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MSc THESIS IN FINANCE

Initial Public Offerings in Sweden: the Effect on Employment and Corporate Performance

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ABSTRACT

This paper investigates the effect of Initial Public Offerings (IPOs) on employment and operating performance of Swedish firms. In order to capture the true effect of IPOs we match firms that underwent an IPO with similar firms that opted to remain private. This approach allows us to correct for firm and industry characteristics and general economic trends. By means of a difference-in-difference regression we find that IPO firms that undergo an IPO on average grow substantially faster than their private peers in the IPO year and the years following the IPO. We measure growth as an increase in number of employees, net sales, and total assets. We also find this excess growth does not translate into an excess increase in profitability, as measured by return on assets, nor do we find a significant change in financial leverage for the sample as a whole. We perform additional triple difference regressions to test whether the IPO year, age of the firm, and exchange venue impact post-IPO performance. Apart from the year of listing exhibiting an effect on leverage, we find no conclusive evidence to substantiate the effect of the other factors. Our study highlights the importance of well-functioning public markets and IPOs for the overall economic development of Sweden.

Keywords: Initial Public Offerings, Employment, Operating Performance, Public Markets, Economic Development

Acknowledgements: we would like to thank our tutor Ramin Baghai for his guidance and feedback. Also, we are grateful to Björn Hertzberg and Ulf Persson of Nasdaq OMX Stockholm for providing us with critical data input for our research, as well as the Swedish House of Finance for providing access to the Serrano Database.

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1. Introduction

The recent financial crisis and the subsequent prolonged global recession have revitalized the interest in employment policies and the effects of corporate policy on national economic performance. Policy makers have been concerned with the lower number of initial public offerings over the past years, and the effect this may have on employment and revenue growth. In order to address this concern, the Kaufmann Foundation commissioned Kenny et al (2012) to investigate the effect of US initial public offerings between 1980 and 2010 (a period over which average annual new listings fell from 298 to 90) on the US economy as a whole. Their paper attributes firms that underwent an IPO to be responsible for a significant number of jobs created throughout the US over the past decades, as do a number of other studies (e.g. Haltiwanger et al, 2013) and (Borisov et al, 2015).

This study aims to add to the existing body of literature by investigating the effect of IPOs on corporate performance and employment growth in Sweden. Sweden has one of the most complete freely accessible databases on performance of both public and private firms available. This unique facet enables us to employ a matching methodology where we match firms that opted to go public with private 'control' firms, based on certain firm characteristics such as the number of employees, the industry code, and profitability. This allows us to distinguish between growth that can be attributed to regular business cycles or the effect of the economic climate as a whole, and growth caused directly by the decision of the firm to go public. By isolating this 'IPO effect' we are able to provide novel insight into the functioning of the Swedish public markets, elaborate on the merits of IPOs for the Swedish economy, and link our findings to other phenomena such as 'hot and cold' IPO cycles, operational leverage vis-à-vis financial leverage, and the functioning of Sweden's relatively new public growth market First North.

Taking a closer look at the Swedish economy, currently a total of 1,158,349 companies are registered in Sweden and 71,668 new firms were established last year (2014)[1]. In line with most other advanced economies Sweden is service-oriented with 62.4% of companies active in this sector. Agriculture, forestry and fishery is the second largest category (23%), followed by industrial companies (14%). Out of the newly started firms about 60,000 are within services while 10,000 are industrials. In terms of size, 98.18% of the companies have less than 50 employees and only 0.08% have more than 500 employees. Small companies with less than 50 employees contribute to 31% of the total jobs while the large companies (+500) employ around 50% of the working population. Between 2008 and 2013

[1] Statistics Sweden, 2014 – www.scb.se

the number of employed people increased by 209,509 - about 80% of that increase is attributable to firms with 200 employees or less. With these numbers in mind we set out to assess to what extent IPOs contribute to the Swedish economy compared to their private peers.

The notion that companies that undergo a public offering grow quicker in terms of sales and employees has been investigated on multiple occasions (e.g. Takahashi and Yamada, 2015 and Borisov et al, 2015). The question we aim to answer is whether this growth would have occurred in these companies irrespective of going public or not. By means of a matching methodology employed in various corporate-event driven papers (e.g. Boucly et al, 2011), we aim to match our IPO observations with comparable private entities. The key assumption of the matching methodology is that it is possible to estimate the counterfactual outcome as if a treated observation (a firm undergoing an IPO) had *not* been treated, by using an observation from a control group with the same, or as similar as possible, characteristics as those of the treated observation. The data sample we are using covers firms that went public in Sweden between 1999 and 2012. Our sample window covers the four years around the IPO. By means of the matching methodology we are able to match 65 IPO firms with a total of 223 control firms, an average of approximately 3.38 control firms per IPO.

Subsequently, we perform a difference-in-difference (DiD) regression using return on assets, number of employees, net sales, leverage, and total assets as dependent variables. This provides us with an estimate of the average difference in corporate performance between the IPO firm and its control firms in the years surrounding the IPO. We find that companies that are subject to an IPO on average experience a 70.8% higher employment growth than the firms in the control group in the period around the IPO. This result is in line with Borisov et al (2015), who found that companies that go public have a higher overall employment growth as well as a higher growth than a group of matched control firms. Similarly, we find that firms that underwent an IPO on average increase their sales with 157.8% more than their matched control firms and increase their asset base with 85.6%. On the other hand, we do not find a significant effect on profitability, as expressed by return on assets. Leverage appears to be negatively affected by the decision to go public, though this may simply be due to a higher equity level as a result of the IPO. We expand our model by investigating possible differences between post-IPO performances of different sub-groups in our sample (triple difference regression), compared to their respective control groups. As opposed to other studies (e.g. Kenny et.al, 2012), we do not find a significant relation between age of the firm and post-IPO performance. Similarly, we do not see a significant effect when we correct for the year of listing or the type of exchange the firm lists on.

The existing body of literature on initial public offerings comprehensively cover the underlying reasons as to why and when companies opt to go public - the main contribution of this paper is that it is (as far as we know) the first paper investigating the effect of IPOs on employment and sales growth in Sweden. We add to the overall diversity of the literature by employing a novel matching methodology not commonly used in studies on post-IPO corporate performance. We do note that, notwithstanding the merits of our methodology, the use of observational data inherently caries a risk of introducing endogenous bias in a research design. This factor, combined with the relatively small size of the total Swedish IPO market (and hence our sample), warrants our paper suitable to be considered on descriptive rather than causal merits.

2. Previous Research

In the following section we elaborate on various theories related to public firms and the decision to go public. We discuss the motivations underlying the decision to going public, the types of companies that opt to go public in various geographical markets and the circumstances in which they go public. We also elaborate on how employment theory relates to IPOs.

2.1 IPO Rationale

In spite of the importance of job growth for Sweden's economic growth and overall welfare, there are few academic papers elaborating on the possible role of IPOs in these matters. Although similar research exists for other countries, for instance for the US market (Borisov, 2015), this paper is the first attempt -as far as we know- to investigate the relation between the IPO decision and employment growth in the Swedish market. There is a large body of existing literature elaborating on the reasons for companies to go public. An influential paper on this topic by Pagano, Panetta and Zingales (1998) found there are various factors that influence the likelihood of a company going public, for instance the company's size, market-to-book ratio, and rebalancing of their accounts *after* a period of high investment and growth. However, over the years a number of other theories have been proposed to explain the rationale behind the IPO decision. As a result, there are various explanations on the impact of IPOs on employment.

A commonly provided reason for firms to consider an IPO is an improved access to capital markets. More flexibility between alternative means of funding give a firm an advantage compared to their privately held peers. A consequence of a more flexible capital structure can be access to a great amount of external funding and/or lower costs of credit (Rajan, 1992). A large study by Kim and Weisbach (2006), covering 17,226 IPOs from 38 countries, showed that in the year following an IPO firms increase R&D expenditures with 18.5 cents and capital expenditures with 9.9 cents for each incremental dollar raised during the IPO. This effect rises to respectively 78 and 19.9 cents when measured over the four year period post-IPO. This indicates that firms specifically aim to obtain investment related financing as a motivation for their equity offerings. In a paper published by Brau and Fawcett (2006) several theories on IPOs are tested against industry practice. The authors conclude that the primary motivation for going public is to facilitate acquisitions. Other concerns which are considered include overall market conditions and the prospect of yielding part of the decision-making control and ownership. Celikyurt et al (2010) also conclude that "newly public firms make acquisitions at a torrid pace". The authors argue this also holds when comparing newly public firms to more mature firms in the same industry; as such they conclude the effect to be largely industry independent.

Given the influx of capital following an IPO, as well as the subsequent improved access to capital markets, it could be argued that one may expect IPO companies to pursue growth opportunities, either organically or, as mentioned in the previous paragraph, through mergers and acquisitions. One may expect the growth of capital to be supplemented by a growth in investments and as such by an increase in the number of employees. The theories above assume the rationale behind IPOs is primarily to gain improved access to capital markets and a desire to exploit new growth opportunities, which should lead to employment growth at the time of the IPO and the period following the IPO.

However, other theories attempt to explain the rationale behind IPOs from a different standpoint. For instance, an influential paper by Ritter (1991) concludes firms take advantage of periods, or "windows of opportunity", in which investors are temporarily overoptimistic about the earning potential of young growth companies. Ritter's paper builds on the body of literature discussing cycles in IPO popularity. A study by Helwege and Liang (2004) elaborates on these 'cold' and 'hot' IPO markets. Based on their research, covering 6,419 IPOs in the US market between 1975 and 2000, Helwege and Liang conclude that the characteristics as well as the quality of companies that decide to go public do not differ much in hot or cold markets. They find that companies that IPO in either hot or cold markets are similar in most respects: firms are approximately the same age when going public, do not exhibit faster sales or profit growth in the post-IPO years, and while IPOs in hot markets tend to have lower earnings they also have lower average capital expenditure and R&D ratios. The notion that hot markets are more attractive for IPOs of start-up companies only draws weak support based on their sample. What appears to be key for the rationale of firms to go public is the willingness of investors to purchase IPO stocks, rather than particular firm characteristics. Alti (2006) finds that although a larger stake of equity is issued by firms during hot markets than during cold markets, this increased cash inflow does not result into larger investments in the IPO year and the years following the IPO. If the decision to go public is motivated by any of the aspects discussed above one should not necessarily expect an increase of employment growth. To properly assess employment growth as a result of IPOs it is not only of importance to consider the rationale behind IPOs, but also to elaborate on the type of companies that tend to go public.

2.2 Type of Companies Going Public

In order to assess the effect of IPOs on the economy in terms of growth in employment it is of importance to understand which types of companies are most likely to go public at a certain stage of their life. Whereas some industries may be characterized by a relatively high level of fixed assets, R&D costs or capital expenditures, others may be known for its rapid asset turnover, high growth, and lean

business models. Each of these factors may have an effect on overall employment as well as employment growth. For instance, between 1980 and 2014 Ritter (2014) registered 8,060 IPOs in the US, of which 2,961 (37%) could be classified as technology companies and 628 (8%) as bio-technology firms. As such these two industries would have a significant influence on Ritter's (2014) estimates of IPO performance and employment growth.

