Introduction

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The issue of firm growth – how it is achieved and managed, and what consequences it has for different stakeholders – is both theoretically interesting and practically important. It is also an area of scholarly enquiry that has expanded very significantly since we started doing research on it in the 1980s and 1990s. In this volume we present and comment upon the most recent contributions we have made to this field of inquiry – separately, jointly and with various colleagues (who are included in the ‘we/us = article authors’ used in the remainder of this introduction). While the chapters have been published before in various places, we think it valuable to gather them in one easily accessible place, which also allows space for our reflective commentary across the individual chapters. We hope readers will find the work a useful and worthwhile addition to the extant body of knowledge about firm growth. We also hope they will find that it – as its title suggests – brings new perspectives on firm growth and its study, and that it can inspire future contributions by other researchers. This is important, because despite the growing volume of research on firm growth, many important questions still lack satisfactory answers.

The current volume may be regarded as a follow-up of a previous collection where we – and Frederic Delmar – presented and commented on eight articles on (mostly small) firm growth that we had jointly or separately published up until that time (Davidsson et al., 2006). In that volume we organised the works under three broad themes: *the conceptual and empirical complexity of the firm growth phenomenon*; *growth aspirations and motivations*; and *patterns and determinants of actual growth*. The current volume builds on and extends these themes. Only one of the chapters in the previous volume directly addressed the issue of drivers of actual growth. We add three more in this book, two of which expand on the ‘aspirations and motivations’ theme by relating growth aspirations and motivations (or lack thereof) of the owner-manager to the actual growth achieved in the subsequent period. We present these three chapters in *Part I – Explaining why and how much firms grow*.

Not least because of the ‘conceptual and empirical complexity’, it has

been a growing insight of ours that research on firm growth needs to move beyond attempts at trying to explain variance in the (total) amount of growth firms achieve, undertake or undergo. Therefore, *Part II – Changing the firm growth research agenda* presents three chapters providing critical reviews of prior research (including our own) and the growth indicators it has used as well as outlining a research agenda to address the problems and gaps identified through these reviews.

The last two parts and sets of chapters outline beginnings of such a changed research agenda by addressing antecedents and effects of different forms (or modes) of growth; and the outcomes of growth. As our examples far from exhaust these topics, we use somewhat narrower part titles: *Part III – A critical look at the growth–profit relationship* and *Part IV – Theory-driven research on specific forms of growth*. These parts comprise two chapters each.

In the remainder of this Introduction we provide a short summary of each part and the chapters within them, organised in the order they appear in the volume.

PART I – EXPLAINING WHY AND HOW MUCH FIRMS GROW

In the previous volume (Davidsson et al., 2006) we provided as a background a discussion of our (including Delmar's) respective dissertation works. The first chapter in our current volume, 'Building an integrative model of small business growth', can be seen as the culmination of these works, as it is in effect a refinement of core contents of Wiklund's dissertation study (other parts of the study were published much earlier, e.g., Wiklund, 1999). The work draws on five different streams of literature to arrive at an integrated model that considers individual (growth attitude), environmental (industry; hostility-munificence-dynamism) and firm-level (resources; strategy) drivers of growth. The model puts strategy – specifically, entrepreneurial orientation (Lumpkin and Dess, 1996) – at the centre stage as the most direct driver of growth, but allows for direct effects of individual and environmental factors as well in the empirical analysis.

To us, this chapter represents an example of useful phenomenon-driven research (cf. Wiklund et al., 2011). It is decidedly phenomenon-driven but not atheoretical; instead it takes on the task of integrating several theoretical perspectives. It uses a large sample representing a select set of different industries and size classes, thus allowing for broad generalisation while avoiding some of the heterogeneity and micro-firm dominance of simple random samples. It employs a longitudinal design, thus separating the

actual growth in time from its theorised antecedents. It lets both theory and data speak by allowing revision of the theoretical model. It assesses growth with multiple indicators rather than a single measure that may be differentially valid across a broad sample. Finally, it explains a sizeable share of the variance of 'total' growth – not just a statistically significant but possibly practically irrelevant share of a variance that through the researchers' design choices has already been made much smaller than that occurring in the real economy. We agree that the type of carefully designed single-theory research on homogeneous samples of firms that is the current fashion in leading management journals can reach further and deeper as regards both measurement and estimation of the effects of the factors highlighted by that particular theory. However, we hold that it benefits the collective sobriety of academic research on firm growth to once in a while get an idea of where these factors sit in the bigger scheme of things. This first chapter of our current collection is arguably a good example of the latter.

