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The January Anomaly and Size Effect:

Developed and Emerging Markets

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Abstract: This paper examines the January effect and its relation to size on international stocks, more specifically on developed and emerging markets. The main findings indicate the effect to be non-existent for the aggregate market proxy in both developed and emerging markets. After allocating the stocks to equally weighted decile portfolios, however, the effect becomes apparent for developed markets in the smallest decile. The effect remains non-existent, on the other hand, for emerging markets even after incorporating size.

Keywords: January effect, size effect, international stocks, developed markets, emerging markets.

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SECTION I: INTRODUCTION

The efficient market hypothesis states that stock prices reflect all available and relevant information and, as a result, stocks always trade at their fundamental value. It should, therefore, be impossible to beat the market, since it is impossible for investors to purchase stocks which are under- or overvalued. However, literature shows that the efficient market hypothesis does not hold as many financial market anomalies have been reported over the past decades. One of these anomalies is the January effect, which suggests stock returns are higher in January than in any other month of the year. The most prominent behavioural explanation of the effect is the tax-loss selling hypothesis. Investors tend to sell bad performing stocks at year-end to offset realized capital gains in order to reduce their tax liability. Consequently, prices drop substantially at year-end due to intensified selling pressure and rebound in January when investors start buying again, resulting in high January returns. Another pronounced explanation is the window dressing hypothesis, which argues that sophisticated investors tend to sell stocks that have depreciated over the past year at year-end in order to improve the appearance of their stock portfolio performance to clients. In January, the investors reinvest the proceeds from the sales which drive up prices. A weaker explanation of the January effect is the information hypothesis. For most firms January is the start of the fiscal year, hence accounting information about the previous year is usually made public in January. Uncertainty about this information therefore increases around the turn of the year. Higher uncertainty results in higher risk which, in turn, results in higher expected returns.

Regardless of what the exact reasoning behind the January effect is, literature shows the effect has been robust and consistent over the past decades. However, the focus is predominantly on U.S. stock markets and is mostly conducted in the seventies, eighties, and nineties. This paper adds to existing literature in a twofold manner. Firstly, it examines the effect internationally, more specifically on developed and emerging markets, thereby examining the Morgan Stanley Capital International (MSCI) index. The MSCI index is a major international stock index provider, focusing on a large array of developed and developing countries. The index covers approximately 85 percent of the global investable equity opportunity set. Secondly, the paper uses a recent time period, namely 1990-2016. The goal is to observe whether the effect is present in both market segments using an aggregate market proxy, and by forming equally weighted size decile portfolios to examine the effect's relation with size.

The next section discusses the January effect and its behavioural explanations in more detail. Section III describes the applied methodology, time-series regressions, and hypotheses. Section IV provides an overview of the data used in this research. The results and their interpretations are accordingly discussed in section V. Sections VI and VII, at last, conclude and discuss limitations and potential further research.

SECTION II: LITERATURE REVIEW

The January effect, i.e. the tendency for stocks to strongly appreciate in value during the month of January compared to the other months in the year, was first observed by investment banker Sidney B. Wachtel in 1942. By examining the Dow-Jones Industrial Average from 1927 to 1942, Wachtel (1942) shows that the aggregate performance of the thirty stocks in the index (at the time) displays bullish behaviour from December to January. In more detail, the index displayed bullish movements in eleven of the fifteen examined years, whereby the four bearish movements were all relatively insignificant. Traditional finance and asset pricing theories argue that markets are efficient and, therefore, that stock prices follow a random walk (i.e., stock prices are unpredictable). The January effect contradicts these theories since research shows that stock returns in January are consistently higher than in any other month of the year. To explain the effect, one should incorporate behavioural insights and examine investors' behaviour.

One of the most pronounced behavioural explanations for the January effect is the tax-selling hypothesis, which claims that investors tend to sell stocks that have depreciated in value at year-end to offset realized capital gains in order to reduce their tax liability. In the United Stated, income tax regulations only apply to realized capital gains and losses which generally occur when the asset is sold. Hence, investors are incentivized to implement tax strategies at year-end. It is most preferable for an investor who has an unrealized gain in a particular stock to defer the taxation of the gain (lock-in effect). Vice versa, an immediate tax deduction is preferred to a differed tax deduction and, therefore, investors tend to sell loser stocks at year-end. As a result, the trading volume for stocks that performed well during the year tends to be exceptionally low, whereas the trading volume for loser stocks tends to be abnormally high. Dyl (1977) tests this hypothesis by examining the trading volume of one hundred stocks from the Center for Research in Securities Prices (CRSP) database from 1948 to 1970. The results indicate that trading volume was abnormally low for stocks that increased in value over the preceding year, thereby supporting the capital gains tax lock-in effect. Conversely, abnormally high trading volume is observed for stocks that depreciated over the preceding year, hence supporting the tax-loss selling hypothesis.

Furthermore, Reinganum (1982) similarly examines whether the January effect is associated with tax-loss selling and provides evidence in favour of the tax-loss selling hypothesis, however, that the effect is particularly generated by small stocks. Reinganum (1982) uses CRSP data from 1962 to 1980, thereby covering approximately 1500 firms in the mid-sixties and over 2500 firms in the mid-seventies. Whereas Watchtel (1942) argued that the high abnormal returns are generated before the middle of the month, Reinganum (1982) finds that most of the returns are generated in the first week. As a matter of fact, the return on the smallest firm portfolio generated a return over three percent already on the first trading day of January. To examine the tax-loss selling effect, Reinganum (1982) constructed stock portfolios based on market capitalization (to incorporate the size effect) and a measure of potential tax-loss selling (PTS). The high returns in the beginning of January were found to be related to both variables. Thus, small stocks with high PTS do, at least to some extent, explain the January effect.

