labour supply
w	real wage
W	present value of expected wage changes
X	minimal labour input coefficient
y	ratio of labour released by scrapping and labour absorbed in front production.

Greek letters

α	labour elasticity of front production function
θ	technique factor
Θ	total factor productivity
λ	labours income share in the aggregate
ν	rate of wage change
ξ	labour input coefficient
Π	average labour productivity in total production
σ	elasticity of substitution
τ	time index of vintage
φ	capacity flow in new investment
Φ	aggregate production as a function of capital and labour
ψ	{ production capacity density over input coefficients
Ψ	accumulated capacity distribution

Top notation:

$\dot{x}$ time derivative of x
$\ddot{x}$ second time derivative of x
$\hat{x}$ logarithmic time derivative of x
$\hat{\hat{x}}$ second order logarithmic time derivative of x
$\tilde{x}$ expected value of x

2. The basic condition in terms of the minimal input coefficients.

Equation (4.5) will hold under the assumptions in section 4 if we interchange the maximal labour/output ratio for the minimal one. This is obvious since the Cobb–Douglas assumption fixes the ratio X/R. But it may be instructive to derive this independently. From (3.7) it follows that the front rate of change in labour flow

(A2.1) $$\hat{v} = \frac{\psi'(X)}{\psi(X)}\dot{X} + \hat{X} + \frac{\ddot{X}}{\dot{X}} = (e(X) + 1)\hat{X} + \frac{\ddot{X}}{\dot{X}} \text{ when } \dot{X} < 0$$

By noting that $\hat{\hat{X}} = \ddot{X}/\dot{X} - \hat{X}$ this can be rewritten as

(A2.2) $$\hat{v} = (2 + e(X))\hat{X} + \hat{\hat{X}}$$

and since $\hat{X} = \hat{v} - \hat{\varphi}$

(A2.3) $$\hat{\varphi} = (1 + e(X))\hat{X} + \hat{\hat{X}}$$

so the changes in flow variables will be dependent on elasticities of the density function as well as the second order proportional changes in productivity and it follows, when $\hat{X} = \hat{R}$, that the elasticities in both ends of the utilized part of the capacity distribution must be equal. Note that, this holds even if V is variable while (4.5) holds only for V fixed.

3. Convexity of present value function

It is convenient here to take advantage of the constant returns to scale we have assumed in the front production function and work with the average labour productivity π as a function of capital intensity $\varkappa$ and define present value per labour unit p by

(A3.1) $$p(\varkappa)v = P(k,v) \text{ i.e. } p(\varkappa) = \pi(\varkappa)D - wW - \varkappa$$

and maximize this instead with the first order condition

(A3.2) $$p'(\varkappa) = \pi'(\varkappa)D - 1 + \pi D_\varkappa - wW_\varkappa = 0$$

where D and W depends on $\varkappa$ via $\tilde{l}$. By the scrapping condition we have

(A3.3) $$w\tilde{v}(t + \tilde{l}) = \pi(\varkappa)$$

It is now easy to verify by differentiation that $W_\varkappa = \tilde{v}(t + \tilde{l})D_\varkappa$ so the last two terms will always cancel out. That could also have been inferred by the envelope theorem. But the problem with dependence on capital intensity in the expectations will recur in the second order derivative and it is this that causes the present value function to possibly fail concavity even if the ex ante production function is concave by definition.

(A3.4) $$p'' = \pi''D + \pi'D_\varkappa$$

The second term here may be positive and hence outweigh the first term, thus indicating a convex present value function. Obviously this depends crucially on how expectations react to changes in $\varkappa$. Within our unspecified expectation framework it is not possible to be sure about the effect. But as Bliss(1968) have shown it is possible to say somewhat more on a steady state path with perfect foresight. Then $v(t + l) = e^{\beta l}$ where we skip the tilde since expected life time equals actual life time, which is fixed, as is the technical change parameter β. Using the scrapping condition (A3.3), and differentiating both sides w.r.t. $\varkappa$ it follows

(A3.5) $$l_\varkappa = \frac{\pi'}{\pi}$$

and differentiating D then yields

(A3.6) $$p'' = \pi''D + \pi' e^{-rl} \frac{\pi'}{\pi} < 0$$

as a condition of strict concavity. Without specification of the production function this still does not guarantee concavity. There are examples of concave production functions that do not fulfill this requirement. But with our Cobb-Douglas assumption, $\pi = \varkappa^{1-\alpha}$ it is possible to be more specific.

(A3.7)

$$p'' = -D\alpha(1 - \alpha)\pi/\varkappa^2 + e^{-rl}\pi(1 - \alpha)^2/\varkappa^2 = (1 - \alpha)\frac{\pi}{\varkappa^2}(-\alpha D + e^{-rl}(1 - \alpha))$$

so if

(A3.8) $$e^{-rl} < \frac{\alpha D}{1 - \alpha}$$

p will be concave in $\varkappa$. $D = (1 - e^{-rl})/r$ on the steady state path which in general will be substantially higher than 1. But given only that $D > 1$ it will be enough if $\alpha > 0.5$ and for any reasonable values of r and l it can be substantially less.