Maksimovic and Pichler (2001) elaborate on the type of companies seeking to obtain either private or public financing. They show that the decision to obtain private or public financing depends on a number of factors: the public perception of the industry's viability, the probability a superior technology will emerge, and the initial cost of R&D that must be paid by new entrants. Companies are most likely to seek public financing if they belong to industries which are perceived to be viable, in which there are low first-stage costs, and the probability of being displaced by more technology advanced rival firms is low. Carpenter and Petersen (2002) find that industries, such as the high-tech industry, in which returns are highly variable, information is asymmetrical, and a lack of collateral is common, lead to a limited access to debt markets. For firms belonging to these types of industries the equity market in form of IPOs is an important source of funding and permit a significant increase in firm size. Poulsen and Stegemoller (2008) compare 1,074 IPOs from 1995 through 2004 with 735 acquisitions of private firms by public companies from the same period. What they conclude is that firm characteristics play a significant role in the decision to either go public or to be acquired by another firm. Firms that go public through an IPO tend to be high-growth firms with higher valuation ratios and, similar to what Carpenter and Petersen (2002) found, limited access to debt markets. Also, firms that IPO tend to face capital constraints, have fewer intangible assets, are less likely to be in the development stage, and are more commonly backed by venture capital firms.

Apart from the notion that the majority of firms that choose to go public exhibit similar characteristics, it could also be argued that management may influence the decision to go public. Chemmanur and Paeglis (2005) investigated this phenomena and reached a number of conclusions, for instance: controlling for firms size and firm quality, firms with better and more reputable managers tend to have larger IPO offer sizes. Also, firms with superior management tend to correlate with more reputable underwriters (underwriter reputation has a positive effect on long-run firm performance of IPO stocks (Carter et al, 1998). Furthermore, underwriting expenses are negatively correlated to management quality and reputation. Most strikingly, management quality and reputation explain some of the cross-sectional variation in the post-IPO stock returns and post-IPO operating performance. With the understanding that firms that choose to go public may exhibit particular treats, it is also important

to elaborate on whether there are differences between the types of firms that create jobs and have a higher than average employment growth rate.

2.3 IPOs and Employment – Prior Evidence

Job growth and the contribution of small and medium sized enterprises (SMEs) to the economy is a widely debated issue in many countries. In a large study Ayyagari et al (2011) elaborate on the contribution of SMEs to total employment, job creation, and employment growth across 104 developing countries. Their main conclusion entails that small firms (<20 employees) have the smallest contribution to total employment compared to medium (20-99 employees) and large companies (100+ employees). SMEs (<99 employees) in aggregate are comparable in their contribution to overall employment. However, the authors also find small firms contribute more to job growth than large firms, i.e. their total stake of employment is significantly smaller, but growth rates are substantial. Small firms also experience higher sales growth and employment growth. However, whereas it can be concluded from this that small firms are important to the overall economy, it is also found that they suffer from a lower productivity growth compared to their larger counterparts (Ayyagari et al, 2011).

Looking at Sweden in particular, in the period from 1960 to 1990 the country used to be characterized by a large fraction of tiny firms (1-9 employees) as well as very large firms with more than 500 employees. Henrekson and Johansson (1999) studied this phenomena and attributed the size distribution of firms to institutional factors that tampered growth in small firms and did not allow them expand to out of their size class. Performance was also poor among small firms (10 to 199) employees during this period. In this respect the Swedish distribution of companies differed from that of many other European countries, which were characterized by fewer small firms (10-99 employees) and more large firms per capita. In a later study conducted in 2012, Henrekson et al (2012) found that the traditional relationship had changed and employment among firms with 10 to 199 employees had grown between 1993 and 2009. They also concluded that the prospects of growing and expanding a company were substantially more favorable than 20 years ago. Currently the distribution of companies of different size as measured by employees in Sweden is in many respects comparable and similar to other European countries such as the Netherlands, Great Britain and Germany.

In a study aiming to provide nuance to the debate on the role of firm size in employment growth in the US, Haltiwanger et al (2013) argue the relationship between size and employment growth is more complex than usually propagandized by, primarily, policymakers. Even though some evidence exists to support the popular perception that net employment growth rates tend to be higher for smaller than

for larger firms, this relationship is biased by neglecting to sufficiently take into account the return to the mean effect in regressions. The authors, using a different method, find the same relationship to be far less robust than results found in other literature. The authors show that a key component is the role of firm age. In the US, start-up firms account for approximately 3% of employment any given year – conditional on survival these start-ups contribute significantly to overall job creation. As such, the authors conclude it is not primarily size that is of critical importance to employment growth. Policymakers should instead focus more on age and the development of companies from start-ups to fully mature firms. A study by Hesmati (2001) on the relationship between the size, age and growth for Swedish firms provided inconclusive results - the author concluded that results are sensitive to the estimation method and definition of growth and size. In terms of employment there was a negative correlation with age, while assets and sales correlated positively with the age of the firms.

In the progress of moving from the start-up stage to becoming a mature company a selection of firms decides, at some point in their life, to go public via an IPO. In investigating how the decision to go public impacts growth, Carpenter and Rondi (2006) looked at the characteristics and post-IPO performance of Italian firms. The research concluded the companies that went public could be categorized as either an 'Old style' firm or a 'New style' firm, the former being an established firm with controlling shareholders that seek to diversify their wealth and maximize their IPO proceeds, and the latter being firms that use equity to facilitate expansion and use rebalancing of their capital structure. Conclusively, the research shows that going public does *not* necessarily guarantee faster growth or faster job creation. A recent paper by Takahashi and Yamada (2015) looks at the operating growth of public and private firms over a 30 year time period. What they find is that compared to private firms, over time, excess growth in profitability and productivity that firms experience after going public diminishes to normal levels of growth. Excess growth in sales and number of employees however is an effect that can be observed for a prolonged period of time after the IPO.

In a policy oriented report for the Kaufmann Foundation, Kenny et al (2012) investigated the US IPO market between 1996 and 2010. Similar to Carpenter and Rondi (2006) the authors group the IPOs into 'Emerging Growth Companies' (EGCs) and 'other' firms. The EGCs aggregated growth in employees over the entire period was 156% compared to only 29% for the 'other' firms. Sales growth of EGCs was 259% compared to 78% of the 'other' firms. It should be noted that there were a few large outliers in the EGC sample: companies such as Amazon, eBay, Google, and Texas Roadhouse heavily impacted growth figures. The authors also found that growth in employees was largest during the 5

years post-IPO. For all IPOs between 1996 and 2005 the annual growth amounted to an average of 6.7% and an average of 10.1% for EGC firms.

In a paper investigating the relationship between the going public decision and employment growth Borisov et al (2015) look at a substantial sample of IPOs between 1980 and 2010. Apart from looking at the change in employment for the sample as a whole, the authors also segregate the groups based on their dependence of external equity financing, acquisition behavior, VC backing, default risk, and age when becoming a public firm. They argue private firms that are younger at the time of their IPO may be more growth oriented and capital constraint. As such, this type of firm should be in greater need of human capital and post-IPO use proceeds to hire new employees (Borisov et al, 2015). This hypothesis is confirmed since the authors found that firms with a private age below the sample median experience a greater increase in employment levels than firms with above-median private age. The authors conclude the average firm in their sample increases employment with 43% during its IPO year, followed by 36% year in the year post-IPO. Interestingly, the growth experience in the year preceding the IPO is also a significant 33%. The average growth in the three years post-IPO equals 8.7%, dropping to 5.9% on average at 5 years post-IPO. It was also found that employment growth in the period around the IPO positively relates to operating performance, firm value, and long-term post-IPO survival prospects.

With regards to profitability, early literature on public offerings has shown that firms that list experience a lower operating performance measured as operational revenue over assets post the IPO event (Bharat & Omesh, 1994, Mikkelson et al, 1997). This effect has a tendency to be long-lasting and may be observed for several years. Bharat & Omesh (1994) provide one potential explanation, namely reduced governing by the owner as a result of a lower stake in the company following the IPO. Mikkelsson et al (1997) contradicts this finding and find no evidence that reduced director and management ownership is related to the diminishing performance, but rather that diminishing performance is an effect of firm characteristics such as increased size and age. Also, they find that existing shareholders offering a significant number of shares in the IPO has a pronounced negative relation with performance post-IPO, possibly due to existing shareholders assessing the offer price as high compared to the value of the firm.

3. Data and Empirical Design

3.1 Data Construction

In order to properly assess the relationship between the going public decision and subsequent effects on certain parameters, for instance employment growth, we evaluate a set of IPOs that took place in Sweden between 1999 and 2012. To establish this sample NASDAQ OMX kindly provided us with all data on listing events on public exchanges in Sweden over the aforementioned time period. The data provided by NASDAQ OMX includes all listings on the main list, separated in three categories of large, medium and small cap companies, and NASDAQ's growth market, First North. First North was founded in 2006 to offer smaller companies the advantages of being publically traded on a MTF (multilateral trading facility) while having the benefit of a more lenient regulatory environment without imposing the substantial costs associated with being listed on the main market. Commonly, smaller and rapidly growing companies choose to list on First North – at a later stage in their life cycle they may opt to relist on the main exchange. Each firm listed on First North is required to have a "Certified Adviser" that ensures compliance with the requirements provided by the exchanges[2].

Figure 1 shows the total number of listing events on the exchanges per year in three different categories: spin-offs, re-listings (including secondary listings), and new issues. As shown IPOs make up 157 out of 321 listing events, approximately 48.9% percent, or an average of 10.47 IPOs per year. In order to analyze the true casual effect of going public on employment growth our analysis will solely include companies from the 'new issues' category. We exclude companies that are relisted or obtain a secondary listing on the Stockholm indices because it is likely that these companies already absorbed, to a certain degree, the effect of going public around the time of their initial listing. Similarly, spin-offs are often originally part of a larger publically traded firm and hence it can be argued that these firms also already, to a certain extent, experienced the possible effects of going public. Furthermore, for our purpose it is of importance to obtain information on the IPO firms of several years prior to listing. For spin-offs this information is often not readily available. Also, past operational performance of spin-offs may not be fully comparable to that of stand-alone firms, as the spin-offs were part of a larger corporation. As such the spin-offs may not have solely acted as a profit maximizing entity but instead fulfilled a particular role in the broader operations of the larger conglomerate.

In Figure 1 one can observe clear trends in IPO activity throughout our sample time frame. IPO activity peaks in the years 1999-2000 and 2006-2007, whereas in the intermediate periods activity is

[2] Nasdaq – First North - http://www.nasdaqomx.com/firstnorth

lower. This trend is in line with studies elaborating on 'cold' and 'hot' IPO markets and particular windows of opportunity (e.g. Ritter, 1991, and Helwege and Liang, 2004). Do note that as of 2006 onwards results include listings on 'First North', the newly founded Swedish growth market. Since the focus of this paper is to investigate economic growth in Sweden measured through job creation, we further reduce our sample size by excluding companies with a foreign country of origin, or companies of which the vast majority of daily operations take place abroad. Also, we omitted companies which were delisted within one year of their respective IPO year, as we cannot measure any effect of the IPO over such a brief timeframe. Reasons for early delisting can be for example a bankruptcy or a rapid acquisition of the company by another firm. These criteria combined further reduce our sample size to a total of 119 IPOs.

After establishing the initial sample group we sought to identify our sample of IPO companies in the Serrano Database[3], which is a database with financial history on corporate level. The data in the Serrano Database is primarily based on records from the Swedish Companies Registration Office (Bolagsverket). Additionally, the database contains information from the Statistics Sweden Group (Statistiska Centralbyrån) and data from the PAR register. PAR, a Bisnode subsidiary, updates the database semi-annually. The distinct advantage of the Serrano Database it is completeness and sheer size. Whereas information of companies at their private stage is often missing in databases such as Compustat, the Serrano database offers a comprehensive overview of both private and public companies in Sweden. It contains annual records as of 1998 onwards. In order to identify our IPO companies in the Serrano database we could not match on name of the firm, as firms in the Serrano Database do not always have the same name as in the data provided by NASDAQ OMX. Therefore, we manually identified the organization number of the IPO firm through the IPO prospectus, or, if the prospectus was unavailable, through the Swedish Tax Office (Skatteverket). As a final option we resorted to annual reports and company websites. By this means we were able to match 110 firms with entries in the Serrano Database. We subsequently further reduced our sample by taking out companies with less than five employees in the year before its IPO. This last criterion reduced the sample down to 102 IPO firms.