Lakonishok and Smidt (1984) and Ritter (1988) both examine the buying and selling behaviour of individual investors at year-end in relation to the tax-loss selling hypothesis. Ritter (1988) tests his own parking-the-proceeds hypothesis, which is a generalization of the tax-loss selling hypothesis and argues that individuals tend to sell stocks in order to realize losses for tax purposes and 'park' the proceeds of the sale until early January (hence, not immediately reinvesting the proceeds). Consequently, buying pressure causes small stock prices to increase of which the majority is held by individual investors (in general, individual investors invest in low-priced, low-capitalization stocks). Ritter (1988) concludes that the January effect is a combination of investors' behaviour regarding the tax-loss selling hypothesis and the lag in reinvesting the proceeds from selling the stocks. Individual investors sell stocks in December that have declined in price for tax purposes, do not directly reinvest the proceeds from the stock sales, and wait until early January to invest in small stocks. As a results, prices of small stocks are driven upwards causing the January effect. Lakonishok and Smidt (1984) examine the trading volume and frequency of small stocks several days before and after the turn of the year. Similarly as Ritter (1988), they find high trading volume and frequency in December, supporting the tax-loss selling hypothesis. Both papers use CRSP data covering the seventies and eighties.

The January effect can, however, not be fully explained solely by the tax-loss selling hypothesis. If the January effect is caused merely by tax-loss selling in December, the magnitude and significance of the effect should, *ceteris paribus*, be related to the level of income tax rates on capital gains (Keim, 1982). E.g., the effect should be of lesser magnitude and significance prior to WWII, when tax rates on capital gains were relatively lower in the United States. Yet Keim (1982) finds that the January effect is larger, on average, in the 1930's compared to any period thereafter. Moreover, Tinic, Barone-Adesi, and West (1987) examine the January effect and the tax-loss selling hypothesis in Canada between 1950 and 1980. Prior to 1972, capital gains were not taxed in Canada which makes the dataset suitable to test the hypothesis. They conclude that the January effect was present in the entire time horizon (hence, before and after the implementation of taxes on capital gains). Lastly, Haug and Hirschey (2006) find that the seasonal effect is extraordinary consistent over time in small capitalization U.S. stocks and does not tend to be affected by the introduction of the Tax Reform Act in 1986. Thus, the January effect is only partly explained by the tax-loss selling hypothesis.

Another explanation of the January effect is the information hypothesis. As Rozeff and Kinney (1976, p. 393) explain: "January marks the beginning and ending of several potentially important financial and informational events. As examples of the latter, January is the start of the tax year for investors, the beginning of the tax and accounting years for most firms and the period during which preliminary (and in many cases final) announcements of the previous calendar (fiscal) year's accounting earnings are made." Hence, given the fact that most firms end their fiscal year in December, uncertainty about to-be-released accounting information increases during the turn of the year. As a result, the high information uncertainty in January results in higher risk and, thus, in higher expected returns. Additionally, this uncertainty effect might even be larger for small firms than for large firms since gathering information on small stocks is generally more costly than for large stocks. Unfortunately, empirical evidence on the information hypothesis is scarce, especially in relation to the tax-loss selling hypothesis.

Reinganum and Gangopadhyay (1991) test the information hypothesis by using firms listed on the AMEX and NYSE (which they refer to as the accounting-information hypothesis), thereby analysing monthly returns from 1963 to 1987. They attempt to answers two research questions. First, are stock returns abnormally higher one month after the fiscal year-end close, regardless of which month that would be? Second, are stock returns abnormally higher in January (on average), regardless of their fiscal year-end close month? However, their results are unable to support the information hypothesis. They find that the average returns in the month after the fiscal year-end close month are not significantly higher than the average returns in any of the other months. In addition, they find that the average returns on small firms are abnormally high in January, regardless of their fiscal year-end close month. The results indicate that the increase in uncertainty at the end of the calendar year cannot be attributed to the impending release of accounting information (Reinganum and Gangopadhyay, 1991).

Chen and Singal (2004) similarly examine, among other explanations for the January effect, the information hypothesis. They do not find support for the information hypothesis based on two tests. First, they argue that if the information hypothesis is correct, one would expect that returns for small firms are not only higher in January but also in April, July, and October because firms are required to release their accounting information each quarter. They test whether returns are significantly higher for small firms in July compared to any other month of the year, yet do not find any evidence which supports the statement. Second, the information hypothesis implies that the trading volume of small stocks should be lower in December than in January since investors wait until January when new information is expected. They test whether the trading volume is lower in December than in January and lower in June than in July. Conversely, the trading volume of small stocks was significantly higher in December than in January, and no significant difference in trading volume was found for June and July. Chen and Singal (2004) therefore conclude that the information hypothesis is not the main driver of the January effect. Given that empirical evidence on the information hypothesis is lacking and that Reinganum and Gangopadhyay (1991) and Chen and Singal (2004) find no evidence which supports the hypothesis, the information hypothesis does not seem a strong candidate for explaining the January effect.

A different behavioural clarification for the January effect, that does not lack empirical research, is the window dressing hypothesis. This hypothesis states that sophisticated investors tend to eliminate bad performing stocks from their portfolios in December (which, for most firms, is the end of the fiscal year) to improve the appearance of their performance to shareholders or clients. The investors then reinvest the proceeds of the sales in January, resulting in high abnormal returns due to high buying pressure. Lakonishok, Shleifer, Thaler, and Vishny (1991) examine the investment strategies of 769 U.S. equity pension funds between 1985 and 1989, thereby covering approximately 20 percent of the total actively managed pension equity holdings. They conclude that, indeed, portfolio managers sell underperforming stocks near the end of each quarter to impress shareholders or clients with their performance. The sale of underperforming stocks is higher for the fourth quarter and the results are stronger for small funds. Meier and Schaumburg (2006) examine the window dressing hypothesis by investigating the investment strategies of over four thousand U.S. domestic equity mutual funds (approximately half of the total U.S. mutual fund universe) over the period 1997 to 2002. They likewise conclude that a substantial part of mutual fund managers engage in window dressing activities during the last trading days before the end of the quarter. Agarwal, Gay, and Ling (2014) corroborate the findings on window dressing by mutual fund managers.