In the previous volume we included two chapters investigating drivers of owner-managers' expectations and/or motivation for future growth (Delmar and Davidsson, 1999; Wiklund et al., 2003). While such studies may yield unexpected and sometimes provocative (to some) results – such as the expected financial outcome of growth being only of secondary importance – they leave unanswered the important question of whether the managers' stated expectations and motivations have any bearing on what actual growth materialises in the following period.

The second chapter, 'Aspiring for, and achieving growth: the moderating role of resources and opportunities' expands on the same theme. Here, we take a more explicit theoretical vantage point than in the *Theory of Planned Behavior* (TBP) (Ajzen, 1991). This also leads to searching for moderator variables representing TBP's notion of 'perceived behavioral control'. In short, we argue that growth aspirations positively affect growth, but more strongly so: (a) for managers with more human capital (HC), that is, more experience and education; (b) where the access to financial capital is better; and (c) under conditions of high environmental dynamism. Assessing growth with a four-indicator index reflecting sales and employment growth relative to firm size and relative to competitors, we find support for (a) and (c) but not for (b). Further, the introduction of the moderator variables led to very considerable increases in the variance explained. Thus, while knowledge about the managers' aspirations or motivations towards growth has some informational value, it appears at least as important to consider the conditions under which these aspirations are supposed to translate into actual growth. The results also shed light on the often surprisingly weak effects on performance that researchers

obtain for various aspects of HC (Davidsson and Gordon, 2012; Unger et al., 2009). The implication is that such research is often blind to what the managers are trying to achieve. Having, for example, pursued a long education does not force anyone to pursue growth. However, if a manager wants their firm to expand, then it appears that having extensive education or rich experience is helpful for making such ambitions materialise.

The third chapter, ‘The effect of small business managers’ growth motivation on firm growth: a longitudinal study’, was one of the first to address this problem by following up on the actual growth over the following three to four years. The motivation–actual growth relationship was estimated for two separate samples and in each case using two different measures of growth (in sales and employment, respectively). The analysis applies hierarchical cross-lagged regression with Heckman correction and inclusion of potentially important control variables, thus controlling for issues of reverse causality, survivor bias and alternative explanations. The results thus stand on fairly solid ground. These results show that while the influence is bi-directional (that is, growth in period 1 also influences growth motivation in period 2), growth motivation is a predictor of future growth over and above what past growth (and control variables) can explain. Further, at least with respect to employment growth, the motivation of the founder is the relatively more important predictor. This finding has important implications. First, studies of growth motivation are meaningful even if the ‘real interest’ should be in actual growth. Second, policies and pedagogical approaches that affect growth motivation are also likely to affect actual firm growth.

PART II – CHANGING THE FIRM GROWTH RESEARCH AGENDA

While the chapters in the previous part in our (possibly biased) view are examples of well-conceived and well-crafted research on their respective topics, they also represent ‘old school’ research on firm growth in two important ways. First, they are restricted to trying to explain variance in the ‘amount’ of growth. Second, although they may use more than one indicator of growth, they conceptually treat growth as a unified or undifferentiated phenomenon. Firm growth is *one* phenomenon, with assumed common antecedents and effects.

The critical reviews that we present in the current part call this into question. There may be more important questions and fruitful research avenues available to researchers than to try to further improve our ability to explain or predict differences in the amount of growth. For example,

growth is rarely a sound end in itself, so it would be useful to know under which conditions growth is actually conducive to achieving more terminal goals. Further, the fact that different measures of growth are only weakly correlated is not just a methods problem or peculiarity – it suggests that we are at least in part dealing with different phenomena, which require different theoretical explanations. On a more positive note, this insight also suggests there are great opportunities for researchers to make valuable contributions within a revised agenda of research on firm growth.