[3] SHoF – Serrano - http://houseoffinance.se/wp-content/uploads/2014/02/serrano120815engt_ny_v3.pdf

3.2 Building the Control Group

The goal of our research is to properly assess the effect of an IPO event on a company's operations in the years following the IPO, i.e. compare the treatment effect with the non-treatment effect, which is the decision to remain private rather than go public. Naturally, it is not possible in practice to observe both the treatment effect and non-treatment effect for the same company. This is referred to as the Model of Potential Outcomes, first introduced by Rubin (1974):

$$Y_{it} = D_i * Y_{it}^T + (1 - D_i)Y_{it}^C \qquad (1)$$

where the outcome of Y_{it} of observation i at time t it the result of treatment Y_{it}^T or non-treatment Y_{it}^C. D, being a dummy variable of one or zero, indicates whether an observation has been treated or not. If one were to believe an IPO event was truly random the above problem would not be of relevance, since it would be sufficient to compare the ex-post outcomes of firms that underwent an IPO event at a particular point in time to those firms that did not undergo an IPO event. However, due to the fact that we are using observational data rather than completely randomized data we opted to employ matching methodology, a technique popularized by Rosenbaum and Rubin (1983) in order to reduce the bias caused by non-random treatment. The key assumption of the matching methodology is that it is possible to estimate the counterfactual outcome as if a treated observation had *not* been treated, by using an observation from a control group with the same or as similar as possible characteristics as those of the treated observation. Formally, this can be expressed as followed (Heckman et al, 1998):

$$\mathrm{E}(Y_0|D = 1, X) = E(Y_0|D = 0, X) \qquad (2)$$

where the outcome of Y should be independent of the status of the treatment variable, D, the IPO event in our case. This only holds when the observed covariates, given by X in the equation above, does not contain variables that affect both D and Y (D=0). If this assumption is met the accurate use of matching methodology yields an estimate of the average treatment effect on the treatment group. Ideally, one would seek to match treatment firms with control firms on all possible variables – this 'exact matching' unfortunately becomes practically impossible for a dataset with a large set of control variables (Imai, 2005). As such we opted to match on a number of variables in a method similar to the one employed by Boucly et al (2011). In order for observations to be considered a 'matching company', they need to meet the following criteria: (1) it has the same first two digits of the company's Swedish Industrial Classification (SNI) code as the IPO firm, (2) the number of employees one year prior to the IPO falls within the a 50% range of the employment of the IPO firm one year prior to the IPO, (3) the

ROA, defined by EBITDA over Total Assets, one year prior to the IPO falls within the a 50% range of the ROA of the IPO firm one year prior to the IPO.

The choice for ROA and employment figures as matching criteria is motivated by the fact that both are suitable proxies for respectively profitability and size, and both variables tend to mean revert. We opted to match on variables in the year *prior* to the IPO rather than the year of the IPO, because this reduces the likelihood of any effects of the IPO already having been captured. A paper applying a similar methodology is the current working paper of Borisov et al (2015). In the latest version of their paper the authors match a sample of IPO firms on comparable IPO firms based on IPO year, industry, and the propensity to go public, which is defined as function of sales, employment, and growth rates in sales and employment. Interestingly, Borisov et al (2015) also have the possibility to match their sample with firms that planned to IPO but opted to withdraw their listing prior to going public. The authors argue these firms are comparable in their motivation to go public to the actual IPO firms – by also controlling for unfavorable market circumstances (which may prompt a firm to not go public) the authors seek to capture the realization of the effect of going public on employment growth.

Employing the matching methodology we were able to match 76 IPOs out of our total sample of 102 firms with at least one control firm, resulting in an average match rate of 74.5%. In total we matched 1,296 control firms with the 76 IPO companies, an average of 17.05 per IPO firm. A number of IPO firms (32 out of 76) are matched with more than five control firms. This poses a challenge, since there is a trade-off between the number of suitable control observations and the reliability of these control observations. A larger number of accurate matches should lead to a more reliable measurement of the possible effect of an IPO, however, one also wants to ensure similarity of the control observations to the IPO observations, where ideally one would only want to select only the most similar control observations. A common approach is to rank each control observation on a distance scale and subsequently only select the ones closest to the treatment observation on the distance scale. There are a variety of models to measure distance, most based on Mahalanobis' model (1936), and the propensity score model (Rosenbaum and Rubin, 1983). We have opted to select only the five matching observations with the smallest distance to their respective IPO observation, where distance is defined as followed:

$$D = \frac{(ROA^i - ROA^c)^2}{(ROA^i * B^2) * 2} + \frac{(Emp^i - Emp^c)^2}{(Emp^i * B^2) * 2} \qquad (3)$$

Where D is a distance indicator with minimum of zero and a maximum of one, zero being an exact match with the treatment observation. ROA^i represents the ROA of the IPO firm, ROA^c the ROA of the control firm, Emp^i is the number of employees of the IPO firm, Emp^c is the number of employees of the control firm. B equals 0.5, which represents the 50% bracket to determine eligibility of an observation to be treated as a control observation, as discussed in the previous section. This approach yields us a total of 272 matched observations, or an average of 3.57 for each of the 76 IPO observations. The average (mean) distance indicator equals 0.2615 out of a maximum of one, which shows the majority of control observations are in the lower end of the allowed 50% deviation range. We seek to assess employment growth between two years prior to the IPO to two years post-IPO. In our further analysis we opted to exclude firms for which we miss one or more observations in this timeframe. This applies to eleven firms in total; nine firms were only founded one year prior to the IPO, and two firms went public in 2012 and hence did not yet report on their performance two years post-IPO at the time of writing. This brings our final sample to 65 IPO firms. Figure 2 gives an overview of the construction of our sample, as described in the paragraphs above. In Table 1 we illustrate the accuracy of our matching methodology by comparing the means of the two continuous variables we matched on (next to the categorical variables pre-IPO year and Industry Code) of both our treatment group and the matched control group. As can be seen the difference in mean of both the number of employees and the ROA exhibit no significant difference. We verified this by means of a paired t-test on the year prior to the IPO, which is the year we matched the observations on. For both variables the null hypothesis of there being no significant difference in mean between treatment and control groups cannot be rejected.

Furthermore, as can be observed from Table 2 the other variables of our control sample are also fairly similar to the treatment sample in the year pre-IPO. As mentioned earlier, the 'number of employees' is highly comparable, with a median value of 92 employees for IPO firms in the year pre-IPO, compared to 89 employees for a control firm. Sales and EBITDA appear to be slightly higher for the IPO firms, though leverage is fairly similar. IPO firms also have a higher median value of assets, though the average value of assets is higher for the control firms. Even though the control firms in our sample serve as suitable proxies for IPO firms that would have decided to not go public, it is imperative to discuss the limitations of this particular method. As mentioned earlier, when one does not include all covariates as matching factors, there is a risk of a certain level of endogeneity due to omitted variable bias (Imai, 2005). This can be defined as the risk of excluding variables that affect the outcome of a firm in both the scenario in which it decides to stay private or decides to go public. However, the

decision to go public is not fully exogenous and can therefore at present not be fully explained, nor can all variables be included, by a single model. For example, a company may be prompted to IPO due to favorable growth prospects unique to that company. By matching a number of variables we have aimed to reduce the effect of endogeneity in further analyses, but nonetheless any result should be considered on its descriptive merit rather than its causal value.

3.3 Data Comparison

In evaluating our data in a global context we notice several distinct differences compared to data from different markets. The most evident difference is the number of observations in our study compared to larger studies on for instance the US market. Borisov et al (2015) conducted a study on the effect of IPOs on employment in the US market between 1980 and 2010. In total the dataset used contained 7,953 offerings, of which they were able to compute employment growth for 3,657 observations. The average number of IPOs per year over the 2001-2008 period was 157 firms per year, compared to an average of approximately 10.4 firms going public in Sweden. However, when considering the size of the US and Swedish economy (the Swedish GDP is approximately 3.5% of that of the US[4]), the Swedish IPO market appears to be the more active one. Earlier it was discussed that Swedish IPO activity was substantially higher in 1999-2000 and 2006-2007 (Figure 1), compared to the other years in our sample period. As can be seen in Figure 3 the US market exhibits a similar trend. The number of IPOs in 1999-2000 is substantially higher than the long-term average. However, the number of IPOs in the years 2006-2007, albeit higher than average, is not as pronounced as the difference from the long-term average as observed in Sweden for these years. Borisov et al (2015) find the average age of a firm before going public is 16.24, compared to 14.11 years in our sample. The median age however is very comparable, respectively 8 years in the US to 8.5 years in Sweden. Furthermore, the typical US firm has a median of 240 employees at the year-end before its IPO, compared to 89 in Sweden. When considering employment growth figures both US and Swedish firms display impressive results, as also further discussed in the next chapter. US firms boasts a growth rate in the IPO year of 24%, compared to 32.5% for Swedish firms. The annualized growth rate from the IPO to the year-end post IPO is respectively 23% and 19.68%. The growth rate from IPO to two years post-IPO is respectively 19% and 22.9%.

As highlighted by Carpenter and Rondi (2006) a variety of factors may be of influence on the type of firms applying to go public in different countries. Examples referred to in their study, which

[4] World Bank Data - http://data.worldbank.org/data-catalog/GDP-ranking-table

considers Italian public firms in a global context, include the costs of going public including the level of underpricing, listing requirements, public disclosure requirements, and the corporate tax structure in particular countries. Particularly referring to employment, Carpenter and Rondi (2006) find that the median number of employees at the time of going public is 579 for Italian firms, compared to 118 for Swedish firms. The median age of Italian firms is no less than 23 years, which is relatively old compared to the earlier mentioned 8 years for US firms and 8.5 years for Swedish firms. The median growth rate of employment for Italian firms is approximately zero on the year of IPO, 2.5% in the year post-IPO, -2.7% two years post-IPO, and approximately zero three years post-IPO. This is a significant difference compared to the earlier discussed growth rates of US and Swedish firms. The authors elaborate on possible reasons for this discrepancy referring to size as a possible explanatory variable, as well as the motivation for firms to go public. Another reason may be the nature of a country's economy. For instance, in the US substantially more high-tech firms find their way to the exchange than in Italy, which may materially impact the median number of employees at IPO as well as post-IPO growth rates. With these factors in mind, it is important to consider the context of an economy as a whole when evaluating the effects of IPOs.

As discussed in the previous section, in the US between 1980 and 2014 Ritter (2014) registered 8,060 IPOs, of which 2,961 (37%) could be regarded as tech companies and 628 (8%) as bio-tech firms. A similar pattern is evident in our sample of Swedish firms where five industry categories (Figure 4), including tech and bio-technology, make up the majority of all listings. Manufacturing on the other hand, which traditionally used to be a major category, only accounts for three of the 65 listed firms. Notable is also the relatively high number of wholesale firms, which could be a country specific factor due to for instance the success of major public retail companies in Sweden such as Hennes & Mauritz (H&M). The other top-five categories are Financial Services and PR and Consulting.