The issue with the tax-loss selling hypothesis and the window dressing hypothesis is how to distinguish between the two, as both hypothesis predict the same investor behaviour at the turn of the year. Chen and Singal (2004) attempt to make the distinction between the two hypotheses by looking at different points in time in the calendar year. The tax-loss selling hypothesis applies predominantly around the turn of the year, since for most individual and institutional investors the tax year ends in December. The window dressing hypothesis, on the contrary, applies to multiple times during the calendar year. In the United States, mutual fund managers are required by the Investment Company Act of 1940 to provide semi-annual accounting and performance information to shareholders. This gives mutual fund managers an incentive to engage in window dressing activities during the last trading days of June. Thus, the trading behaviour related to window dressing of these mangers in June-July should be similar to the trading behaviour related to window dressing in December-January. One can therefore observe the window dressing hypothesis in June-July without interference of the tax-loss selling effect because no tax-selling behaviour would be likely to occur in June-July. Chen and Singal (2004) examine the returns on the last five trading days in June and the first five trading days in July. The five-day returns in June and July vary between -2.0% and 1.6% and between -2.0% and 2.2%, respectively. Hence, no pattern in returns is found which supports the window dressing hypothesis in June-July. Furthermore, they examine the abnormal turnover during this period and find no significant difference in trading volume between the end of June and the beginning of July. Overall, Chen and Singal (2004) find no support for window dressing.

In a similar attempt to distinguish between the tax-loss selling hypothesis and the window dressing hypothesis, Sias and Starks (1997) examine differences in returns of portfolios held by individual investors (thereby testing the tax-loss selling hypothesis) and institutional investors (thereby testing the window dressing hypothesis) and test whether the January effect can be largely explained by the investment behaviour of either investor type. In summary, they find that U.S. stocks held mostly by individual investors exhibit substantially lower average returns at the end of December and substantially higher average returns in the beginning of January (consistent with the tax-loss selling hypothesis). Moreover, the tax-loss selling behaviour by individual investors is more important than the window dressing behaviour by institutional investors in explaining the January effect.

Evidence on the January effect in global stocks is rather scarce and mostly reported for developed markets only. E.g., Officer (1974) examines seasonality amongst Australian stocks and report evidence in favour of the January effect. Brown, Keim, Kleidon, and Marsh (1982) similarly examine Australian capital markets and support the findings of Officer (1974). Furthermore, they argue that Australia conducts a July-June tax year and one would therefore assume a July seasonal effect rather than a January effect. They do, however, find significant seasonal effects for both periods. The results of Brown et al. (1982) and those of Tinic et al. (1987) and Haug and Hirschey (2006) previously discussed illustrate that the tax-loss selling hypothesis is not the only explanation for the January effect. Thus, even though different countries might employ different tax regulations on capital gains, the effect might still be present due to the, for example, the information hypothesis or window dressing hypothesis.

To build upon the international evidence, Reinganum and Shapiro (1987) report evidence on return seasonality for stocks listed on the London Stock Exchange in January and in April (the tax year for British individual investors ends April 5th). Thereby examining the period prior to the implementation of taxes on capital gains in 1965 as well as the period ex post. No seasonality was detected before the implementation, whereas tax effects in January and April were present after the implementation. In addition, Jaffe and Westerfield (1985) examine Japanese capital markets between 1970 and 1983 and conclude that average abnormal stock returns are higher in January than in any other month of the year. In short, international evidence on the January effect is available but in far less quantities than for U.S. stock markets and mainly available for developed countries.

The January effect is a robust financial market anomaly and has been widely reported in the United States and to a lesser extend in developed markets during the late twentieth century, as discussed thus far. One would expect, on the other hand, that the trend of the effect should be fading over time as the efficient market hypothesis implies that arbitrageurs would step in and exploit the seasonal effect. The arbitrage opportunity is, however, restricted due to limits to arbitrage such as transaction costs, short selling constraints, and low levels of liquidity of small stocks. Stoll and Whaley (1983), for example, claim that small stocks face higher transaction costs than large stocks due to infrequent trading activity, higher risk, and higher broker commission fees (also see Keim, 1989). Consequently, these transaction costs take away most of the potential gains. Yet some researchers provide a methodology to trade on the January effect using the futures markets. Ziemba (2011) argues that transaction costs on index futures are over ten times smaller than those for a corresponding basket of stocks. And, more importantly, he argues that the market impact is much less. It might, therefore, be profitable for arbitrageurs to trade on the effect by applying a spread trade, i.e. taking long positions in small stock index futures and selling short large stock index futures. Hence, by buying and selling futures contracts, one is able to exploit the January effect. One way to do so is by trading futures on the Value Line Index (VL) and the S&P500 Index (the VL/S&P spread). The VL/S&P spread is the difference between the VL and S&P indices (Ziemba, 2011). Clark and Ziemba (1987) apply the following trading rule for the 1984/1985, 1985/1986, and 1986/1987 turn of the year: buy the spread on December 15th and sell the spread on January 15th. By doing so, they manage to earn substantial profits using real money (see Clark and Ziemba (1987) for more detail). Hensel and Ziemba (2000) updated the findings by Clark and Ziemba (1987) by applying the spread trades in the late 1980s and 1990s. They conclude that one could exploit the January effect in the futures markets during this time period, with exception of the 1994-1998 turns of the year where the effect was present only in the second half of December (again, by examining the VL/S&P spread). In turn, Rendon and Ziemba (2007) examined the turn of the year from 1998 to 2005 and show that the January effect remains robust in the futures market during the time period. Furthermore, they find that besides the VL minus S&P500 spread trade the Russell2000 minus S&P500 spread trade also deems to be profitable. Thus, the spread trade seems to be robust from the late 1980s until the early 2000s.

This paper does not focus on trying to determine what the behavioural driver behind the January effect is (tax-loss selling, window dressing, information hypothesis, or a combination of the three), it rather focuses on examining whether the effect is present in developed and emerging markets. As previously discussed, the international evidence on the effect is scarce as most researchers focus on U.S. stock markets. As for developed markets, Officer (1974) and Brown et al. (1982) focus on Australian stock markets, Reinganum and Shapiro (1987) examine the London Stock Exchange, and Jaffe and Westerfield (1985) investigate whether the effect is present in Japanese stock markets. The focus is therefore mainly on large developed markets, leaving out most small developed markets and definitely not considering emerging markets. In addition, most of the existing research is outdated since it incorporates data from the seventies, eighties, and nineties. This paper examines a wide array of stocks, thereby covering approximately 85 percent of the global stock market and it uses a recent time period. As a result, it includes the smaller developed markets and, perhaps even more relevant, many emerging markets which are barely covered in the existing literature at all.