Chapter 4 ‘Are we comparing apples with apples or apples with oranges? Appropriateness of knowledge accumulation across growth studies’ first provides a review of past research. This review focuses on which *indicators* of growth researchers have used (sales, employment, assets, equity, profits, other); which growth *formulae* they employ (especially absolute vs. relative or percentage growth) and across what *time span* (for example, one-, three- or five-year periods) growth is measured. The most revealing and troublesome finding in relation to these method choices is that they appear to be made relatively independently of each other and independently of what theoretical perspective is used (Davidsson and Wiklund, 2000). This indicates that the research is often under-conceptualised.

The chapter then moves on to a very comprehensive, longitudinal investigation of how different measures of growth relate to each other. The shorthand summary of the results is that few correlations are above 0.5 and that most are low and many even negative. Interestingly, relative sales growth – often argued the ‘best indicator’ if one has to pick a single one – is *not* empirically the best compromise candidate for representing the phenomenon of growth ‘in its entirety’. Further, the many low and negative correlations suggest that such ‘entirety’ is a mirage: what the measures represent cannot reasonably be said to reflect one and the same (meaningful) theoretical construct. Hence the suggestion that ‘perhaps our theories are too broad, and by narrowing the boundary conditions, we are able to build richer theories that, when empirically tested, better describe *a particular aspect* of growth’ (p. 134, emphasis added).

This view is reinforced and elaborated in the next chapter, ‘Advancing firm growth research: a focus on growth mode instead of growth rate’. We here start out by observing that theoretical development has been notably slow in research on firm growth, Penrose (1959) still being something of a sole, bright star in otherwise rather dark skies. We then launch the suspicion that a major reason for this is that researchers have been too eager asking ‘how much’ before answering (or considering) the question ‘how’ firms grow.

This is then underpinned with a review of three streams of research. The first and largest is *growth as an outcome*. This stream is fraught with

problems indicated above: restricting the perspective to the question of ‘how much’; applying an undifferentiated view of this phenomenon while operationalising it in any number of ways; and turning a blind eye towards whether growth is always ‘good’ or not with respect to reaching more terminal growth. *The outcome of growth* theme is associated with the criticised ‘stages’ or ‘life-cycle’ stream of research (cf. Levie and Lichtenstein, 2010). We here note that the potential for practical relevance of this perspective is limited not just by lack of empirical evidence, but by limiting the perspective to assuming that the ‘problem’ at hand is one of an organisational unit that grows organically, leaving little room for acquisition, diversification, and so on. The main problem with the third stream, *growth as a process*, is that it is close to non-existent. Interestingly, the review of extant work includes two works from this volume (Chandler et al., Chapter 9; Lockett et al., Chapter 10) that could alternatively be regarded as research on types or modes of growth. This reflects the dual meaning of ‘how’ as ‘in what form?’ and ‘through what temporal pattern?’

Most importantly, the chapter suggests a future research agenda for each stream. For growth as outcome the suggestion is a focus on choice between, combination of and sequential pattern of growth modes. For the outcome of growth the managerial and performance implications of different modes of growth are highlighted – questions that arguably are of the highest practical relevance. For growth process research what is suggested can be characterised as it being time to do Penrose all over again, with deep empirical work and theoretical analysis starting from more current business realities.

The third chapter in the part, ‘Towards an integrative framework for future research on small firm growth’, is a ‘conclusions’ excerpt from a monograph based on a comprehensive review of the literature on (small) firm growth. Being based largely on the same literature as the previous chapter, and by researchers from a similar background, it is not surprising that some of the conclusions are the same. For example, the chapter’s suggested research agenda holds that while how antecedents relate to the ‘amount’ of growth ‘in general’ may have been the most common focus in past research, it is only one of at least nine possible foci for growth researchers, and not the most promising path for the future. We make the somewhat pessimistic but possibly realistic remark that ‘the most relevant “growth factor” in each individual case may be some idiosyncratic factor that is not even represented by the generic variables used in research [. . .] thus it can be questioned whether broadly based generalizations about the antecedents of growth can *ever* be precise enough to be of much immediate value for managers’ (p. 181). However, on a more optimistic note, we find reason to speculate that there may be more commonality across

cases in the managerial challenges that growth leads to, and therefore in this chapter we emphasise the need for research into the consequences of growth. This review reinforces the need for research focusing on particular forms or modes of growth. It also highlights that related literatures actually inform questions of growth even if growth is not regarded as a 'keyword' in the research. In particular, we discuss the potential in integration with research on internationalisation, but we also mention the integration potential in literatures on diversification and on mergers and acquisitions. In combination, the three chapters in this part point out a multitude of research opportunities to be seized by future research.