3.4 Empirical Design

In the following section we elaborate on the various types of regressions used in this paper. In the next chapter we first analyze the effect of going public on employment by means of a series of graphical representations of the employment and sales growth of both IPO and control firms over the period of four years around the IPO event. Sequentially, we formally test the observed effects by means of a typical difference-in-difference (DiD) technique that was popularized in papers such as Ashenfelter and Card (1985) and Card and Kreuger (1994). Our specific regression model can be formalized as followed:

$$Y_{i,t} = \alpha + \gamma POST_{i,t} + \delta IPO_i + \beta(POST_{i,t} * IPO_i) + \varepsilon_{i,t} \qquad (4)$$

where Y is the dependent variable we are evaluating, for example employment, variable i represents the company in the sample, and t is a time-factor expressed in years. In the above equation the term IPO is a dummy which equals one for firms that undergo an IPO event and zero for firms that serve as control companies. POST is another dummy that equals one for IPO firms for an observation which occurs after the IPO event and zero if the observation occurs before the IPO. For control firms the dummy is one if its corresponding IPO observation occurs after the IPO event, and zero if its corresponding IPO observation occurs before the IPO event. The β represents the difference between the treated and non-treated (respectively IPO and control firms) change in the dependent variable (for example number of employees) over the course of the investigated time-frame, and can be formally presented as followed:

$$\beta = \left(\bar{Y}_{B,2} - \bar{Y}_{B,1}\right) - \left(\bar{Y}_{A,2} - \bar{Y}_{A,1}\right) \qquad (5)$$

where B is the IPO firm variable and A is the control firm variable. 1 indicates the pre-IPO window and 2 the post-IPO window. Our regression includes both company and time fixed effects to control for firm and time specific effects that may otherwise bias results. Adding firm fixed effect subsumes one of the constituent of the interaction terms (the IPO dummy) which is therefore not reported in the results tables, the reason for this being that the dummy is time-invariant. The first series of regressions considers the number of employees, net sales, ROA, leverage, and total assets as dependent variables. Since the lower boundaries of net sales, employees, and total assets are zero, and hence positively skewed, we use the natural logarithm of both of these variables. Essentially, key in this type of regression and the matching methodology discussed earlier, is the assumption that in the absence of an (IPO) event the trend in the dependent variables would be equal for both the IPO firms as their control firms (Angrist and Pischke, 2008). By means of our regression method (Equation 4) we are able to capture the deviation in the dependent variables as caused by the IPO. Figure 5 illustrates the exact effect we aim to isolate by means of the regression.

An influential study by Bertrand et al (2004) showed a majority of published papers in six major journals between 1990 and 2000 failed to address the issue of serial correlation in difference-in-difference regression, which led to bias and severe underestimation of the standard errors of the estimators. In their study they found 5% significance in 45% of the placebo interventions used. We will address this issue by means of clustering the error terms on a firm-level, as suggested by Bertrand et al (2004). This method is shown to be valid as long as the error terms are independent and identically

distributed, and is commonly applied by papers using a similar methodology to ours (e.g. Boucly et al, 2011).

Another key issue concerns the length of the pre-IPO and post-IPO event window. For a number of firms in our original sample we would miss observations if we were to extend our current sample time-frame (which ranges from two years prior to the IPO to two years post-IPO). An explanation for missing observations can for instance be a bankruptcy. However, it is also possible that a firm seizes reporting due to for example a complex restructuring or a merger, as discussed by Davis et al (2014). After such an event a firm may still continue operations but is inherently difficult to distinguish as a separate entity, and therefore we would be unable to track the growth of the original company. Various studies use different methods to deal with this problem. For example, in their policy paper Kenney et al (2012) chose to lock employment figures at the latest known year if a firm has been acquired, and elect to input zero employees if the firm has not been acquired but has been delisted. As mentioned earlier, our sample of 76 IPOs that were matched with a control firm included nine firms which we were forced to exclude because they were founded only one year prior to their IPO, and two firms which we had to exclude because they had not yet reported on their two years post-IPO results. For the remainder of the observations all required annual observations were available. If however we would have opted to extend our time-frame to include three year post-IPO results, we would have lacked data for seven firms that were either acquired or dissolved or had not yet reported data on the third year post-IPO. We also opted for a time-frame of four years around the IPO (rather than six years) because the possibility of introducing endogenous bias becomes larger in long-horizon event studies, hence decreasing validity of the results (Kothari and Warner, 2006).

Following our main regression we seek to further analyze our sample IPO dataset in order to check for several other factors mentioned in previous literature. For instance we check our results for robustness by subdividing our sample based on the year of offering and correcting for average sales growth in the two years pre-IPO. Subsequently, we extend our regression model above by investigating the differential difference between different sub-groups of IPO firms identified in our sample. Firstly, we check whether firms that underwent an IPO in the year 1999 and 2000 have significantly different post-IPO performance from the rest of the sample due to the highly volatile market conditions (in particular for tech firms) in the years 2001-2002. Secondly, we sub-divide our sample based on median age at the time of IPO. Age of firms has been shown to be of influence in post-IPO performance (e.g. Borisov et al, 2015). Lastly, we segmented our sample into two groups of firms that either went public

on the regular exchange (either as a large, medium, or small cap stock) or went public on NASDAQ OMX's growth market, First North.

We formalize the regression by adding the following interaction terms to Equation (4):

$$\rho SUB_f + \vartheta(POST_{i,t} * SUB_f) + \tau\left(SUB_f * IPO_i\right) + \theta\left(POST_{i,t} * IPO_i * SUB_f\right) \qquad (6)$$

This gives us the following equation:

$$Y_{i,t,f} = \alpha + \gamma POST_{i,t} + \delta IPO_i + \rho SUB_f + \beta(POST_{i,t} * IPO_i) + \vartheta(POST_{i,t} * SUB_f) + \tau\left(SUB_f * IPO_i\right) + \theta\left(POST_{i,t} * IPO_i * SUB_f\right) + \varepsilon_{i,t,f} \qquad (7)$$

Apart from the relevant terms given for Equation (4), we introduce a third dummy variable (taking the value of one or zero) to sub-divide our sample into different sub-sets based on the factors we discussed earlier: IPO year, age at the time of IPO, and market of listing. We dubbed this dummy variable SUB_f. The model also includes firm and year fixed effects. The firm fixed effect subsumes some of the constituent parts (IPO, SUB and SUB*IPO) of the interaction terms and these estimates are therefore not reported in regression tables. In the regression above variable of interest is the θ term, which indicates the differential change between the sub-groups and can be formalized as followed:

$$\theta = \left[\left(\bar{\gamma}_{B,2,I} - \bar{\gamma}_{B,1,I}\right) - \left(\bar{\gamma}_{A,2,I} - \bar{\gamma}_{A,1,I}\right)\right] - \left[\left(\bar{\gamma}_{B,2,II} - \bar{\gamma}_{B,1,II}\right) - \left(\bar{\gamma}_{A,2,II} - \bar{\gamma}_{A,1,II}\right)\right] \qquad (8)$$

where B and A respectively represent the treatment and control firms, 2 and 1 represent respectively the post- and pre-event window, and I and II represent respectively the two sub-divided categories (subsequently IPO year, age at the time of IPO, and market of listing) discussed earlier.

4. Results

In the following chapter we present a number of figures to illustrate the effect of the going public decision for a sample of Swedish companies. Simultaneously we illustrate how this effect compares to companies that opted to remain private, the earlier mentioned control firms. We formalize these graphical representations by means of a difference-in-difference regression, which we support with multiple robustness checks. Subsequently, we extend the regression model to isolate the possible effect of the IPO for various sub-groups of firms in our sample. In line with previous literature we opted to sub-divide firms based on whether or not they listed during the 'tech-boom' years of 1999 and 2000, as well as age at the time of IPO. Also, we check whether firms listing on First North exhibit any difference compared to firms listing on the main exchange.

4.1 Graphical Presentation

We illustrate the effect of going public on employment and sales by means of respectively Figure 6 and Figure 7. To obtain the data used to compose these figures we select the 65 firms we use for our empirical analyses. Taking Figure 6 as an example, we subsequently compute the median employment figure for two years prior to the IPO to two years post the IPO for each firm and control firm. As discussed in the previous chapter, we opted to match our IPO firms with control firms on the year prior to the IPO. As such we consider the -1 year as a base year in our graphical representation for both the IPO firms as the control firms. To obtain the year to year growth rate we simply take the ratio of the median employment in year *t* to the median employment in year -1. As can be seen the difference in employment growth over the full period is significant – whereas firms that opt to go public increase the number of employees with 86.51% from the year prior to IPO, and 42.69% from the IPO year to two years post-IPO. Control firms, surprisingly, experience a negative growth of 20.65% from the year prior to IPO, and 29.35% from the IPO year to two years post-IPO. With regards to sales, as can be seen in Figure 7, net sales display a similar trend as discussed above. Growth from the year prior to IPO to two years post-IPO for the firms that went public totals 76.95%, compared to a negative growth of 3.19% for the control firms. Noteworthy is that the trend of increasing sales of the IPO firms from the year prior to the IPO to the year post-IPO flattens out after the first year after the IPO, with a relatively small increase of 6.12%[5].

[5] Although not included in the graphical presentation, we found this slowing trend continues from year two to three, with a growth of 5.2%.

4.2 Regression Analysis

In order to formalize the results discussed above we complement these graphical representations with a regression analysis as shown in Table 3. We employ Equation 4, as discussed in the previous chapter, to test for the effect of the IPO on a number of dependent variables, namely: (1) the log of employees, (2) the log of net sales, and (3) the return on assets, as defined by EBITDA over total assets (4) leverage, as defined by total debt over total assets and (5) the log of total assets. Our regression includes both firm and year fixed effects, in order to account for particular firm characteristics as well as aggregate macro-economic factors. We clustered error terms on the firm level. With reference to the first dependent variable, we find a 70% increase in the number of employees of firms that went public compared to the increase of employees of their respective control firms. This clear indicator of growth is also reflected in the difference of increase of sales: firms that went public increased net sales with no less than 157% compared to their private peers. Both of these results are statistically significant at the 1% level. The total assets variable also exhibits a difference in growth in line with the other estimators, totaling 85.6% compared to the control group.

Whereas IPO firms clearly grow significantly faster than their control peers in terms of number of employees, sales, and total assets this seems to not necessarily lead to higher profitability: we do not find significant results for an effect on RoA. Intuitively, a possible explanation for this could be an increased growth due to acquisitions, as suggested by Celikyurt et al (2010), or a substantially higher organic growth. Both acquisitions and organic growth have a potentially large effect on size as measured through employees, sales, and total assets, however, profitability does not necessarily improve as a result of growth. Unfortunately, our dataset does not provide us with a proper means to test for either organic or external growth in order to correct for these potential influences. However, these findings are in line with findings published by Takahashi and Yamada (2015), who concluded that excess growth in sales and number of employees can be observed for a prolonged period of time post-IPO, whereas profitability and productivity quickly revert back to normal levels. In terms of leverage we observe a significant negative difference of -10% compared to the control group. One explanation is that this can be partly attributed to the fact that when companies list, they commonly issue new equity, which increases the denominator in the leverage-formula (total assets) and hence reduces the ratio. If the post-event window would have been longer we might have observed a less distinct effect. A country-specific factor that might also contribute is the rigorous labor legislation in Sweden. For instance, Simintz et al (2014) suggest that strong labor protection increases the operational leverage which has a crowding-out effect on the financial leverage. The effect is also greater among

firms that are subject to frequent hiring and firing decisions. However, it must be noted that the magnitude of the measured effects on the number of employees, net sales and total assets raises concerns on the validity of the data. As mentioned in the previous chapter our matching methodology is designed to mitigate such concerns, however, it is important to recall the possible endogeneity bias inherent to this type of study.