SECTION III: METHODOLOGY

A handful of authors, e.g. Wachtel (1942), Dyl (1977), and Rozeff and Kinney (1976), do not distinguish between large and small stocks when examining the January effect, whereas most other authors suggest the effect is merely observable in small stocks. This paper, therefore, examines the effect using two approaches. Firstly, it examines whether the January effect is present in the total sample (developed plus emerging markets), developed markets sample, and emerging markets sample by forming a value weighted market proxy for each of the three samples (i.e., no distinction between large stocks and small stocks). Hence, for each sample the following time-series regression is performed:

$$R_{m,t} = \alpha_1 + \alpha_2 D_{2t} + \ldots + \alpha_{12} D_{12t} + e_t , \quad (1)$$

where $R_{m,t}$ is the return on the value weighted market proxy in month t, the intercept α_1 is the return in January and dummy coefficients α_2 through α_{12} represent the difference in returns between January and each respective month. The dummy variables D_2 through D_{12} indicate the month of the year in which the return is observed (D_{2t} = February, D_{3t} = March, etc.) and e_t is the error term. The next section discusses which stocks comprise the market proxy for each of the three samples.

Secondly, this paper examines to what extent the January effect is related to size. For each sample (total sample, developed markets, and emerging markets) the stocks are ranked on market capitalization (stock price times number of shares outstanding) at the beginning of each year. The stocks are subsequently allocated into equal weighted decile portfolios, i.e., the stocks are ranked on size and an equal weight is assigned to each stock. For each decile portfolio, the following time-series regression is performed over the full period:

$$R_{i,t} = \alpha_1 + \alpha_2 D_{2t} + \ldots + \alpha_{12} D_{12t} + e_t , \quad (2)$$

where $R_{i,t}$ is the return on equal weighted decile i in month t. The remaining variables are equal to those in equation (1). Thus, three samples of stocks are examined on the presence of the January effect and its relation with size. The stocks of the total sample (developed plus emerging markets) are ranked on market capitalization each year and allocated into equal weighted decile portfolios, the stocks of the developed markets sample are ranked on market capitalization each year and allocated into equal weighted decile portfolios, and the stocks of the emerging markets sample are ranked on market capitalization each year and allocated into equal weighted decile portfolios. Subsequently, equation (2) is performed on each decile in each respective sample.

To test whether the January effect is present, three conditions must be met. Firstly, the intercept (which is the return in January) must have a significant effect on $R_{m,t}$ and $R_{i,t}$ in equations (1) and (2), respectively. Secondly, the significant return for January must be statistically higher than for any other month of the year, i.e., the dummy coefficients (difference in return between January and each respective month) for months February to December must be negative and significant. Lastly, the F-statistic must be significant. I.e., if seasonality is present, one should be able to reject the null-hypothesis that the dummy coefficients are statistically the same and close to zero. The hypotheses for the first condition is as follows:

H_0: The return for January, α_1, has a significant effect on the dependent variable.

H_1: The return for January, α_1, does not have a significant effect on the dependent variable.

The second condition speaks for itself. The null- and alternative hypotheses regarding the F-test are:

$$H_0: \alpha_1 = \alpha_2 = \ldots = \alpha_{12}$$

$$H_1: \alpha_1 \neq \alpha_2 \neq \ldots \neq \alpha_{12}$$

SECTION IV: DATA DESCRIPTION

The data approximately covers the MSCI All Country World Index (ACWI) which is comprised of 23 developed markets and 24 emerging markets. The categorization of countries into developed or emerging markets is carried out by Morgan Stanley. The 23 developed markets are:

- Americas: Canada, United States
- Europe and Middle East: Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Israel, Italy, Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom
- Pacific: Australia, Hong Kong, Japan, New Zealand, Singapore

The 24 emerging markets are:

- Americas: Brazil, Chile, Colombia, Mexico, Peru
- Europe, Middle East, and Africa: Czech Republic, Egypt, Hungary, Poland, Qatar, Russia, South Africa, Turkey, United Arab Emirates
- Asia: China, India, Indonesia, Korea, Malaysia, Pakistan, Philippines, Taiwan, Thailand

Unfortunately, the databases at Erasmus University do not have access to the MSCI ACWI constituents list, merely to various MSCI indices. Thus, for each country the constituents of the most frequently used index are obtained from Datastream and Worldscope in order to mimic the MSCI ACWI (e.g., AEX for the Netherlands, S&P 500 for the USA, DAX for Germany, etc.), thereby taking into account the size of each country's economy compared to the index as a whole. However, retrieving constituents' ISIN-numbers (International Securities Identification Number, i.e., company specific codes in order to retrieve data per constituent) of an index from Datastream can only be done per month through a static request. Given the fact that 47 country indices are examined over the period 1990-2016 (324 months), it would be extremely time consuming to retrieve the constituents' ISIN-numbers of each index in each month of the entire period examined. Alternatively, the ISIN-numbers of the constituents of each of the 47 country indices are retrieved only for December 2016. These ISIN-number are then used to retrieve the needed variables of each constituent from Datastream and Worldscope, stock price and number of shares outstanding, through a time-series request for the period 1990-2016. By combining the data, one mimics the MSCI ACWI. The downside of this approach is that the number of stocks examined decreases each year moving back in time, since not all companies are part of the mimicked index for the entire period. Nevertheless, for both developed and emerging markets data on a substantial amount of stocks is available. Table I below describes the data in more detail.

Table I

Number of stocks observed each year, 1990-2016.

	Developed markets	Emerging markets	Total sample
1990	806	139	945
1991	822	175	997
1992	881	223	1104
1993	923	254	1177
1994	960	295	1255
1995	999	318	1317
1996	1043	365	1408
1997	1086	418	1504
1998	1131	456	1587
1999	1175	480	1655
2000	1225	524	1749
2001	1259	563	1822
2002	1289	593	1882
2003	1309	617	1926
2004	1347	680	2027
2005	1384	743	2127
2006	1413	785	2198
2007	1447	849	2296
2008	1464	886	2350
2009	1476	925	2401
2010	1495	983	2478
2011	1523	1014	2537
2012	1536	1035	2571
2013	1560	1050	2610
2014	1609	1063	2672
2015	1637	1087	2724
2016	1649	1109	2758

The dataset is, therefore, prone to survivorship bias. Section VII, however, explains why this has a minimal effect on the results. Lastly, 1990 has been chosen as the first year since the number of stocks for emerging markets dramatically decreases moving further back in time (data on 85 emerging market stocks is available for 1989, 65 for 1988, and only 39 for 1987). This is due to the fact that most emerging markets did not have a country index at that time. Including two additional years, 1989 and 1988, to the sample would perhaps be justifiable when examining the aggregate markets proxy, however, less than ten stocks per decile portfolio would then be examined in the second approach during the first two years, which is insufficient.

