False Models as Explanatory Engines

Frank Hindriks (f.a.hindriks@rug.nl)

Forthcoming in *Philosophy of the Social Sciences*

Abstract. Many models in economics are very unrealistic. At the same time, economists put a lot of effort in making their models more realistic. I argue that in many cases, including the Modigliani-Miller irrelevance theorem investigated in this paper, the purpose of this process of concretization is explanatory. When evaluated in combination with their assumptions, a highly unrealistic model may well be true. The purpose of relaxing an unrealistic assumption, then, need not be to move from a false model to a true one. Instead, it may be providing an explanation of some phenomenon by invoking the factor that figures in the assumption. This idea is developed in terms of the contrastive account of explanation. It is argued that economists use highly unrealistic assumptions in order to determine a contrast that is worth explaining. The process of concretization also motivates new explanatory questions. A high degree of explanatory power, then, may well be due to a high number of unrealistic assumptions. Thus, highly unrealistic models can be powerful explanatory engines.

False Models as Explanatory Engines*

Many models in economics are very unrealistic. At the same time, economists put a lot of effort in making their models more realistic. Why would they do this? The obvious answer to this question seems to be that they want their models to be true. The move from an unrealistic model to a more realistic model would then be a move from a model that is false to a model that might be (at least approximately) true (Wimsatt 1987, Nowak 1989). There are indeed parts of economics for which this answer is on the right track (Hindriks 2005). However, I shall argue that this answer fails to make sense of significant parts of economics in which the move towards more realistic models plays a crucial role. A theory at the heart of which lies a very unrealistic model may well be true. I shall argue that it is often misguided to evaluate a model in isolation. Many models should be evaluated in combination with their assumptions instead. Consider Galileo's theory of falling bodies according to which all bodies accelerate at the same rate. One of its predictions – that all bodies fall at the same speed – is clearly false. Furthermore, the assumption that bodies fall in a vacuum is also false. However, the claim that, if there were hardly any air-resistance, all bodies would fall at approximately the same speed, is true. The same line of reasoning can be used to argue that some economic theories that appear to be obviously false may well be true.

* I am grateful for comments from Maarten Franssen, Theo Kuipers, Jaakko Kuorikoski, Aki Lehtinen, Uskali Mäki, Caterina Marchionni, Bauke Visser, Petri Ylikoski, Sjoerd Zwart, and two anonymous referees, as well as from the (other) participants of the Urrutia Elejalde Summer School 'New Philosophy of the Social Sciences' (San Sebastian, July 2006) and the Philosophy of Science seminar of the Department of Philosophy at the University of Helsinki (January 2007).

The case that is investigated below is the Modigliani-Miller irrelevance theorem concerning the capital structure of a firm. According to this theorem, the way in which a firm is financed is irrelevant to its value. This theorem is probably false insofar as the actual world is concerned. But relative to what has become known as a Modigliani-Miller world, a world in which financial markets operate perfectly, it may well be true. In other words, the Modigliani-Miller theory – the combination of the model that generates the theorem and its assumptions – may well be true even though the Modigliani-Miller theorem itself is false. Thus, the case – that is used here as a representative of a wide range of partial equilibrium models – serves to show how economists put falsehoods 'to the service of truth' and supports Uskali Mäki's claim that the truth of a model 'is not reducible to the truth of its assumptions nor to the truth of its predictions' (2006, 16). The question arises, however, why economists bother to produce theorems that do not apply to the actual world but only to worlds that are in some sense ideal. They do so, I shall argue, because they want to provide explanations of real world phenomena, and because theories that rely on very unrealistic models can have a large amount of explanatory power.

I shall take as my point of departure the framework that Caterina Marchionni (2006) has provided for assessing the explanatory power of unrealistic models. Relying on the contrastive account of explanation, I shall argue that unrealistic assumptions serve to delineate the contrast relative to which a fact is explained. The contrast that is suggested by the Modigliani-Miller theorem is the irrelevance of capital structure to the value of a firm. The explanatory question that fits this contrast is this: 'Why is the capital structure of a firm relevant to its value rather than being irrelevant?' So unrealistic assumptions serve an explanatory purpose by suggesting explanatory questions. As I shall illustrate below, answers to this question suggested new explanatory questions. In this way, the theorem has generated a whole new line of research. However, unrealistic assumptions do not only play a role in

determining the contrast and thereby in suggesting explanatory questions, they are also crucial to providing answers to those questions. Relative to the purpose of explanation, then, it can be a virtue of a model that it has many unrealistic assumptions. Highly unrealistic models may have a large amount of explanatory power.