4.3 Robustness Analysis

In order to test whether the observed results discussed in the previous paragraph are robust and representable across the entire sample, we subdivide our sample into two sub-samples depending on the year of offering. To subdivide the sample we take the median of the IPO year, which turns out to be 2006, and use this year to separate our sample into two groups by grouping all observations prior to end of 2006 and grouping all observations post 2006. Subsequently, we perform the same regression as discussed above on the two sub-samples. The results are presented in Table 4 and Table 5. As can be seen we observe similar effects of the IPO for both sub-sets of observations. The number of employees for firms that IPO prior to 2006 increase by 86% compared to their respective control firms in the years following the IPO, whereas employment growth for firms that IPO post 2006 is 55%. Net sales are also significantly different from growth of net sales of the control firms, with respectively 109% and 204%. Total assets grew by respectively 72.46% and 99% for the companies that list before and after 2006. All these results are significant at the 1% level, albeit not statistically different across the sub-periods.

The effect of the IPO on return on assets on the other hand remains insignificant when comparing control groups. Interestingly, the overall decline in leverage among IPO companies seems to be driven by companies that listed before 2006, as we found no significant result for the firms listed post 2006. There may be several factors that can partly explain differences between the results in the two different sub-sets. One possible explanation is that the group of firms that went public prior to 2006 contains firms that went public around 1999/2000 – as such the performance of these firms may be materially affected by the tech-bubble. Note our sample includes 20 firms that went public in either 1999 or 2000; we compare firms listed in these years with firms listed in other years at a later point in this chapter.

We performed another robustness check by means of including a control variable for average sales growth up to a maximum of two years prior to IPO. The model used is similar to Equation (4) and can be formalized as followed:

$$Y_{i,t} = \alpha_i + \delta_t + \gamma POST_{i,t} + +\beta(POST_{i,t} * IPO_i) + \omega\big(POST_{i,t} * Growth_i\big) + \varepsilon_{i,t} \qquad (8)$$

where Growth, as mentioned, is a dummy that represents average sales growth over a maximum period of two years prior to IPO. The additional interaction term ($POST_{i,t} * Growth_i$) is intended to capture potential deviations in growth rate for IPO and control group prior to the IPO. The results (see Table 6) are only marginally different from the first regression we performed (Table 3), which is a validation for our matching methodology and confirms the IPO event indeed has a substantial effect on corporate performance in the period post-IPO.

4.4 Sub-Groups Regression Analyses

In line with existing literature we subsequently aim to assess whether the average age of a firm at the time of going public has a significant effect on employment growth in post-IPO years. Kenny et al (2012) sub-divide firms IPOs into 'Emerging Growth Companies' (EGCs) and 'other' firms, the EGC classification is based on several factors, including age of the firm at data of the IPO. They find EGCs contribute significantly more to overall employment than 'other' firms, with the former exhibiting aggregate growth of 156% to 29%. For sales the difference is even more pronounced at 259% compared to 78% of the 'other' firms. In order to test whether there is any significant effect of age of the firm at the time of IPO we segment our sample into two groups and employ a triple difference regression, based on Equation (7). We test on the same set of dependent variables as before, namely: (1) the log of employees, (2) the log of net sales, and (3) the return on assets, as defined by EBITDA over total assets (4) leverage, as defined by total debt over total assets and (5) the log of total assets. This allows for a novel estimation of whether the effect of the IPO-event differs for companies in different stages of their life-cycle. By taking the median of the age of firms at the time of IPO we are able to segment our sample into two same-sized sub-samples. Subsequently, we perform a regression to check for differences among these two sub-samples. The results are presented in Table 7. As can be seen segmenting our sample on age at the time of IPO does not appear to yield any significant differences between the two groups; the interaction term (in the regression POST * IPO * SUB) yields insignificant results, whereas a positive interaction would have indicated firms with a higher than median age outperformed their lower-aged peers on the relevant dependent variable.

This finding contrasts to some degree with findings of the earlier discussed paper of Kenney et al (2012). Borisov et al (2015) also report a significantly higher employment growth for companies younger than the median age than for companies older than the median age. However, our study differs from these studies in several material ways. Firstly, both Kenney et al (2012) and Borisov et al (2015) investigated the US market and are able to use a larger sample of IPO firms and covered a longer time-frame. Secondly, Kennedy et al (2012) did not use a control group as comparison but rather looked at the overall growth while Borisov et al (2015) did use a control group for part of his analysis, however, not by employing a similar triple difference regression to compare different sub-sets of firms.

As mentioned earlier, a large number of firms in our sample underwent an IPO in either 1999 or the year 2000. It could be argued that likely many of the firms that went public around 1999-2000 had a lower median age than the average firm in our sample, given the 'hot' IPO market for in particular tech companies at that point in time. Using another triple difference regression we investigate whether there are any particular characteristics of firms that list during the years 1999-2000 that distinguishes them from the rest of the IPO firms. As can be seen in Table 8 no material differences are observed between the companies that list in 1999/2000 and the rest of the firms. This is in line with the Helwege and Liang (2004) who, as discussed earlier, also found no specific differences among companies that listed during "hot" and "cold" markets. The estimate for leverage does however indicate a negative difference between the two sub-samples. This difference could be explained by the significant number of small high-technology firms that listed during the period. Usually this type of firms is subject to information asymmetries and lack collateral resulting in difficulties to obtain external debt financing. However, investigating another time-frame, Carpenter and Petersen (2002) show that the equity raised in public offerings by technology firms between 1981 and 1998 allowed for significant growth in size. One could also argue that the leverage ratio at the time of listing is already low for the average 1999-2000 firm, and as such proportionally declines even more when new equity is issued.

A distinct feature of the Swedish market is its fairly new 'growth market', First North. First North was founded in 2006 to offer smaller companies the advantage of being traded publically on a MTF. Companies on First North have the benefit of a more lenient regulatory environment and incur lower listing costs than their peers on the main list. One may argue that firms listing on First North hence might be able to capture more gains from going public than firms on the main list. On the other hand, shareholder demand may be higher on the main list, which could result into higher offering prices. Also, some studies show that long-term performance of firms listed on secondary markets is worse than that of firms listed on the main market (Vismara and Paleari, 2012). A widely debated topic is whether

size of firms has an effect on employment growth. Ayyagari et al (2011) concluded that employment growth exhibited by small firms is larger than the growth displayed by medium and large firms. Firms listed at First North differ in size and could in some instances be larger than firms that list on the main markets (small cap), but we consider it to be a fair approximation that (on average) firms on First North are smaller than most firms listed on the main list. We measure the potential difference in effect of firms listing on First North vis-à-vis firms listing on the main list by means of the same triple difference regression used in the previous analyses of this section. As can be seen in Table 9 we do not observe a significant difference between the two sub-samples in the dependent variables covered in the analysis. In the following chapter we elaborate on the results presented in this chapter and discuss possible implications for Sweden's public market and economy.

5. Discussion

In the following section we discuss the results presented in the previous chapter and elaborate on the general implications of our research. Also, we compare our results with other research and discuss the limitations we encountered throughout our study. We also discuss the wider economic insights that we can derive from our results.

The key finding of our research is that firms that undergo an IPO experience a significant increase in net sales, total assets and number of employees in the year of the IPO and the post-IPO years, when compared to similar firms that did not opt to go public. This result is in line with a range of other studies conducted over the years (e.g. Kenny et al, 2012, Borisov et al, 2015, and Takahashi and Yamada, 2015). This 'IPO effect' can be attributed to a number of reasons. For instance, Rajan (1992) argues a more flexible capital structure leads to a greater amount of external funding and/or lower costs of credit, and as such provide businesses with the funds to expand rapidly. Other studies argue IPOs motivate and incentivize managers to pursue acquisitions (e.g. Brau and Fawcett, 2006 and Celikyurt et al, 2010), which in turn leads to growth, albeit perhaps not organic. Other reasons for an increase in employees may be related to regulatory and listing requirements – companies that opt to go public are required to comply with directives set by the exchange as well as those set by the governmental agency presiding over the financial markets. In order to comply with these requirements one may expect to see an increase in the number of employees around the time of listing.

Apart from testing for effects of an IPO on employment growth, total assets and net sales we also tested for effects on profitability and leverage. We did not find any conclusive evidence to suggest that IPOs have a material effect on operating performance as measured by return on assets. The underlying reason as to why we did not find any effects may derive from multiple factors. For example, profitability may be impacted by firm specific factors such as the size and age of the firm (e.g. Bharat and Omesh, 1994 and Mikkelson et al, 1997). Profitability may also be impacted by one-time costs related to the IPO, this could for example be costs related to advisory and legal advice or costs related to underwriting. For small firms in particular these costs may be quite substantial. Ritter (1987) estimated the average transaction costs of firms using best effort offers (commonly used among small and speculative firms) to be 31.78% of the market value of the issued securities. In our regression we found a significant decrease in leverage compared to the control firms. Intuitively this is to be expected, as firms usually issue equity at the time of listing which reduces the debt/total assets ratio. One could also hypothesize that IPO firms, by increasing the number of employees essentially increase operational leverage (since future salary payments can be seen as a (senior) claim on assets), which in turn needs

to be compensated by a reduction in financial leverage (Simintzi et al, 2014). This may be particularly pronounced in a country with high labor-protection such as Sweden. We check our results for robustness by performing the same regression on two different sub samples of firms depending on the year in which they list. In doing so we observe that the measured leverage effect seems to be attributable to firms that listed prior to 2006 – for firms that went public post 2006 no significant reduction in leverage can be observed.

Regarding our conclusions vis-à-vis existing literature on the topic - our paper differs in several material ways from various other studies on this topic, which needs to be considered when evaluating the conclusions derived from our study. Firstly, an issue our paper shares with the other studies mentioned above is that one cannot factually considered IPOs to be exogenous events. Irrespective of study design, conclusions derived from observational data such as listing events are subject to a certain endogeneity bias. Where our study differs from other studies (e.g. Kenny et al, 2012), is that the access to data from both private and public companies allows us to mitigate the extent of the endogeneity bias by means of a tested methodology employed by a number of comparable papers (e.g. Boucly et al, 2011). By matching our IPO firms with comparable private companies ('control' firms), based on a number of parameters in the year prior to IPO, we are able to capture the IPO effect by comparing the performance of these two samples in the IPO year and the years following the IPO. As such it is not entirely appropriate to draw a direct parallel between conclusions derived from other papers and our own research, though the general trends observed in most papers are in line with our own results.

A consequence of our matching methodology is that we are required to exclude a substantial number of observations on the account that these firms do not have a matched control firm. Also, we opted to exclude firms for which we were not able to access records for the entire time-frame of our study (two years prior to two years post IPO). Reasons for this lack of records could be bankruptcy, corporate restructuring, or delisting or acquisitions. We exclude these observations because we cannot reasonably estimate how the business evolved over the course of time in the absence of records. Naturally, this approach inherently causes a different bias by omitting several observations. Also, a key dissimilarity between our study and studies on the effect of IPOs on for instance the US market, is that we unfortunately have access to far fewer observations, which considerably affects the predictive value of our results. Keeping in mind the earlier mentioned inherent endogeneity bias, as well as the limited size of our data sample, it is advisable to interpret our results as descriptive rather than assess them on their predictive value.