SECTION V: RESULTS

In the first approach, regression equation (1) is performed on the total sample, developed markets sample, and emerging markets sample, thereby incorporating all stocks to form the value weighted market proxy (hence, no distinction between small and large stocks). Table II below reports the results of the time-series regressions. As equation (1) suggests, the intercept is the return for January. The results for February to December indicate the difference in return between January and the respective month. The stars indicate whether the difference between the return in January and the respective month is significant at different significant levels (results are deemed to be significant if $p < 0.05$) . Clearly, the January effect is not present in any of the samples since the three conditions discussed in section III are not met. Table II shows that although the returns in January for the total sample and emerging markets are significant, the dummy coefficients for February to December are not significantly negative indicating that the difference in returns between January and the respective month is indifferent from zero. In addition, the F-statistics presented on the right side in the table indicate that, for all samples, the null-hypothesis of no difference in returns across months is accepted at conventional significant levels (i.e., no seasonal effect in stock returns). If a p-value of 0.10 or smaller would be accepted, the results in table II suggest that the weak form of seasonality for the total sample and developed markets sample can be attributed to the negative returns in June and September for the former, and the negative return in September for the latter. As Chen and Singal (2004) explain, the window dressing hypothesis applies to multiple periods in the calendar year, more specifically, at the end of each quarter. Portfolio managers tend to sell off bad performing stocks to boost the appearance of their portfolio performance to shareholders and clients. In this case, it appears that they sell of their stocks at the end of June. Selling pressure increases and prices tend to drop. The negative return in September might be attributed to the September effect, which argues that investors come back from their vacation in this month and return to their trading desks and pick-up their trading activities. As a result, volatility tends to increase which has a negative effect on stock returns. An alternative explanation for the September effect is that investors tend to sell part of their portfolio in order to pay for their children's schooling costs. The September effect, however, is inconsistent and not robust over time. It is thought of as a quirk in historical stock data rather than a robust financial market anomaly. The explanations for the returns in June and September only apply if a p-value of 0.10 or smaller is considered. The most important finding in table III is the absence of the January effect in all three samples if one would not make a distinction between small stocks and large stocks.

Table II

Estimates of variation in month by month average monthly returns (%) for the value weighted market proxy for the total sample (developed plus emerging markets), developed markets, and emerging markets, over the sample period 1990 to 2016. T-statistics are in parentheses. As equation (1) suggests, the intercept is the return in January and the dummy coefficients for February to December are the differences in return between January and the respective month.

	Jan.	Feb.	March	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	F-statistic
$R_{DM + EM}$	1.81**	-0.01	0.10	0.91	-0.42	-1.88*	-0.47	-1.83	-2.10*	0.01	-0.43	0.90	1.64*
	(2.30)	(-0.08)	(0.09)	(0.82)	(-0.38)	(-1.69)	(-0.43)	(-1.64)	(-1.89)	(0.01)	(-0.39)	(0.81)	
R_{DM}	1.41*	-0.21	0.46	1.30	-0.37	-1.63	-0.57	-1.57	-2.11*	0.36	-0.11	1.07	
	(1.72)	(-0.18)	(0.40)	(1.13)	(-0.32)	(-1.41)	(-0.49)	(-1.36)	(-1.83)	(0.31)	(-0.09)	(0.92)	1.68*
R_{EM}	2.82***	0.14	-0.51	-0.23	-0.93	-2.28*	-0.22	-2.25	-2.22*	-0.93	-1.13	0.59	
	(2.97)	(0.11)	(-0.38)	(-0.17)	(-0.69)	(-1.70)	(-0.16)	(-1.67)	(-1.65)	(-0.70)	(-0.84)	(0.44)	1.07

*** p<0.01, ** p<0.05, * p<0.10

The second approach does distinguish between small stocks and large stocks. Table III below presents the results for the equally weighted decile portfolios for the total sample, developed plus emerging markets (regression equation (2)). The January return of 5.25% in the smallest decile is significant at conventional significant levels. However, the dummy coefficients for February, April, May, and July are not significantly negative, hence the difference between the return in January and the respective month is indifferent from zero. Moreover, the F-statistic of 1.54 suggests that there is no difference in returns across months. The three conditions for the existence of the January effect are therefore not met. Similarly, for deciles two to four, the return in January is significant, though the returns of most of the other months are not significantly negative and the F-statistic is only marginally significant. For deciles one and two, the window dressing in June seems to be present as the return in June is substantially lower than in January. The returns in September for deciles one to four are also substantially lower than in January, which might be attributed to the (weak) September effect, as discussed previously.

The results for the decile portfolios for the developed markets sample are presented in table IV further below. The strong January effect is clearly visible in decile 1 with a magnitude of 5.69%. The returns in the remaining months are significantly negative and the F-statistic of 2.38 implies the rejection of the null-hypothesis of no differences across monthly returns. Interestingly, the effect is merely observed in the smallest decile since the three conditions do not hold from decile 2 onwards. One might expect the effect to be similarly present in decile 2 or 3, as these are still the twenty and thirty percent smallest stocks, respectively. Yet, keep in mind that the selection of stocks in the sample mimics the MSCI ACWI, implying that these stocks are mostly the largest stocks of 23 developed and 24 emerging markets. The smallest decile in the sample already contains relatively large stocks, especially in comparison to various authors in section II which examine several thousand CRSP stocks or the NYSE (2700 U.S. stocks). The stocks in the smallest deciles in those samples are probably considerably smaller in size than those in decile 1 in this paper. Alternatively, one could argue that the January effect is fading away and is simply not as robust anymore as it used to be in the seventies, eighties, and nineties. Nevertheless, the January effect is present for decile 1.

In addition, tables III and IV argue that the returns in January for small stocks (e.g., deciles one to three) are high in general. Although the t- and F-statistics might not confirm that

Table III

Estimates of variation in month by month average monthly returns (%) for the equally weighted size decile portfolios of the total sample (developed plus emerging markets), over the sample period 1990 to 2016. The decile portfolio are constructed in the beginning of each year. T-statistics are in parentheses. As equation (1) suggests, the intercept is the return in January and the dummy coefficients for February to December are the differences in return between January and the respective month.

