The Modigliani-Miller theorems: a cornerstone of finance [*]

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Almost eight years ago, Franco Modigliani agreed to deliver a lecture at the Master in Economics and Finance at the Università di Napoli Federico II. When I asked him which topic he would deal with, he answered with a smile and a twinkle in his eye: "Modigliani-Miller, of course!". So in June 1997, twenty attentive and excited students had the unique opportunity of being taught the MM theorems – as they are commonly known – by one of its two authors. In his typical lively style, Franco kept discussing with the students about the implications of the theorems well beyond the time scheduled for the lecture.

In March 2003, only few months before his demise, I was at MIT and witnessed Franco still teaching with the same enthusiasm another class at the Sloan School of Management. Franco had asked me to wait for him at the end of his afternoon lecture. I waited and waited outside a classroom packed with MBA students, and through the glass pane of the door I could see Franco taking loads of questions from the students and debating with them in a lively manner. When he finally came out together with an animated crowd of students, I told him: "I see that the students liked your class: what are you teaching?". He replied with the usual twinkle in his eye: "The course is named 'Modigliani on Modigliani'". Despite the long lecture, he looked relaxed and energetic. We went for dinner with Jonathan Lewellen, a young professor of the Sloan School, and to my surprise I learnt that Franco and Jonathan were planning to write a paper on a new test of MM, based on data for closed-end fund prices. Franco was very excited about it, as he felt that this was one of the cases in which the theorem should

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apply most fittingly, and preliminary estimates obtained up to that point were firmly in support of the MM predictions. Unsurprisingly, a good deal of the dinner was spent discussing econometric problems, talking of regression coefficients and peering over computer output.

I am reporting this not only to recall Franco's contagious and unflagging enthusiasm for teaching and research, but more specifically to underline the importance that he attached to the MM theorems. Indeed not only are these his most important contributions to financial economics, but are universally considered as a cornerstone of the modern theory of finance, as it has developed in the last half-century. Today, no course in corporate finance can start without explaining the MM theorems, and no researcher could think clearly about corporate finance without them.

There are two main reasons why these results are a cornerstone of teaching and research in finance. The first is *substantive*: it stems from their nature of 'irrelevance propositions', which provide a crystal-clear *benchmark* case. The second is *methodological*, and has to do with their reliance on an *arbitrage* argument, which set a precedent not only within the realm of corporate finance but also – and even more importantly – within that of asset pricing.

1. Benchmark value of MM as 'irrelevance propositions'

Modigliani and Miller produced two propositions, the first concerning the invariance of firm value to its capital structure and the other concerning its invariance to dividend policy. But it is the first of these two propositions that has always attracted most of the attention, including that of MM. Indeed, they produced the dividend invariance proposition mainly to deflect criticisms of their first proposition.

The first MM theorem states the conditions under which the choice between debt and equity to finance a given level of investment does not affect the value of a firm, implying that there is no optimal leverage ratio. The second MM theorem shows that under the same conditions also dividend policy does not affect a firm's value, so that there is no optimal payout ratio. So both theorems belong to a class of surprising results known in economics as 'irrelevance propositions' – otherwise labelled "neutrality propositions" or "invariance proposi-

tions". These are theorems that show the irrelevance of a choice that at first sight would seem very important, such as the capital structure decision and the dividend decision.

The virtue of this type of results does not lie in proving that the specified choice is truly irrelevant, but rather in forcing us to think hard about the assumptions that are necessary for it to be relevant. In other words, these results provide a benchmark with which we must constantly reckon, whenever we think of the choice under scrutiny. As soon as we utter the words 'optimal leverage' or 'optimal payout ratio', we must immediately wonder: "Why in this case MM does not apply?" and detect the assumption or the set of assumptions that took us away from the benchmark case. This requires a healthy dose of intellectual discipline and analytical clarity. It is the main reason that the MM propositions are about the most quoted results in the theory of finance.

The very words of Merton Miller witness that this is the main message of the MM theorems; when reconsidering his work with Franco thirty years later, he stated (1988, p. 100):

> "the view that capital structure is literally irrelevant or that 'nothing matters' in corporate finance, though still sometimes attributed to us (and tracing perhaps to the very provocative way we made our point), is far from what we ever actually said about the real world applications of our theoretical propositions. Looking back now, perhaps we should have put more emphasis on the other, more upbeat side of the 'nothing matters' coin: showing what *doesn't* matter can also show, by implication, what *does*" (emphasis by the author).