Regarding our further results; whereas most studies report an increase in growth post-IPO, some studies (e.g. Carpenter and Rondi, 2006) conclude growth post-IPO is highly dependent on the type of firm that goes public. In particular they established a correlation between age of the firm and the presence of controlling stakeholders. Borisov et al (2015) also found that firms with a private age below the sample median age experience a greater increase in employment levels than firms with above-median private age. We do not find any substantial relationship between age of the firm at the time of going public and post-IPO performance when firm growth is adjusted for by a relevant control group. Another factor of interest when comparing our data with that of notable US papers is the median size of companies going public. The median firm going public in the US is substantially larger than the median firm in Sweden. An explanation for this may be that the US lacks a market with listing requirements as flexible as the growth market (First North) of Sweden. Relaxing of listing requirements offers smaller firms access to public markets, but simultaneously performance of firms listed on secondary markets has been poor compared to the main market (Vismara and Paleari, 2012). We also performed triple difference regressions on sub-samples of our dataset based on firms that listed during the technology boom in 1999/2000 as well as a regression depending on the listing venue. We do not find any significant difference between the two sub-sets of firms apart from a significant decrease in leverage by firms that listed during 1999/2000. This effect could be explained by the many small high-technological firms that listed during this period, as these types of firms usually are not very dependent on external financing due to asymmetric information and lack of collateral. It must be noted that our sub-sets are of such size that the effect of the performance of individual companies, or relatively homogenous groups of companies, may have a substantial effect on the overall regression results.

However, although we are limited by the size of our dataset, which is often inherent to papers on 'smaller' equity markets, we believe we supplement existing literature by offering a comprehensive overview of the effect of IPOs in the Swedish market by means of a matching methodology which is novel in this particular area of research. The novelty of our approach is partly due to the fact that a large database with data on private entities, which is a prerequisite in order to make use /of a matching methodology, is often not available for most markets. Ideally, one would be in possession of a large database with data on private entities in a 'larger' market, or multiple markets combined. With access to such data one would be able to replicate our tests with a higher certainty of causality, also aided by the fact that one could test for multiple effects we were unable to test for due to a limit of available observations. For instance, it would be interesting to compare firms of similar size that listed in different markets (e.g. the Swedish main list and First North), in order to capture the effect of listing on various

exchanges. Also, a more accurate comparison between different industries would aid to the understanding of the effect of IPOs on a firm's operational performance, as would a clear distinction between organic growth and growth due to acquisitions. Furthermore, another interesting further research possibility would be to complement our research with a study on the development of wages and compensation in the period around an IPO. Also, one may want to further assess the relationship between operational leverage and financial leverage in the years following an IPO. Regarding the matching methodology applied in this paper, size of the sample permitting, it may be interesting to further refine this methodology by testing the accuracy of results after adding additional parameters such as for instance age of the firm and regional focus of the firm's operations.

With regards to the overall economic insights that can be derived from our study, we can state that it appears that on average, firms that opt to go public in Sweden perform significantly better in the years post-IPO compared to their peers that remain private. The economy of Sweden as a whole benefits substantially from this outperformance, as public firms help to reduce unemployment and act as a catalyst for investments and innovation. Furthermore, it should be taken into account that start-ups in Sweden likely account for a significant percentage of annual employment growth (in the US start-ups contribute to 3% of employment any given year (Haltiwanger et al, 2013). As such Sweden regulators should continue to aid young and growing firms that seek to go public by assisting them in their efforts. Sweden's growth exchange, First North, contributes in this respect. To uphold and expand on this practice it is vital for regulatory bodies to continue to critically assess the rules governing capital markets and to continuously promote an entrepreneurial and risk-rewarding business climate.

6. Conclusion

In this study we find that on average companies that go public between the years 1999 to 2012 grow significantly faster in the years following the IPO than similar firms that opted to remain privately owned. We define growth as an excess increase of the number of employees, net sales, and total assets. We also find that this excess growth does not directly translate into higher profit margins. Going public appears to have a negative effect on financial leverage, though this result is not consistent across the full sample. The effect on leverage may be a direct consequence of an increase in equity due to the IPO, or the result of a possible trade-off between an increase of operational leverage (for instance a larger number of employees) at the expense of financial leverage.

Our findings are largely in line with similar results published in papers on the US market. However, we differentiate ourselves by our research design, which is primarily based on a matching methodology commonly used in other papers on event-driven corporate performance. Our methodology allows us to correct for industry specific trends and developments in the overall economy, yielding a more accurate representation of the true effect of an IPO. On the other hand, the data sample we investigate is significantly smaller than that of other studies looking into the effect of IPOs. This, in combination with the inherent endogenous bias of using observational data, yields our results to be more suitable to be interpreted as descriptive rather than causal.

We supplement our main findings by a series of robustness checks. We also add on to our regression model by means of an introducing an additional term, allowing us to segment our sample into various groups depending on respectively the age of the firm at the time of the IPO, whether or not the firm went public in 1999-2000, and the type of exchange the firm listed on. Due to constraints on the accuracy of data on employees' salary and compensation we were unable to take this factor into account as an additional term in our regression. In general we do not observe many significant differences in our dependent variables as a result of our sub-division. We do note that leverage of firms that went public in 1999-2000 was significantly lower in the years post-IPO compared to the leverage level of firms that went public in other years.

By means of our study we hope to give relevant institutions insight into the functioning of corporations of various sized and characteristics in the Swedish market, as well as make regulators and other relevant entities more aware of the importance of IPOs and a well-functioning public market for the overall economic development of Sweden.

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Figures and Tables

Figure 1: Listing Events on Swedish Public Markets

Number of listing events on public exchanges in Sweden from 1999 to 2012, segregated in three categories: new issues (IPOs), Secondary listing or relisting, and Spin-offs.

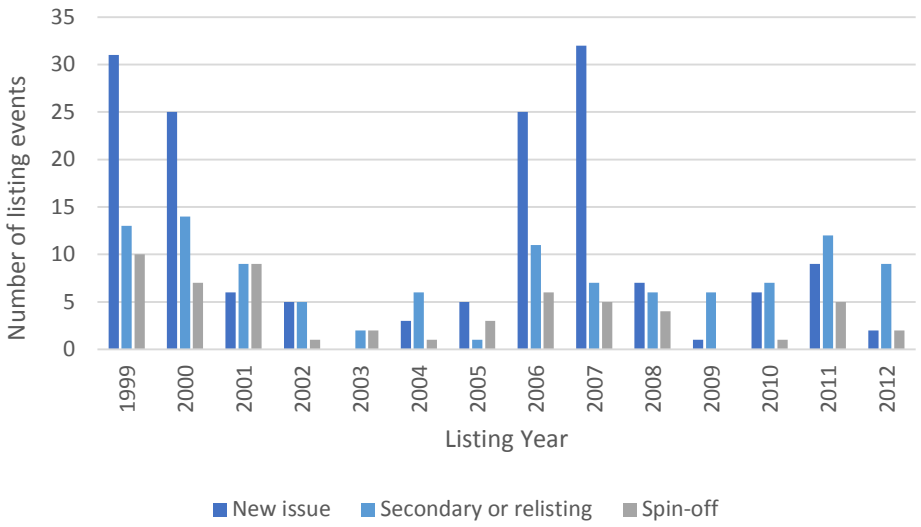

Figure 2: Construction of Treatment Sample

Waterfall chart illustrating the construction of our treatment sample. Our initial sample comprises all IPOs in Sweden between 1999 and 2012, a total of 157. Subsequently we exclude 38 firms because either (1) primary business activities do not take place in Sweden, and hence the full effect of an IPO on the economy of Sweden could not be observed, or (2) the firms were delisted (due to acquisitions or bankruptcy) before reporting data for the year post-IPO. Secondly, we exclude all firms for which public records are unavailable. Thirdly, we exclude all firms which we seem too small, as measured by the number of employees at the year pre-IPO. Fourthly, we exclude all companies for which we did not find a suitable 'match', or control firm, as described in our control sample construction section. Lastly, we exclude observations for which we do not have records for the time period of two years prior to two years post-IPO. The final sample hence comprises of 65 IPO firms.

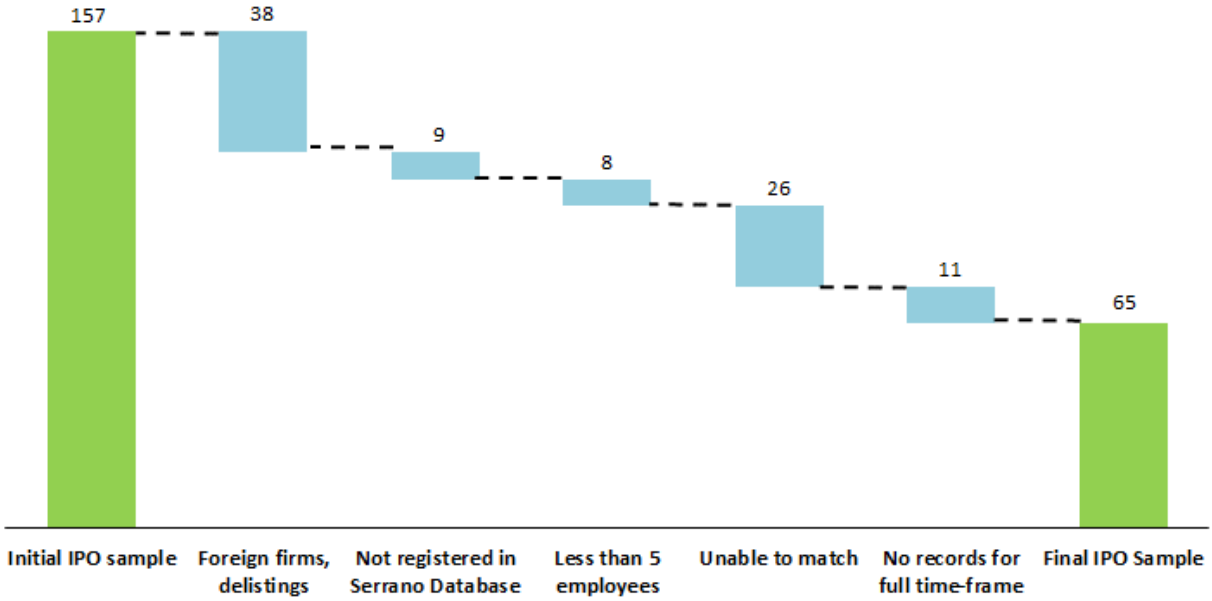

Figure 3: Listing Events on US Public Markets

*N*umber of listing events on public exchanges in the US from 1999 to 2012.

Figure 4: Segregation of IPO firms by Industry

Segregation of IPO sample firms per industry based on the first two digits of the Swedish Industrial Classification (SNI) code (see text for details). The six largest categories in total account for 50 out of 65 IPOs. The 'Other' category comprises all other industry categories and totals 15 observations.

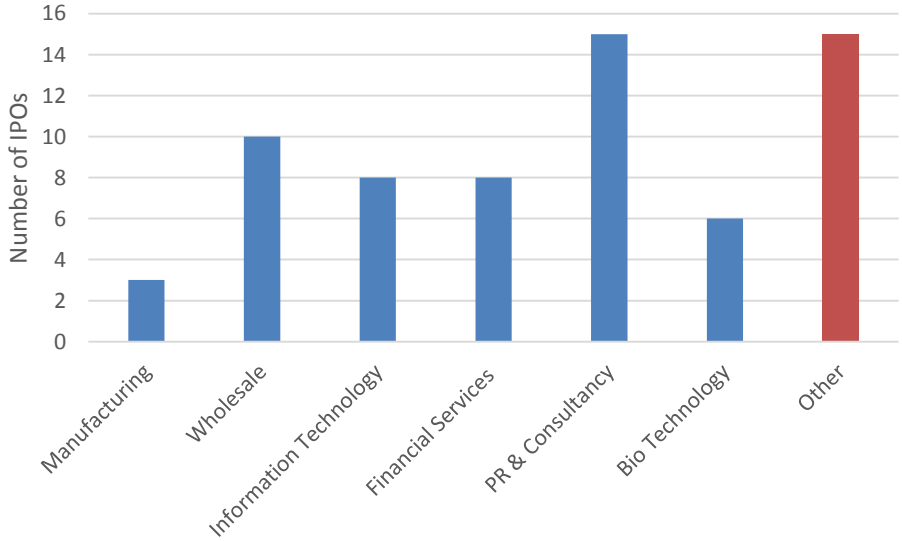

Figure 5: Graphical Representation of the Treatment Effect

Illustration of the treatment effect being isolated by means of the regression model and matching methodology. Note the treatment effect is the difference between the growth in the dependent variable (employment in this example) occurring post event (IPO in this example), and the counterfactual growth in the dependent variable in absence of the event. The counterfactual trend is derived from the trend in the dependent variable of the control firm over the investigated time-frame.