PART III – A CRITICAL LOOK AT THE GROWTH–PROFIT RELATIONSHIP

The 'integrative framework' excerpt commented on immediately above concludes 'future studies should either make a strong case for why firm growth is interesting in its own right, or explicitly include in the design those outcomes that growth is otherwise only assumed to lead to' (p. 182). In line with this advice, the chapters in this part investigate how growth relates to profitability. In the first chapter, 'Growing profitable or growing from profits: putting the horse in front of the cart?' we ask the questions: 'How do some firms manage to combine high growth with high profitability? Do they become profitable as a result of their growth, or do they attain high profitability before embarking on a high growth trajectory?'

Several theories suggest growth leads to high profitability. In a separate work (Davidsson et al., 2008) we also established that there is a pro-growth bias – a tendency to use growth as the sole outcome measure when what the researchers really are after is 'good performance' – in empirical research, and that this tendency is particularly pronounced in entrepreneurship research compared with research in management and strategy. In the chapter, we question whether the positive growth–profit relationship postulated in theories and assumed in much prior research generally holds up to scrutiny. Using the resource-based view as our theoretical frame, we argue that if firms try to grow without having unique resources (converted to market offerings that stand out), they would have to do so through price cuts and/or increased marketing, both of which would hurt profits. Instead, we argue, it is firms that first develop high profitability – reflecting having developed an attractive market offering – that can grow with sustained profitability. Empirical testing using two large data sets from different countries supports these ideas: firms in the enviable 'high

growth/high profitability' category are much more likely to originate from the pool of firms that first show high profitability at low growth than from those showing high growth at low profitability. Firms in the latter category instead often retract to a 'low growth/low profitability' situation. This pattern shows considerable robustness across different time spans and categories of firm grouped by industry, size and age. This gave us reason to conclude that '[o]ur findings are a strong reason for practitioners and researchers alike to question a universal and uncritical growth ideology' (p. 214). However, we do not regard our results as the final word and therefore suggest a research agenda (p. 210–13) outlining how to seek further corroboration, rejection or boundary conditions for our tentative conclusions.

The second chapter, 'Performance configurations over time: implications for growth- and profit-oriented strategies', expands on the themes of the previous chapter. Conceptually, it offers an integrated model of growth–profit dynamics, where the superior 'exploitation ability' of large established firms and the (sometimes) superior 'discovery ability' of small, young and independent organisations are important components. Empirically, the chapter extends the analysis of growth–profitability relationships as related to firm age, testing hypotheses regarding where younger and older firms are over- and under-represented in terms of position as well as movements within the two-dimensional growth–profitability performance space.

PART IV – THEORY-DRIVEN RESEARCH ON SPECIFIC FORMS OF GROWTH

The chapters in this final part represent what – based on the critical reviews discussed above – we believe to be one of the most important routes forward for research on firm growth: theory-driven research into the antecedents, interrelationships and effects of particular forms or modes of growth. The first chapter is titled 'Asset specificity and behavioral uncertainty as moderators of the sales growth–employment growth relationship in emerging ventures'. As we noted in Chapter 4, different indicators of growth represent different facets of the overall growth phenomenon. It also calls for future research to examine how different indicators might be conceptually linked to each other. The current chapter heeds this call and links employment growth and sales growth – the most popular growth indicators – conceptually and empirically. The research illuminates the suitability and limitations of using either as the sole indicator of a general construct of 'firm growth' in different

contexts. Specifically, we use transaction cost economics (TCE) reasoning to derive and test hypotheses regarding when growth in sales and employment are and are not likely to be highly correlated. Technically, we hypothesise and model this as indicators of asset specificity and behavioural uncertainty acting as positive moderators of the relationship between sales growth and employment growth. The analysis yields consistent support for these ideas. Perhaps most interestingly, however, while the results hold up for the full sample and for a subsample of firms operating in resource-scarce environments, the hypotheses are not supported in a breakdown analysis of resource-abundant environments. This arguably speaks to the boundary conditions of TCE and similar theories.