To elucidate this point, consider the MM theorem about the irrelevance of capital structure. It states that the amount and structure of debt taken up by a company do not affect its value if: 1) there are no taxes; 2) bankruptcy does not entail any real liquidation costs for the company nor any reputation costs for its directors and 3) financial markets are perfect, that is, are competitive, frictionless and free of any informational asymmetry.

The theorem establishes that a company's value – the market value of its shares and debt – equals the present discounted value of the company's cash flow, gross of interest, where the discount rate is the required return for firms of the same 'risk class'. Hence, the firm's

value is determined solely by this discount rate and its cash flows, that is, by its assets, and it is wholly independent from the composition of the liabilities used to finance those assets. The theorem implies also that the average cost of capital is independent of the volume and structure of debt, and it equals the return required by investors for firms of the same 'risk class'. Even though debt may appear cheaper than equity, due to the absence of a risk premium, increasing leverage does not reduce the average cost of capital to the firm, because its effect would be precisely offset by the greater cost of equity capital. As a result, investment decisions can be totally decoupled from their financing: they should be guided only by the criterion of maximizing firm value, and the cost of capital to be used in rational investment decisions is its total cost, as measured by the required rate of return on fully equity-financed firms of the same 'risk class'.

Now, the *entire* development of corporate finance since 1958 – the publication date of the first article by Modigliani and Miller – can be seen and described essentially as the sequential (or simultaneous) relaxation of the three assumptions listed before.

The no-tax assumption was the *first* to be relaxed, at the hands of MM themselves, who recognized that the preferential treatment of debt by the US tax code implied that an optimal capital structure would require a larger leverage than that observed in reality. Much of the later work by the two authors – and many others – consisted in refining this basic point, and studying how it should be modified to take into account the differential taxation of interest income and capital gains at the personal level. In different ways, this analysis led to a considerable downward revision of the earlier MM conclusion about the huge value gains that US corporations could obtain by increasing their leverage.

Others went in a different direction to find an offsetting cost to the tax advantage of debt, and identified it in the costs of bankruptcy – thereby relaxing the *second* MM assumption. Increasing leverage would bring value increases in the form of tax benefits, but would also raise the probability of incurring the cost of bankruptcy. Under suitable assumptions, this could generate an interior optimum – a value-maximizing leverage that would equate the marginal benefit from tax saving with the marginal cost from the increased likelihood of bankruptcy. Many generations of MBA students have been exposed to this model, but academics have continued arguing whether the estimated

magnitude of bankruptcy costs could be reconciled with such an important role in capital structure decisions.

Finally, a truly tidal flow of advances in corporate finance occurred by relaxing the *third* MM assumption – that of frictionless markets. The most widely analyzed 'friction' was that arising from asymmetric information in financial markets, that is, adverse selection and/or moral hazard between external financiers and company managers. It is fair to say that in the last 25 years most of corporate finance has been an exploration of the consequences of introducing asymmetric information into the picture, both at the theoretical and at the empirical level.

The literature has shed light on the different *incentive properties* of the various financial instruments that firms can issue to finance their investment. For instance, in costly state verification models, standard debt was shown to be the optimal contract (Townsend 1979, Gale and Hellwig 1985). In the context of innovative firms backed by venture capital, several authors have shown that convertible debt and stage financing have desirable properties (Casamatta 2003, Cornelli and Yosha 2003, Schmidt 2003), while others have highlighted the need for (and documented the actual occurrence of) financial contracts with sophisticated covenants to allocate control and cash flow rights between venture capitalists and entrepreneurs in various contingencies (Kaplan and Stromberg 2003). In general, this literature explains why the allocation of cash flow and control rights, which would be irrelevant in the stylized MM world, is central to the incentive structure of real-world companies and thereby to their performance.

Apart from their incentive properties, capital structure decisions have been shown to be possible *conveyors of information*, to the extent that they can reveal the superior information of managers or entrepreneurs about the profitability of the firm's investment opportunities. For instance, in the model by Leland and Pyle (1977), the amount of equity retained by the entrepreneur can signal the profitability of the firm's investment – the credibility of the signal arising precisely from the forgone diversification. Similarly, in Myers and Majluf (1984) the issuance of equity is interpreted by the market as a bad signal, since owners with superior information tend to sell their shares when the market overvalues them. By the same token, the dividend payout decision can be far from irrelevant if dividends act as a credible signal of the company's profitability (see, for instance, Batthacharya 1979).

So also the second MM irrelevance proposition comes into question in a world of asymmetric information.