Decile	Jan.	Feb.	March	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	F-statistic
Smallest	5.25***	-1.91	-3.11**	-2.24*	-2.56*	-4.93***	-1.70	-3.67***	-3.72***	-2.83**	-2.86**	-2.17**	1.54
	(5.70)	(-1.42)	(-2.31)	(-1.66)	(-1.90)	(-3.44)	(-1.26)	(-2.73)	(-2.77)	(-2.11)	(-2.12)	(-1.61)	
2	3.75***	-0.72	-1.30	-0.27	-0.99	-3.14**	-1.71	-2.99**	-3.44***	-2.45**	-1.53	-0.79	1.72*
	(4.26)	(-0.58)	(-1.04)	(-0.22)	(-0.80)	(-2.52)	(-1.37)	(-2.40)	(-2.77)	(-1.97)	(-1.23)	(-0.63)	
3	2.94***	-0.10	-0.65	0.03	-0.96	-2.38	-1.23	-2.77**	-2.88**	-1.25	-1.13	0.27	1.61*
	(3.40)	(-0.08)	(-0.53)	(0.02)	(-0.78)	(-1.95)	(-1.04)	(-2.27)	(-2.36)	(-1.02)	(-0.93)	(0.22)	
4	2.10**	-0.30	0.13	0.81	-0.52	-1.96	-0.63	-2.16*	-2.53**	-0.18	-0.63	1.20	1.81*
	(2.47)	(-0.25)	(0.11)	(0.68)	(-0.43)	(-1.62)	(-0.52)	(-1.80)	(-2.11)	(-0.15)	(-0.53)	(1.00)	
5	1.55*	0.41	0.66	1.29	-0.10	-1.79	-0.70	-1.79	-2.13*	0.46	-0.31	1.09	1.83**
	(1.83)	(0.55)	(1.08)	(1.08)	(-0.09)	(-1.49)	(-0.58)	(-1.50)	(-1.77)	(0.39)	(-0.26)	(0.91)	
6	1.18	-0.07	0.52	1.51	-0.11	-1.23	-0.38	-2.02*	-1.68	0.57	0.27	1.58	1.79*
	(1.41)	(-0.06)	(0.44)	(1.28)	(-0.10)	(-1.04)	(-0.32)	(-1.71)	(-1.42)	(0.48)	(-0.23)	(1.33)	
7	0.48	0.29	0.87	1.97*	0.37	-0.96	-0.07	-1.21	-1.38	1.31	0.36	2.01*	1.91**
	(0.59)	(0.25)	(0.75)	(1.71)	(0.32)	(-0.83)	(-0.06)	(-1.05)	(-1.20)	(1.14)	(0.31)	(1.74)	
8	0.51	0.17	0.83	1.73	0.24	-1.09	0.05	1.21	-1.42	1.44	0.39	1.74	1.86**
	(0.64)	(0.15)	(0.74)	(1.54)	(0.22)	(-0.96)	(0.04)	(-1.08)	(-1.26)	(1.28)	(0.35)	(1.54)	
9	0.04	0.43	1.36	2.30**	0.52	-0.69	0.25	-0.93	-1.10	1.66	0.68	2.13*	1.95**
	(0.05)	(0.38)	(1.17)	(1.99)	(0.45)	(-0.60)	(0.22)	(-0.81)	(-0.96)	(1.44)	(0.58)	(1.84)	
Largest	-0.23	0.18	1.11	2.44**	0.31	-0.17	0.57	-0.94	-0.83	2.20*	1.15	2.41**	1.96**
	(-0.27)	(0.16)	(0.93)	(2.05)	(0.26)	(-0.14)	(0.48)	(-0.79)	(-0.70)	(1.85)	(0.96)	(2.02)	

*** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table IV

Estimates of variation in month by month average monthly returns (%) for the equally weighted size decile portfolios of the developed markets sample, over the sample period 1990 to 2016. The decile portfolio are constructed in the beginning of each year. T-statistics are in parentheses. As equation (1) suggests, the intercept is the return in January and the dummy coefficients for February to December are the differences in return between January and the respective month.

Decile	Jan.	Feb.	March	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	F-statistic
Smallest	5.69***	-3.10**	-3.18**	-1.90**	-4.36***	-5.80***	-2.48**	-3.81***	-3.87***	-3.31**	-3.53**	-2.67**	2.08**
	(5.84)	(-2.25)	(-2.30)	(-1.98)	(-3.16)	(-4.21)	(-2.12)	(-2.76)	(-2.81)	(-2.40)	(-2.56)	(-2.16)	
2	2.98***	-0.56	-0.73	0.59	-0.36	-2.63**	-1.41	-2.13*	-3.02**	-1.27	-0.99	-0.12	1.50
	(3.33)	(-0.45)	(-0.58)	(0.46)	(-0.29)	(-2.08)	(-1.12)	(-1.69)	(-2.39)	(-1.00)	(-0.78)	(-0.10)	
3	2.44***	-0.19	-0.24	0.58	-0.45	-2.38*	-1.44	-2.07	-2.94***	-0.49	-0.49	0.77	1.62*
	(2.65)	(-0.14)	(-0.19)	(0.44)	(-0.35)	(-1.83)	(-1.10)	(-1.59)	(-2.25)	(-0.38)	(-0.35)	(0.59)	
4	1.78**	-0.04	0.74	1.17	-0.43	-1.55	-0.71	-1.45	-2.55**	-0.08	-0.25	1.31	1.61*
	(1.99)	(-0.03)	(0.59)	(0.92)	(-0.34)	(-1.23)	(-0.56)	(-1.14)	(-2.01)	(-0.06)	(-0.20)	(1.04)	
5	1.73**	-0.06	0.81	1.33	-0.55	-1.86	-1.09	-1.64	-2.59**	0.16	-0.44	0.71	1.83**
	(1.99)	(-0.05)	(0.66)	(1.08)	(-0.45)	(-1.51)	(-0.88)	(-1.34)	(-2.11)	(0.13)	(-0.36)	(0.58)	
6	1.27	-0.33	0.59	1.44	-0.33	-1.35	-0.71	-1.74	-1.94	0.45	-0.18	1.08	1.57
	(1.50)	(-0.27)	(0.49)	(1.20)	(-0.28)	(-1.13)	(-0.59)	(-1.45)	(-1.61)	(0.37)	(-0.15)	(0.90)	
7	0.51	0.28	1.02	2.04*	0.41	-0.94	-0.28	-0.98	-1.49	1.11	0.61	1.84	1.76*
	(0.62)	(0.24)	(0.87)	(1.72)	(0.35)	(-0.80)	(-0.24)	(-0.83)	(-1.26)	(0.95)	(0.52)	(0.61)	
8	0.51	0.17	0.98	1.78	0.15	-0.94	0.04	-1.10	-1.52	1.46	0.49	1.62	1.78*
	(0.63)	(0.15)	(0.86)	(1.55)	(0.13)	(-0.82)	(0.04)	(-0.96)	(-1.33)	(1.28)	(0.43)	(1.41)	
9	0.30	0.07	1.22	2.03*	0.26	-0.93	0.00	-1.09	-1.41	1.42	0.59	1.83	1.91**
	(0.36)	(0.06)	(1.05)	(1.76)	(0.23)	(-0.80)	(0.00)	(-0.94)	(-1.22)	(1.22)	(0.51)	(1.58)	
Largest	-0.15	0.12	1.06	2.39**	0.26	-0.15	0.54	-0.96	-0.92	2.08*	1.10	2.29*	1.87**
	(-0.18)	(0.10)	(0.89)	(2.00)	(0.22)	(-0.13)	(0.45)	(-0.80)	(-0.77)	(1.74)	(0.92)	(1.92)	