The title of the final chapter in our collection, 'Organic and acquisitive growth: re-examining, testing and extending Penrose's growth theory', specifies both the theoretical vantage point and the modes of growth addressed in the chapter. Some reflection suggests that organic and acquisitive growth are likely to pose very different management challenges, both as regards to how growth is achieved and what organisational and performance-related consequences expansion brings. It is therefore surprising how little theoretical and empirical attention has been paid to this important distinction in previous research on firm growth. Chapter 5 represents one of the few attempts to bring conceptual clarity to the issue. Further, Penrose (1959) discussed the two modes at some length and depth. However, while some of her ideas and concepts were useful for developing hypotheses for this chapter, she did not work out in detail how the two modes relate to each other.

In the chapter we use the concepts of productive opportunity set (POS) and adjustment costs (AC) to predict how organic and acquisitive growth, respectively, in one period affect the firm's ability to grow organically in the following period. In short, we hypothesise that both forms of growth are associated with ACs. However, while organic growth adds little to the firm's POS, acquisitive growth has the potential of making additions to POS that dominate the negative effect of ACs. Thus, the amount of organic growth in the first period would have a negative effect on organic growth in the following period, while acquisitive growth brings new opportunities for organic growth in the following period. The carefully conducted econometric analysis of a large, ten-year panel data set supports these ideas. An important managerial implication of this research is that acquisitions may be an underused route to growth among small- and medium-sized firms. Inspired by the apparent usefulness of the Penrosean theoretical frame, we also offer in this chapter some ideas about future extensions of Penrose's work.

REFLECTIONS ON THE FUTURE OF FIRM GROWTH RESEARCH

Each chapter contains recommendations for future research, many of them quite detailed. We will not repeat those recommendations here. Instead, we would like to take the opportunity to make some broad reflections about what we would like to see in the future. In essence, we advocate a redirection of firm growth research.

We would like to see less of research that attempts to test hypotheses related to explaining variance in growth rates across firms. Even if such research is theory-driven and uses the most appropriate statistical methods, we simply don't see that it will address or provide interesting answers to the most central questions related to understanding firm growth. For reasons discussed in this introductory chapter and detailed throughout the volume, we don't regard additional or more fine-grained explanations of how much firms grow as having the capacity to address the most pressing questions related to firm growth.

Instead we would like to see research that further develops firm growth theory. More precisely, we believe that much more work can be done in terms of extending and developing Penrose's theory. As some of the chapters discuss, research addressing 'how' firms grow in terms of growth modes or patterns is likely to lead to findings that are more exciting and radical theoretically, and more relevant and valid empirically, than is research related to explaining variance in growth rates across firms. Penrose's theory as it now stands is informative to this research, not least in terms of related and non-related diversification. However, there are certainly additional issues related to growth modes that her theory does not address or does not pursue in detail, which deserve theoretical attention. Extension of Penrose's theory into these areas should certainly be possible and feasible. We foresee that purely conceptual work, qualitative theory-developing work and quantitative theory-testing work all will play important roles in advancing such theory.

In a related manner, it appears that Penrose's theory has been tested and validated to a very little extent. In Chapter 10 we take an important step in this direction, but we believe that much more can be done. One option is to use the same approach as we do in Chapter 10 – to identify arguments in Penrose's theory that lend themselves to hypothesis formulation and explicit empirical testing. A careful reading of her works shows that it contains the substance for several testable hypotheses. Alternatively, it would seem highly fruitful to examine the extensive existing empirical body of firm growth research and to reinterpret it from a Penrosean perspective. Reinterpretation of existing results, re-analysis of resisting samples and

cross-study meta-analyses seem like feasible routes for providing important insights into the validity of the Penrosean approach to firm growth.

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