But these few examples do not do justice to what is by now an enormous literature. The models and their variations are so numerous that even well-read scholars often lose track of the overall picture. It is precisely to try and provide a unified view of this enormous and somewhat chaotic literature that a theorist of the calibre of Jean Tirole has recently taken to write a handbook of corporate finance entirely devoted to asymmetric information models (Tirole 2005). The size of the book's manuscript (640 pages) gives an idea of the magnitude of this literature. Equally revealing is the book's exclusive focus on information asymmetries, after a passing initial remark on the MM theorem and on the possible role of taxes in capital structure.

But after, this is probably the best tribute to MM. Recall again what Miller wrote (1988, p. 100): "[...] showing what *doesn't* matter can also show, by implication, what *does*". What we have been busy doing – in the past half century – has been precisely this: focusing on what *does matter* in corporate finance. No doubt, we have done it in a piecemeal and disorderly way, sometimes marked by duplication of efforts and wasteful detours, but tidiness is not a requirement of scientific progress. As we shall see, even the original proof of the MM theorems was far from tidy – still, they were true and highly valuable.

2. Methodological value of MM as 'arbitrage-based propositions'

When it was proposed for the first time, the MM leverage irrelevance proposition raised much controversy and attracted much criticism also for methodological reasons. Up until the mid-1950s, the study of finance was mostly confined to the description of methods and institutions of the financial system. The deductive and formal reasoning typical of economic theory was rare. It entered the field of finance precisely with the 1958 article by Modigliani and Miller and with the portfolio choice theory simultaneously developed by James Tobin, Harry Markowitz and William Sharpe (not surprisingly all Nobel prize winners). It was with these contributions that a coherent theory started to emerge capable of accounting both for the funding of investment choices by firms and for the allocation of saving by house-

holds – a theory based on the assumptions of rational behaviour by investors and of market equilibrium. Once these basic elements were all in place, the theory of finance could develop rapidly.

However, when Modigliani and Miller set out to prove their first proposition, they could not yet count on the well-developed equilibrium models of securities pricing that we find today in every finance textbook. This explains why they based their proof on a more fundamental and at the same time less demanding notion than that of competitive equilibrium: they went for an arbitrage argument.

In a way, this proof strategy was at least as important as the substantive result that they set out to prove, for two reasons. First, the notion of arbitrage is at the same time more compelling and more general than that of equilibrium – the absence of arbitrage does not require the economy to be in equilibrium, though a competitive equilibrium is invariably arbitrage-free. Second, this method became then standard to price redundant securities in finance: derivatives pricing is typically 'pricing by arbitrage'. Black and Scholes (1973) relied on MM-type arbitrage arguments to derive their celebrated option pricing formula and, as noted and elegantly shown by Miller (1988, p. 110) himself, "the familiar Put-Call Parity Theorem [...] is really nothing more than the MM Proposition I in only a mildly concealing disguise!".

The actual MM arbitrage proof was rather clumsy, and it involved the comparison between two firms whose cash flows had the same risk characteristics – or, to use the original wording, in the same "risk class". The argument went approximately as follows. Suppose that the MM leverage irrelevance proposition were not true, so that under the conditions listed before (no taxes, no bankruptcy costs, perfect markets and symmetric information) the value of a company is greater if it chooses a certain leverage – say, 50% – rather than another – say, 0. Let us then consider two companies within the same "risk class" but different capital structure. Company A chooses the 'better' leverage (50%), while company B refuses to take on any debt, and stays wholly equity-financed. Then, company A would be worth more than company B. But then investors could sell the shares of company A, buy the cheaper shares of company B and issue themselves enough debt so as to replicate 'synthetically' the supposedly optimal mix of the liabilities of company A. (Note that households can borrow at the same terms as companies, under the maintained assumption of perfect

capital markets.) These households would have replicated the capital structure of company A at a lower cost relative to the market value of that company, and therefore would have earned an arbitrage profit. Since this opportunity remains open until the value of company A exceeds that of company B, households would have a money machine at their disposal, which obviously cannot be consistent with equilibrium. For the equity and debt market to be in equilibrium, company A and company B must command the same market value, independently of their capital structure.

This illustrative argument is deceivingly simple compared to the proof in the original 1958 article by Modigliani and Miller. As Franco humorously put it in an interview (Barnett and Solow 2000, pp. 233-34),

> "The theorem [...] was proven very laboriously in about 30 pages. The reason for the laboriousness was in part because the theorem was so much against the grain of the teachings of corporate finance – the art and science of designing the 'optimal capital structure.' We were threatening to take the bread away, and so, we felt that we had to give a 'laborious' proof to persuade them. Unfortunately, the price was paid by generations of students that had to read the paper. I have met many MBA students that remember that paper as a torture, the most difficult reading in the course. It's too bad because, nowadays, the theorem seems to me so obvious that I wonder whether it deserves two Nobel Prizes".