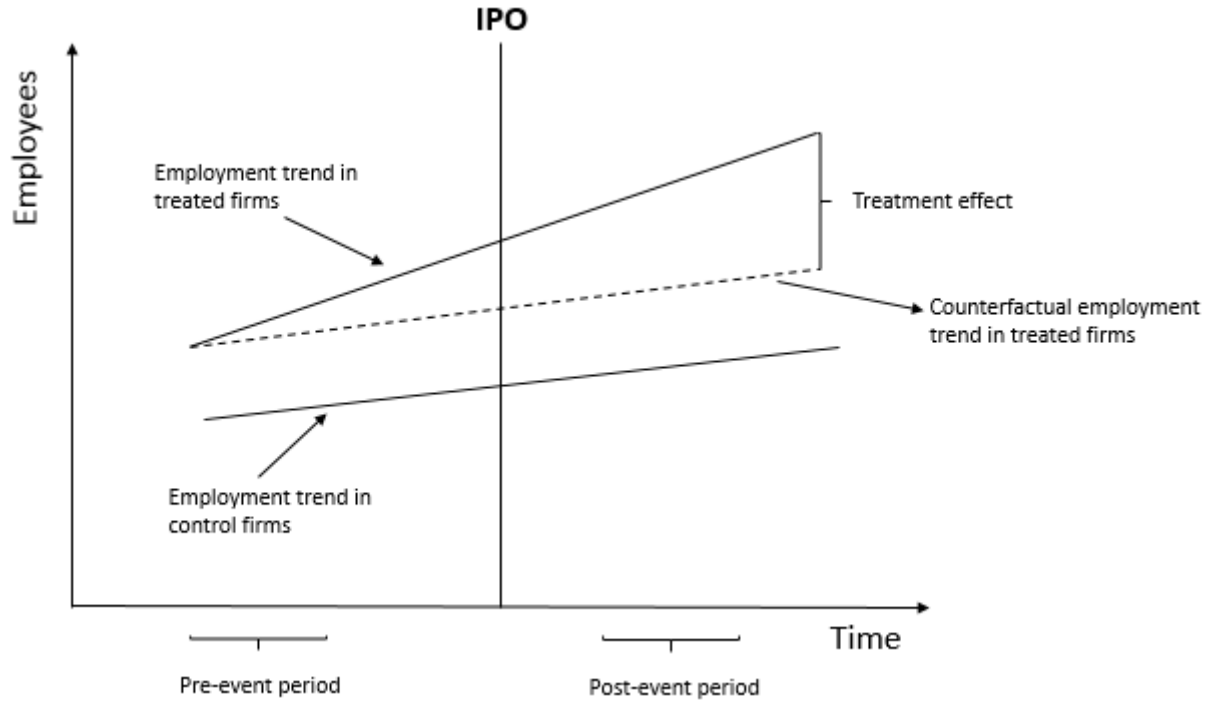

Figure 6: Graphical Representation of Employment Growth Post-IPO

Graphical representation of cumulative growth of the number of employees for the IPO sample and the control firms. The presented data is computed as followed: for each year from two year prior to the IPO to two years post-IPO we compute the median employment figure for respectively IPO firms and control firms. Subsequently we calculate the change compared to a base year, with the base year being the year prior to IPO.

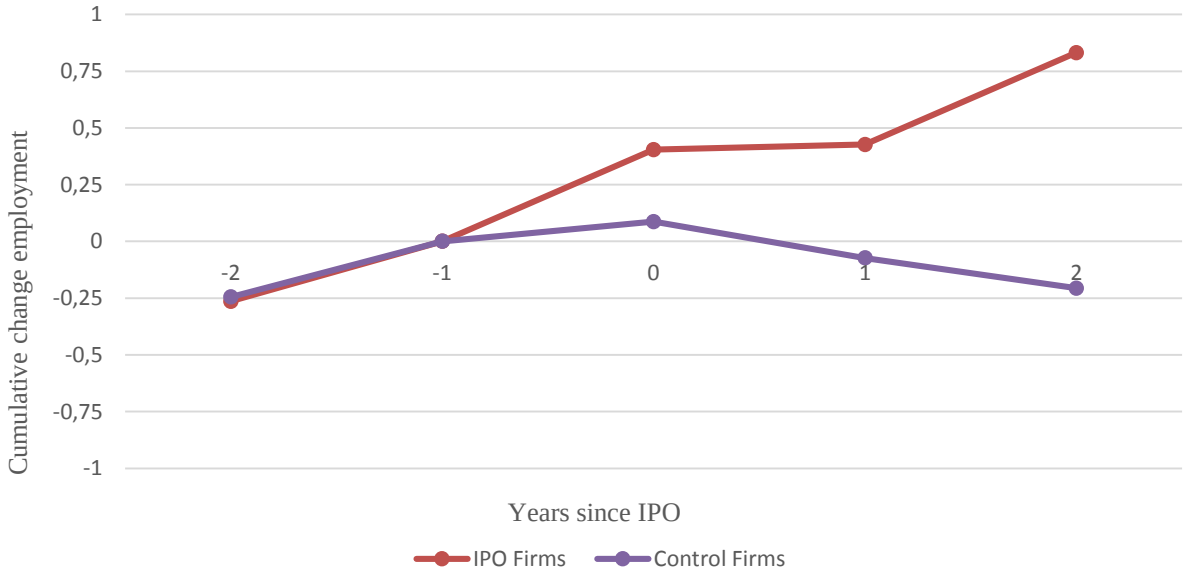

Figure 7: Graphical Representation of Sales Growth Post-IPO

Graphical representation of cumulative growth of of net sales for the IPO sample and the control firms. The presented data is computed as followed: for each year from two year prior to the IPO to two years post-IPO we compute the median net sales figure for respectively IPO firms and control firms. Subsequently we calculate the change compared to a base year, with the base year being the year prior to IPO.

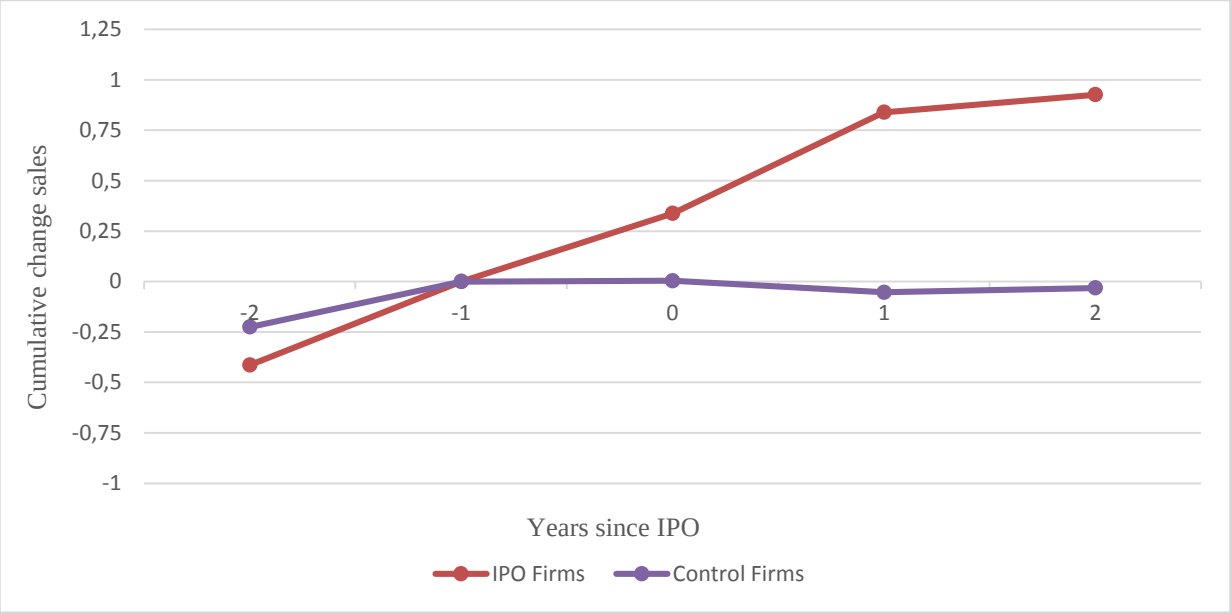

Table 1: Treatment Variables vs. Control Variables

Table comparing the matching continuous variables 'Number of employees' and 'ROA'. The matching methodology ensures the mean values of our treatment group and control group for both variables are not statistically different from one another. This is verified by means of a paired t-test in the year prior to the IPO, allowing us to not reject the null hypothesis of there being no statistical difference between the treatment and control group.

	Mean	Std. Err.	Std. Dev.	P-value
Employees - treatment	453,14	172,09	1387,47	
Employees - control	455,16	181,38	1462,32	
Difference	2,03	16,19	130,52	**0,90**
ROA - treatment	0,018	0,061	0,489	
ROA - control	0,023	0,058	0,467	
Difference	0,005	0,007	0,055	**0,442**

Table 2: Distribution of Variables of IPO firms vs. Control Firms

Summary of data of treatment observations and control observations of the year pre-IPO. For each of the variables listed we provide the Median, Mean, Standard Deviation, and 25% and 75% values. RoA equals EBITDA over Total Assets and Leverage equals Debt over Capital. The rest of the variables are self-explanatory.

IPO Firms	**Median**	**Mean**	**Std. Dev.**	**Q.1**	**Q.3**	**Number of firms**
Employees	92	453	1.387	28	253	65
Net Sales	213.413	905.651	2.317.359	65.534	597.744	65
Total Assets	160.603	838.816	2.380.259	46.832	571.056	65
EBITDA	21.382	70.378	174.700	3.049	74.430	65
RoA	0,112	0,023	0,467	0,061	0,209	65
Leverage	0,632	0,629	0,230	0,495	0,793	65
Control Firms						
Employees	89	455	1.462	28	211	65
Net Sales	146.536	816.756	2.345.651	42.174	446.449	65
Total Assets	85.522	1.631.898	5.648.282	30.552	329.391	65
EBITDA	12.523	196.346	918.872	2.239	35.375	65
RoA	0,111	0,018	0,489	0,061	0,210	65
Leverage	0,711	0,721	0,200	0,617	0,813	65

Table 3: Regression Table – Effect of IPO on Treatment Group

IPO Sample and control firms over the 1999-2012 period. Estimates for the effect of an IPO on the treatment group (POST x IPO) and non-treatment group (POST). POST is a dummy which equals one for an observation which occurs after the public offering event and zero if the observation occurs before the public offering. IPO is another dummy, taking the value of one for firms that undergo an IPO, and zero for control firms. Log (Employees) is the logarithm of employment, log (Net Sales) is the logarithm of net sales, RoA is Return on Assets, as measured by EBITDA over Total Assets. Leverage is measured as Debt over Total Assets, log (Total Assets) is the logarithm of Total Assets. All regressions include Firm and Year Fixed Effects. The firm fixed effect subsumes the effect of some of the constituent parts of the interactions terms (the dummy variable IPO). The remaining estimates are reported in the table below.