Although this does not prove the concavity of p on a general path it at least seems reasonable to guess that the problem with convexity is less likely to occur when we use a Cobb–Douglas *ex ante* production function and restrict the labour elasticity to empirically reasonable values.

4. Conditions for a falling ratio W/D

Recall the definition of W and D from (3.13)

$$D = \int_t^{t+l} e^{-\int_t^z r(x)dx} dz \text{ and } W = \int_t^{t+l} v(z) e^{-\int_t^z r(x)dx} dz$$

where $v(z)w(t) = w(z)$, where we to simplify notation skip the tilde on expectational variables.

We parametrized the expectations functions, by assuming that a constant average rate of interest is used as well as a constant expected exponential rate of wage increases. I.e. we wrote in (5.11)

$$D = \int_0^l e^{-rz} dz = \frac{1 - e^{-rl}}{r} \text{ and } W = \int_0^l e^{(\beta - r)z} dz = \frac{1 - e^{(\beta - r)l}}{r - \beta}$$

Differentiating W and D w.r.t. time and dividing through we got (5.14)

$$\hat{W} - \hat{D} = \left[\frac{W_l}{W} - \frac{D_l}{D}\right]\dot{l} + \frac{W_\beta}{W}(\dot{\beta} - \dot{r}) - \frac{D_r}{D}\dot{r}$$

and also (5.15)

$$\beta \dot{l} + \dot{\beta} l = \hat{W} - \hat{D}$$

and assumed (5.16):

$$\hat{r} = \eta \hat{\beta} \text{ so } \dot{r} = \eta \frac{r}{\beta} \dot{\beta}$$

deriving then (5.17)

$$\hat{W} - \hat{D} = \left[\frac{W_l}{W} - \frac{D_l}{D}\right]\dot{l} + \left[\frac{W_\beta}{W}\left(\frac{\beta - \eta r}{\beta}\right) - \frac{\eta r D_r}{\beta D}\right]\dot{\beta}$$

and (5.18)

$$\hat{W} - \hat{D} = \frac{al - b\beta}{a - \beta}\dot{\beta} \text{ if } a \neq \beta$$

1. We will now prove that the denominator is strictly negative and the sign of the numerator will be the opposite of the sign of $1 - \eta$.

(A4.1)
$$a - \beta = \frac{e^{(\beta - r)l} D - e^{-rl} W - \beta D W}{DW}$$

where the sign is determined by the numerator, which when expanded reduces to

(A4.2)
$$r(e^{(\beta - r)l} - e^{-rl}) - \beta(1 - e^{-rl}) < 0 \text{ iff}$$
$$\frac{r}{\beta} < \frac{e^{rl} - 1}{e^{\beta l} - 1}$$

which will hold true as long as $\beta < r$ since then $\int_0^l e^{\beta z} dz < \int_0^l e^{rz}\, dz$.

2. We then proceed to the second assertion and prove $al - \beta b$ to have the opposite sign of $(1 - \eta)$

$$al - \beta b = le^{-rl}\left[\frac{e^{\beta l}}{W} - \frac{1}{D}\right] - \left[\frac{\beta - \eta r}{W}\left[\frac{W}{r-\beta} - \frac{le^{(\beta - r)l}}{r-\beta}\right] - \frac{\eta r}{D}\left[\frac{le^{-rl}}{r} - \frac{D}{r}\right]\right] =$$

(A4.3)
$$= le^{-rl}\left[\frac{e^{\beta l}}{W}\left[\frac{r(1-\eta)}{r-\beta}\right] - \frac{1-\eta}{D}\right] - \frac{(1-\eta)\beta}{r-\beta} =$$

$$= (1-\eta)\left[\frac{rle^{-rl}(e^{\beta l} - 1)}{(1 - e^{(\beta - r)l})(1 - e^{-rl})} - \frac{\beta}{r-\beta}\right]$$

Taking the expression in brackets and multiplying with $1 - e^{(\beta - r)l}$ will not change its sign, if $\beta < r$, and we get

(A4.4)
$$rl\frac{e^{\beta l} - 1}{e^{rl} - 1} - \beta W = \beta\left[l\frac{\int_0^l e^{\beta z} dz}{\int_0^l e^{rz} dz} - W\right]$$

Obviously, when $\beta = r$ the bracket above vanishes, since W then equal l and the ratio is unity. Otherwise we know the integrands to be strictly positive and can rewrite the bracket in (A4.4) once again, using B for the integral involving β and P for the one involving r

(A4.5)
$$lB - PW = H$$

The sign of H will determine the sign of $al - \beta b$ when η is given. We will prove that H is negative in the interior of the set $\Omega = \{ (r,\beta) : 0 \leqslant \beta \leqslant r \}$. It is easy to verify that $H = 0$ when $\beta = 0$ or $\beta = r$. We will proceed by examining how H changes when we increase r for any fixed positive β. Note that the first term in (A4.5) depends only on β and the second only on r. Hence only PW changes as we increase r, keeping β fixed. We shall show that PW increases monotonically with r, and so H must be negative in the interior of Ω.