The 'laboriousness' of the proof, however, had to do also with the fact that no-arbitrage arguments were still in their infancy in the theory of finance. Of course now we can do much better. We know that the absence of arbitrage implies a linear rule to determine the prices of all assets as functions of their payoffs. Armed with this conceptual apparatus, we do not need to go through the comparison of two firms of the same 'risk class'. It suffices to remark that 1) the total value of a firm is the sum of the value of its debt and equity; 2) the cash flow of the firm must go either to debt or to equity; 3) the linearity of the price rule implies that the sum of the values of debt and equity (the value of the firm) equals the value of the sum of its cash flow, irrespectively of how it is apportioned between debt and equity. The reasoning clearly applies not only to debt and equity, but to any other financial instrument used to finance the firm – derivatives,

convertible debt or any other security that the fantasy of a financial engineer can design.

Alternatively, we could abandon the light gear of the no-arbitrage argument, and go for the heavy artillery of a full-fledged general equilibrium model to make the same points, as for instance was done by Stiglitz (1974), among others. Also with this strategy the proof can dispense with the assumption that there have to be at least two firms in the 'same risk class'.

However, if one is willing to sacrifice the generality of the no-arbitrage argument and to prove the MM theorem within a particular model of asset pricing equilibrium, a new and potentially intriguing light is cast on the 'risk class' element, as noted by Ross (1988). For instance, if the asset pricing model used does not price idiosyncratic risk – but only covariance risk – one could redefine the 'risk class' characterization of the two firms in the MM original proof as the requirement that their cash flows have identical covariance risk but potentially different idiosyncratic risk. But again, we can do this with the hindsight of asset pricing models that Modigliani and Miller could not call upon. They introduced the vague notion of 'risk class' in their proof precisely to fill this theoretical void.

3. Concluding remarks

In no point of the previous discussion did I mention empirical evidence on the MM propositions. It is true that both Franco and his co-author spent much effort and many pages to compare the predictions of MM-cum-taxes with the US evidence, and took great pains to understand whether the inconsistency between the two arose from mistakes in the formulation of the theory or rather incompetence by company managers.

My omission was deliberate, however, because I view this as a less lasting aspect of the legacy of Modigliani and Miller. Now we know that the existence of taxes is only one of the several ways in which reality departs from the MM assumptions, and that proper empirical analysis of capital structure decisions must be far more inclusive – taking into account also bankruptcy costs and informational issues. This, together with the difficulty of identifying truly

exogenous variables (a pervasive problem in applied corporate finance), explains why it is so hard to do good empirical work in this area.

But even in this respect, the empirical efforts of Modigliani and Miller contain two more memorable lessons for all of us. The first lesson comes from their passion to relate the theory to observed phenomena, and to be ready to question and reformulate one's own theory when it is inexorably challenged by the facts. This is witnessed by the series of successive reformulations and corrections that the two authors made to MM-cum-taxes model, first together and then separately.

The second lesson is their 'lay' attitude *vis-à-vis* even the assumption traditionally most 'sacred' to economists – that is, the rationality of economic agents. The account by Miller (1988) witnesses that Modigliani and Miller entertained seriously the possibility that the shortfall of US corporate leverage relative to the prediction of MM-cum-taxes was due to the irrationality (or incompetence) of managers. In the same spirit, in his work with Cohn on the effect of inflation on stock prices (1979), Franco was open to the idea that the gulf between theoretical predictions and observed behaviour might arise from irrational (or incompetent) choices by analysts and investors. As he later put it (1979, p. 157), he had "become a bit disenchanted with the indiscriminate use of superrationality as the foundation for models of economic behavior". It is remarkable that these words, as well as the conclusions reached by Modigliani and Cohn (1979), were uttered in the midst of the 'rational expectations revolution' in macroeconomics and at a time when asset pricing researchers held the rationality of investors as a universal article of faith. The time of books on the 'irrational exuberance' of investors and on behavioural finance were still far away, yet Franco had no hesitation to cast doubt on this assumption. This is not to say that Modigliani and Miller were in any way precursors of 'behavioural finance' – indeed I guess that Franco would have been very sceptical of much of what now goes under this label. But it shows their intellectual independence from the 'common wisdom' of their time.

To me, these are at least as important teachings as the substantive insight by Modigliani and Miller or their arbitrage-based proofs. The MM theorems are a cornerstone also because they are an enlightening example of a research method that can still inspire scholars for many years to come.

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