	(1)	(2)	(3)	(4)	(5)
	log (Employees)	log (Net sales)	RoA	Leverage	log (Total Assets)
POST * IPO	.708***	1.578***	-.0630	-.0998***	.856***
	(.1559)	(.437)	(.0714)	(.0367)	(.1448)
POST	-.363***	-1.024***	.120	-.074**	-.2872**
	(.1293)	(.3819)	(.0914)	(.0296)	(.111)
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
Observations	650	650	650	650	650
Number of listing events	65	65	65	65	65
Adjusted R^2	0.8939	0.6193	0.2953	0.5919	0.9049

*** indicates statistical significance at the 1% level

** indicates statistical significance at the 5% level

Table 4: Regression Table – Robustness Check on Year of IPO (Upper Half of Observations)

IPO sub-sample and control firms over the 1999-2012 period, sub-sample represents all firms for which the IPO year is higher than the Median IPO year. Estimates for the effect of an IPO on the treatment group (POST x IPO) and non-treatment group (POST). POST is a dummy which equals one for an observation which occurs after the public offering event and zero if the observation occurs before the public offering. IPO is another dummy, taking the value of one for firms that undergo an IPO, and zero for control firms. Log (Employees) is the logarithm of employment, log (Net Sales) is the logarithm of net sales, RoA is Return on Assets, as measured by EBITDA over Total Assets. Leverage is measured as Debt over Total Assets, log (Total Assets) is the logarithm of Total Assets. All regressions include Firm and Year Fixed Effects. The firm fixed effect subsumes the effect of some of the constituent parts of the interactions terms (the dummy variable IPO). The remaining estimates are reported in the table below

	(1) log (Employees)	(2) log (Net sales)	(3) RoA	(4) Leverage	(5) log (Total Assets)
POST * IPO	.553**	2.044 **	-.020	-.0599	.727***
	.2623	.7660	.1249	(.0537)	(.2182)
POST	-.292	-1.427**	.202	-.107**	-.159
	.1907	.6901	.1821	(.0489)	(.1367)
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
Observations	330	330	330	320	320
Number of listing events	33	33	33	32	32
Adjusted R^2	0.8725	0.5953	0.2769	0.5165	0.6460

*** indicates statistical significance at the 1% level

** indicates statistical significance at the 5% level

Table 5: Regression Table – Robustness Check on Year of IPO (Lower Half of Observations)

IPO sub-sample and control firms over the 1999-2012 period, sub-sample represents all firms for which the IPO year is lower than the Median IPO year. Estimates for the effect of an IPO on the treatment group (POST x IPO) and non-treatment group (POST). POST is a dummy which equals one for an observation which occurs after the public offering event and zero if the observation occurs before the public offering. IPO is another dummy, taking the value of one for firms that undergo an IPO, and zero for control firms. Log (Employees) is the logarithm of employment, log (Net Sales) is the logarithm of net sales, RoA is Return on Assets, as measured by EBITDA over Total Assets. Leverage is measured as Debt over Total Assets, log (Total Assets) is the logarithm of Total Assets. All regressions include Firm and Year Fixed Effects. The firm fixed effect subsumes the effect of some of the constituent parts of the interactions terms (the dummy variable IPO). The remaining estimates are reported in the table below

	(1) log (Employees)	(2) log (Net sales)	(3) RoA	(4) Leverage	(5) log (Total Assets)
POST * IPO	.869***	1.098***	-.107	-.141***	.990***
	(.1633)	(.4008)	(.0675)	(.0502)	(.1922)
POST	-.407***	-.472	.075*	-.0333	-.365**
	(.1489)	(.0448)	(.0448)	(.0355)	(.1538)
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
Observations	320	320	320	320	320
Number of listing events	32	32	32	32	32
Adjusted R^2	0.8993	0.6361	0.3619	0.6753	0.8950

*** indicates statistical significance at the 1% level

** indicates statistical significance at the 5% level

* indicates statistical significance at the 10% level

Table 6: Regression Table – Robustness Check Correcting for Sales Growth Pre-IPO

IPO Sample and control firms over the 1999-2012 period. Estimates for the effect of an IPO on the treatment group (POST x IPO) and non-treatment group (POST). POST is a dummy which equals one for an observation which occurs after the public offering event and zero if the observation occurs before the public offering. IPO is another dummy, taking the value of one for firms that undergo an IPO, and zero for control firms. The GROWTH dummy allows us to correct for sales growth in the two years pre-IPO. The GROWTH variable is a constant dummy calculated by taking the average sales growth in the period two years prior to IPO. Log (Employees) is the logarithm of employment, log (Net Sales) is the logarithm of net sales, RoA is Return on Assets, as measured by EBITDA over Total Assets. Leverage is measured as Debt over Total Assets, log (Total Assets) is the logarithm of Total Assets. All regressions include Firm and Year Fixed Effects. The firm fixed effect subsumes the effect of some of the constituent parts of the interactions terms (the dummy variable IPO). The remaining estimates are reported in the table below

	(1) log (Employees)	(2) log (Net sales)	(3) RoA	(4) Leverage	(5) log (Total Assets)
POST * IPO	.708***	1.518 ***	-.0634	-.1034***	.856***
	.1573	(.4371)	(.0719)	(.0371)	(.1465)
POST *Growth	.000	.014	.000	.001***	-.000
	(.0008)	(.0027)	(.0003)	(.0002)	(.0010)
POST	-.363***	-1.016***	.120	-.074**	-.287**
	(.1296)	(.3815)	.(0915)	(.0296)	(.1113)
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
Observations	650	650	650	650	650
Number of listing events	65	65	65	65	65
Adjusted R^2	0.8937	0.6207	0.2939	0.5926	0.9047

*** indicates statistical significance at the 1% level

** indicates statistical significance at the 5% level

Table 7: Regression Table – Triple Difference Regression on Firm Age at IPO

IPO Sample and control firms over the 1999-2012 period. Estimates for the effect of an IPO on the treatment group (POST x IPO) and non-treatment group (POST), and triple difference regression group (POST x IPO x SUB). POST is a dummy which equals one for an observation which occurs after the public offering event and zero if the observation occurs before the public offering. IPO is another dummy, taking the value of one for firms that undergo an IPO, and zero for control firms. SUB is another dummy, taking the value of one for firms that have a higher-than-median age at the time of IPO, and zero for firms which have a lower-than-median age. Log (Employees) is the logarithm of employment, log (Net Sales) is the logarithm of net sales, RoA is Return on Assets, as measured by EBITDA over Total Assets. Leverage is measured as Debt over Total Assets, log (Total Assets) is the logarithm of Total Assets. All regressions include Firm and Year Fixed Effects. The firm fixed effect absorbs the effect of some of the constituent parts of the interactions terms (IPO, SUB and IPO*SUB). The remaining estimates are reported in the table below

	(1) log (Employees)	(2) log (Net sales)	(3) RoA	(4) Leverage	(5) log (Total Assets)
POST * IPO * SUB	-.003	-.115	-.037	.034	.004
	(.3150)	(.8764)	(.1420)	(.07311)	(.2879)
POST * IPO	.710**	1.637**	-.044	-.117**	.854***
	(.2791)	(.6713)	(.1257)	(.0544)	(.1898)
POST*SUB	-.027	-.217	-.1	.027	-.1655
	(.2786)	(.5142)	(.1189)	(.0448)	(.1247)
POST	-.350	-.922**	.167	-.086**	-.208*
	(.2305)	(.3736)	(.1443)	(.0414)	(.1183)
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
Observations	650	650	650	650	650
Number of listing events	65	65	65	65	65
Adjusted R^2	0.8935	0.6182	0.2979	0.5933	0.9050

*** indicates statistical significance at the 1% level

** indicates statistical significance at the 5% level

* indicates statistical significance at the 10% level

Table 8: Regression Table – Triple Difference Regression on IPO Year

IPO Sample and control firms over the 1999-2012 period. Estimates for the effect of an IPO on the treatment group (POST x IPO) and non-treatment group (POST), and triple difference regression group (POST x IPO x SUB). POST is a dummy which equals one for an observation which occurs after the public offering event and zero if the observation occurs before the public offering. IPO is another dummy, taking the value of one for firms that undergo an IPO, and zero for control firms. SUB is another dummy, taking the value of one for firms that went public in 1999 or 2000, and zero for firms that went public in years other than 1999 or 2000. Log (Employees) is the logarithm of employment, log (Net Sales) is the logarithm of net sales, RoA is Return on Assets, as measured by EBITDA over Total Assets. Leverage is measured as Debt over Total Assets, log (Total Assets) is the logarithm of Total Assets. All regressions include Firm and Year Fixed Effects. The firm fixed effect subsumes the effect of some of the constituent parts of the interactions terms (IPO, SUB and IPO*SUB). The remaining estimates are reported in the table below

	(1) log (Employees)	(2) log (Net sales)	(3) RoA	(4) Leverage	(5) log (Total Assets)
POST * IPO * SUB	.117	-.857	-.101	-.141*	.281
	(.2562)	(.6992)	(.1377)	(.0757)	(.2649)
POST * IPO	.672***	1.842***	-.031	-.056	.768***
	(.2169)	(.6112)	(.0925)	(.0445)	(.1919)
POST*SUB	.095	.951**	.014	.118**	.044
	(.2336)	(.469)	(.1183)	(.0598)	(.2026)
POST	-.376**	-1.263**	.112	-.106***	-.282**
	(.1759)	(.487)	(.1135)	(.0355)	(.1387)
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
Observations	650	650	650	650	650
Number of listing events	65	65	65	65	65
Adjusted R^2	0.8937	0.6191	0.2934	0.5968	0.9050

*** indicates statistical significance at the 1% level

** indicates statistical significance at the 5% level

* indicates statistical significance at the 10% level

Table 9: Regression Table – Triple Difference Regression on Exchange Venue

IPO Sample and control firms over the 1999-2012 period. Estimates for the effect of an IPO on the treatment group (POST x IPO) and non-treatment group (POST), and triple difference regression group (POST x IPO x SUB). POST is a dummy which equals one for an observation which occurs after the public offering event and zero if the observation occurs before the public offering. IPO is another dummy, taking the value of one for firms that undergo an IPO, and zero for control firms. SUB is another dummy, taking the value of one for firms that went public on First North, and zero for firms that went public on the regular OMX exchanges. Log (Employees) is the logarithm of employment, log (Net Sales) is the logarithm of net sales, RoA is Return on Assets, as measured by EBITDA over Total Assets. Leverage is measured as Debt over Total Assets, log (Total Assets) is the logarithm of Total Assets. All regressions include Firm and Year Fixed Effects. The firm fixed effect subsumes the effect of some of the constituent parts of the interactions terms (IPO, SUB and IPO*SUB). The remaining estimates are reported in the table below.

	(1) log (Employees)	(2) log (Net sales)	(3) RoA	(4) Leverage	(5) log (Total Assets)
POST * IPO * SUB	.323	.905	-.091	.065	.099
	(.2461)	(.7559)	(.1188)	(.0752)	(.2626)
POST * IPO	.480***	.937*	.001	-.145**	.785***
	(.1233)	(.4848)	(.0703)	(.0599)	(.1807)
POST*SUB	-.334	-.035	.172	-.054	-.212
	(.3174)	(.7939)	(.1175)	(.0462)	(.1586)
POST	-.133	-.984	.003	-.036	-.142
	(.1422)	(.7573)	(.0665)	(.0332)	(.1603)
Firm Fixed Effect	Yes	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes	Yes
Observations	650	650	650	650	650
Number of listing events	65	65	65	65	65
Adjusted R^2	0.8946	0.6195	0.2964	0.5918	0.9048

*** indicates statistical significance at the 1% level

** indicates statistical significance at the 5% level

* indicates statistical significance at the 10% level