*** p<0.01, ** p<0.05, * p<0.10

the return in the other months are statistically different and that seasonality is not always statistically present, both tables do illustrate that small stocks tend to perform substantially well in January. The period examined is 1990-2016, as mentioned previously. If this period could have been extended, the absolute values of the t- and F-statistics might have increased to such an extent that one might have observed the presence of the January effect in deciles one and two in table III. The returns in the other months are lower than in January in the two deciles, however, the t-statistic does not confirm that the difference in return between January and the other months is statistically different from zero. The F-statistic implies no seasonality, yet if a longer time horizon would have been examined, these statistics might have increased to conventional significant levels.

Furthermore, although not always significant, the returns in January systematically decrease from the smallest to the largest decile in both tables III and IV. These results are in line with Brown et al. (1983), Keim (1982), and Reinganum (1982) and indicate that the January effect, if present, is generated by small stocks. Lastly, the negative return in June for decile one in table IV again supports the window dressing hypothesis (similarly as in table III). The poor performance of small and mid-sized stocks is again visible in September for deciles one to five.

The results for emerging markets are presented in table V below. Although the returns for January are higher than for any other month of the year in the smallest two deciles, several of the dummy coefficients and the F-statistics are insignificant confirming the absence of a January effect (similarly as the results of decile one and two in table III). Again, the underperformance of small stocks in June and September is visible as also observed in tables III and IV. Interestingly, August seems to be a bad month for small and mid-sized stocks as well. The low returns for August in table III are clearly driven by results for August in the emerging markets sample. Other authors, who do however focus mostly on U.S. stock markets, do not find similar results for August. Given the fact that no such thing as an August effect exists, the poor performance of small and mid-sized stocks in August is likely to be a data dependent result. Overall, there seem to be relatively few seasonality effects in emerging markets, considering the F-statistics in table V.

Several conclusions can be drawn from the results in presented in tables II to V. Firstly, the January effect is not present in the total sample, developed markets sample, or emerging

Table V

Estimates of variation in month by month average monthly returns (%) for the equally weighted size decile portfolios of the emerging markets sample, over the sample period 1990 to 2016. The decile portfolio are constructed in the beginning of each year. T-statistics are in parentheses. As equation (1) suggests, the intercept is the return in January and the dummy coefficients for February to December are the differences in return between January and the respective month.

Decile	Jan.	Feb.	March	April	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	F-statistic
Smallest	4.63***	-0.95	-2.85*	-2.60	-1.34	-3.50***	-0.64	-3.18**	-3.50***	-1.98	-2.30	-1.53	1.03
	(4.07)	(-0.59)	(-1.77)	(-1.61)	(-0.84)	(-2.17)	(-0.40)	(-1.97)	(-2.18)	(-1.23)	(-1.43)	(-0.95)	
2	4.45***	-0.76	-1.74	-1.25	-1.79	-3.27***	-2.04	-3.91***	-3.96***	-3.35**	-2.12	-1.46	1.33
	(4.11)	(-0.49)	(-1.13)	(-0.82)	(-1.16)	(-2.13)	(-1.33)	(-2.55)	(-2.58)	(-2.18)	(-1.38)	(-0.95)	
3	3.37***	0.20	-1.07	-0.52	-1.61	-1.66	-0.47	-3.60**	-2.89*	-2.31	-1.88	1.23	1.34
	(3.16)	(0.13)	(-0.71)	(-0.35)	(-1.07)	(-1.10)	(-0.31)	(-2.39)	(-1.92)	(-1.53)	(-1.24)	(0.08)	
4	2.95***	-1.25	-1.63	-0.26	-1.48	-2.64*	-0.65	-3.63**	-2.82*	-0.51	-1.41	1.18	1.56
	(2.76)	(-0.82)	(-1.08)	(-0.17)	(-0.98)	(-1.75)	(-0.43)	(-2.40)	(-1.87)	(-0.34)	(-0.93)	(0.78)	
5	1.59	1.62	-0.16	0.53	0.07	-1.47	0.03	-2.70*	-2.16	-0.02	-0.56	2.00	1.52
	(1.43)	(1.03)	(-0.10)	(0.34)	(0.04)	(-0.93)	(0.02)	(-1.72)	(-1.37)	(-0.01)	(-0.36)	(1.27)	
6	1.50	0.53	-0.23	0.77	-0.49	-1.00	-0.07	-3.85**	-1.51	0.53	-0.89	3.33**	2.20**
	(1.33)	(0.33)	(-0.14)	(0.49)	(-0.30)	(-0.63)	(-0.04)	(-2.42)	(-0.95)	(0.33)	(-0.55)	(2.09)	
7	0.92	-0.51	-0.46	0.78	-0.69	-1.05	-0.05	-2.67*	-1.95	1.56	-1.09	2.83*	2.03**
	(0.88)	(-0.35)	(-0.31)	(0.53)	(-0.47)	(-0.71)	(-0.03)	(-1.81)	(-1.32)	(1.06)	(-0.74)	(1.92)	
8	0.91	-0.30	-0.42	0.01	0.31	-1.57	-0.83	-1.86	-2.41	0.57	-0.09	3.57**	1.42
	(0.72)	(-0.17)	(--0.23)	(0.00)	(0.17)	(-0.88)	(-0.46)	(-1.04)	(-1.34)	(0.32)	(-0.05)	(1.99)	
9	-1.22	3.25*	1.51	3.25*	1.28	0.82	1.95	-0.77	0.52	2.37	1.87	3.86**	1.12
	(-1.01)	(1.74)	(0.81)	(1.74)	(0.69)	(0.44)	(1.04)	(-0.41)	(0.28)	(1.27)	(1.00)	(2.07)	
Largest	0.50	-1.73	-0.47	0.33	-2.26	-1.16	-0.49	-1.64	-1.66	1.51	0.97	1.35	0.70
	(0.33)	(-0.80)	(-0.22)	(0.15)	(-1.04)	(-0.54)	(-0.22)	(-0.75)	(-0.76)	(0.70)	(0.45)	(0.62)	