2a) First, noting that the partial derivative must be taken before evaluating the expression

$$\frac{\delta}{\delta r}(PW)\bigg|_{\beta = r} = l\int_0^l ze^{rz}dz - \frac{l^2}{2}\int_0^l e^{rz}dz = \frac{l}{r}(le^{rl} - P - \frac{rl}{2}P) =$$

(A4.6)
$$= \frac{l}{2r^2}(rle^{rl} - 2e^{rl} + 2 + rl) =$$

$$= \frac{l}{2r^2} \sum_{i=2}^{\infty} \frac{(rl)^i}{(i-1)!}(1 - \frac{2}{i}) > 0$$

since all terms of the Taylor-expansion is non-negative. So we then have established that *PW* increases as we cross the zero line of *H*. By continuity *H* will then be negative in an open set where $\beta < r$.

2b) To demonstrate that this open set in fact can be identified with the interior of Ω we shall show that the partial derivative of *PW* w.r.t. *r* remains positive throughout the interior.

(A4.6) $$\frac{\delta}{\delta r}(PW) = P_r W + W_r P > 0 \iff -W_r < \frac{W}{P} P_r$$

To determine this some lengthy algebraic exercises is needed. We start by

$$W_r = \frac{le^{(\beta - r)l} - W}{r - \beta} = \frac{1}{(r-\beta)^2}[(r-\beta)le^{(\beta - r)l} - 1 + e^{(\beta - r)l}] =$$

(A4.7) $$= \frac{e^{(\beta - r)l}}{(r-\beta)^2}[(r-\beta)l + 1 - e^{(r-\beta)l}] =$$

$$= -\frac{e^{(\beta - r)l}}{(r-\beta)^2} \sum_{i=2}^{\infty} \frac{((r-\beta)l)^i}{i!} =$$

$$= -e^{(\beta - r)l} l^2 \sum_{i=0}^{\infty} \frac{((r-\beta)l)^i}{(i+2)!} = -e^{(\beta - r)l} l^2 \ S_1$$

which clearly is negative. Note the shorthand for the sum! In an analogue manner we have

(A4.8) $$P_r = \frac{le^{rl} - P}{r} = \frac{1}{r^2}(rle^{rl} - e^{rl} + 1) =$$

$$= \frac{1}{r^2} \sum_{i=2}^{\infty} \frac{(rl)^i}{(i-1)!}(1 - \frac{1}{i}) = l^2 \sum_{i=0}^{\infty} \frac{(r-l)^i}{(i+1)!}(i+1) = l^2 S_2$$

obviously positive. Next step is

(A4.9) $$e^{(r-\beta)l} W = \frac{e^{(r-\beta)l} - 1}{r - \beta} = l \sum_{i=0}^{\infty} \frac{((r-\beta)l)^i}{(i+1)!} = lS_3$$

and

(A4.10) $$P = \frac{e^{rl} - 1}{r} = l \sum_{i=0}^{\infty} \frac{(rl)^i}{(i+1)!} = lS_4$$

It is easy to verify that condition (A4.6) is equivalent to

(A4.11) $$S_1 < \frac{S_3}{S_4} S_2$$

This is still hard to determine so some further work is needed

(A4.12) $$S_1 S_4 - S_2 S_3 = S_1(S_4 - S_2) - S_2(S_3 - S_1)$$

Now

(A4.13) $$S_4 - S_2 = \sum_{i=0}^{\infty} \frac{(rl)^i}{(i+1)!}(1 - \frac{i+1}{i+2}) = \sum_{i=0}^{\infty} \frac{(rl)^i}{(i+2)!} > 0$$

and so have the same form as S_1 . We also have

(A4.14)

$$S_3 - S_1 = \sum_{i=0}^{\infty} \frac{((r-\beta)l)^i}{(i+1)!}(1 - \frac{1}{i+2}) = \sum_{i=0}^{\infty} \frac{((r-\beta)l)^i}{(i+2)!}(i+1) > 0$$

turning out to have the form of S_2 . By comparison term by term we easily verify that

$$S_3 - S_1 > S_1 \text{ and } S_2 > S_4 - S_2$$

(A4.15)

$$\text{or } \frac{S_1}{S_3 - S_1} < 1 \text{ and } \frac{S_4 - S_2}{S_2} < 1$$

Dividing through in (A4.12) by $S_2(S_3 - S_1)$ we do not change sign so

(A4.16) $$\frac{S_1(S_4 - S_2)}{(S_3 - S_1)\, S_2} - 1 < 0 \implies S_1 < \frac{S_3}{S_4} S_2$$

We have then proved that PW increases monotonically with r in the set Ω for each fixed β. It follows that $lB - PW < 0$ in the whole interior of Ω.

We can then finally conclude that in Ω

(A4.17) $$\hat{W} - \hat{D} < (>)\ 0 \text{ iff } \hat{r} > (<) \hat{\beta}$$

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Stockholm, Sweden

ISBN 91-554-2850-9
ISSN 0585-539-X