*** p<0.01, ** p<0.05, * p<0.10

markets sample when examining the value weighted market proxy that mimics the MSCI ACWI. Secondly, after constructing equally weighted decile portfolios based on size for each sample, the January effect exists for the smallest decile of the developed markets sample. Decile one of the total sample (table III) and decile one and two of the emerging markets sample (table V) show that although the return in January is highest, the difference between January and the other months cannot be statistically confirmed. In addition, the insignificant F-statistic implies no seasonality in stock returns for these deciles. Hence, the January effect is not present in these two samples. Thirdly, for all three samples the January returns decline systematically from decile one to ten. These results are in line with existing literature and indicate that the January effect is generated by small stocks, if one is able to detect its existence in the first place.

Regarding the hypotheses, at last, the following statements can be made. The first null-hypothesis, which that implies the intercept (return in January) has a significant effect on the dependent variable R_i, is accepted for the total sample and emerging markets sample in table II, deciles one to four in table III, deciles one to five in table IV, and deciles one to four in table V. For these deciles, however, the second null-hypothesis, which implies that the dummy coefficients are statistically the same and close to zero, is accepted for all samples in table II, deciles one to four in table III, deciles two to four in table IV, and deciles one to four in table V. Together with the third condition discussed in section III (the return in January is higher than in any other month of the year), the January effect is only observed in decile one of the developed markets sample.

SECTION VI: CONCLUSION

The January effect has been extensively examined throughout the past decades, however, with the focus predominantly on U.S. stock markets. Regardless of whether the effect is explained by behavioural phenomena such as the tax-loss selling hypothesis, information hypothesis, the window dressing hypothesis, or a combination of the three, research shows the effect is robust over time in these markets. This paper, on the other hand, sheds light on the presence (or absence) of the January effect in international stock markets, thereby additionally examining a recent time period. By analysing a large array of stocks that mimic the MSCI ACWI, several conclusions can be drawn from the regression results. The first approach examines whether the January effect is present in the total set of stocks for three market segments, namely developed plus emerging markets, developed markets, and emerging markets (i.e., no distinction between small stocks and large stocks). The results show that the January effect is absent in all three samples. The returns in January are significant for the total sample and emerging markets, though the F-statistics are insignificant and the return in January is not higher than in other months of the year. In the second approach, the smallest decile for the total sample and deciles one and two for the emerging markets sample experience the highest return in January compared to the other months, however, the insignificant t-statistics for several of the other months indicate that the difference in return between January and the respective month cannot be statistically confirmed. In addition, the low F-statistic implies no seasonal effects in stock returns across months. The insignificant results for emerging markets might be attributed to the time horizon of the data. If more years could have been added, the power of the t- and F-tests might have increased. Unfortunately, this data was not available as explained in section IV. The results for developed markets, on the other hand, indicate that the January effect is present in the smallest decile.

Moreover, the results on developed markets are in line with existing literature even though the literature mostly focuses on U.S. stock markets and large developed markets, thereby leaving out smaller developed markets. How the results of the emerging markets sample compare to the literature is difficult to argue, since research on the January effect in such a broad sample of emerging markets is scarce. The systematic decrease in January returns from decile one to ten for both developed and emerging markets is, on the other hand, in line with the results of Brown et al. (1983), Keim (1982), and Reinganum (1982). Finally, for developed markets one might expect the effect to be equally present in deciles two or three as these are still the twenty to thirty percent smallest stocks (e.g., see Keim (1982) and Reinganum (1982)).

Yet, the stocks examined in this paper are already amongst the largest stocks globally, as discussed in section IV.

SECTION VII: DISCUSSION

The data used in this paper is prone to survivorship bias, as mentioned earlier. The databases at Erasmus University do not have access to the MSCI ACWI constituents list, hence an alternative index is constructed as discussed in section IV. The issue that arises is that the number of stocks examined decreases moving back in time, as shown in table I, since only survivor stocks stay in the index throughout the period. This paper, however, does not examine some sort of trading strategy of which the return would be biased upwards due to survivorship bias. Indeed, the magnitude of the January effect in decile one of the developed markets sample might be slightly biased upwards because of the survivor stocks, though the focus of this paper is on the presence of the January effect in developed and emerging markets instead of its magnitude. The January effect implies that stock returns in January are higher than for any other month of the year and although the magnitude of the effect found in this paper might be biased upwards, so are the returns in other eleven months. Thus, the results found in this paper regarding the presence or absence of the January effect should not be harmed by the survivorship biased issue in the dataset. Further research should focus on acquiring the complete constituents list for the MSCI ACWI for each point in time, thereby removing the survivorship bias, and compare the results with the results found in this paper.

Furthermore, it would be preferred in further research to extent the period examined for emerging markets. T- and F-statistics are likely to increase, such that one can statistically confirm the difference between January and the other months to be significantly negative, as discussed in section V. As a result, one might be able to detect the January effect for emerging markets.

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