This defense of unrealistic assumptions is very similar to that provided by Milton Friedman (1953), the main difference being that he was concerned with prediction, whereas I focus on explanation. Friedman argued that we should not fault a theory for its unrealistic assumptions, but only for faulty predictions. I argue instead that <u>we should not fault a theory for its unrealistic assumptions if it has a lot of explanatory power</u>. This claim is presented more fully in section 2, which also introduces the contrastive account of explanation. Section 3 discusses the Modigliani-Miller theorem, as well as some of the research that took this theorem as its point of departure. We shall see that the main theses defended in this paper are corroborated by claims that financial economists have made over the years. All this culminates in the thesis that false theorems or models can be powerful explanatory engines.

1. Unrealistic Assumptions, Concretization, and Confirmation

Before turning to explanation, I want to touch briefly on another purpose that might be served by making a model more realistic: confirmation. We shall see that a model need not be very realistic in order to serve the purpose of confirmation. First, however, we need to know what it might mean to say that one model is more realistic than another one. The only sense of 'unrealistic' that I shall be concerned with is that of being false.[1] The assumptions discussed below are regarded as unrealistic, because they are believed to depart significantly from the

[1] Sometimes an assumption is regarded as unrealistic for other reasons, for instance because it is implausible or practically useless, or because it refers to unobservables (Mäki 1992, 1998).

truth, or because there is widespread doubt as to whether the existing evidence provides sufficient reason for believing they are (approximately) true. Such assumptions are imposed either for the purpose of abstraction or for that of idealization (see below). A model can be made more realistic by undoing an abstraction or idealization. Economists talk about relaxing assumptions in this context. I shall use the term 'concretization' for the process of making a model more realistic by relaxing some of its unrealistic assumptions (Nowak 1989, Carwright 1989, Kuipers 2000).[2]

Scientists rely on the processes of abstraction and idealization in order to construct models and theories. Abstraction is a matter of omitting or excluding certain things, or, to use Nancy Cartwright's terminology, of subtracting features from concrete objects, and deleting the concrete circumstances in which causes operate (1989, 187). Many scientific representations do not, for instance, mention the colors of the objects they are meant to represent. And when economists represent firms as production functions, they abstract from the particular kinds of machines firms might use in the production process. Idealization, on the other hand, is a matter of exaggerating things, and is achieved by changing particular features or properties. Rather than deleting factors, in the case of idealization 'we replace them by others which are easier to think about, or with which it is easier to calculate' (ibid.). An idealization is usually, if not always, achieved by assuming that a variable takes on (one of) its limit-value(s). Frictionless planes and mass points are famous examples. In economics, one might think of towns being represented as points in geographical economics.

An important purpose of concretization is confirmation. It is often thought that in order to usefully test the validity of a model, it is necessary to formulate a version that is (thought to be) realistic in the sense that concretization should be carried out to the extent

[2] Other terms used for this process are 'factualization' (Krajewski, 1977) and 'de-isolation' (Mäki 1992).

required for achieving a fit between the model and the test situation. Given that experiments play a minor role in economics, this should be taken to mean that basically all causal factors that actually have a significant effect on the phenomenon under investigated should be included in the model. A problem with this idea is that the process of concretization is rarely carried out to such an extent (Frigg and Hartmann 2006). Ronald Laymon (1985) has argued, confirmation can be achieved without doing so. He proposes the following theses about confirmation and disconfirmation: 'A scientific theory is confirmed (or receives confirmation) if it can be shown that using more realistic idealizations will lead to more accurate predictions. A scientific theory is disconfirmed if it can be shown that using more realistic idealizations will not lead to more accurate predictions.' (Ibid., 155) Nothing seems to count against replacing the phrase 'using more realistic idealizations' by 'concretizing it' and thereby accommodating abstraction as well. The important point to see is that full realism of assumptions is not required for the purpose of confirmation (the idea of full realism makes little sense in any case). If Laymon is right, the following weaker condition suffices for this purpose: a model should survive concretization (mutatis mutandis; see note 10 for a discussion of the relation between explanation and confirmation).[3]

Concretization also plays an important role with respect to the dynamics of theory development. Ernan McMullin makes this point in relation to laws:

---

[3] It appears that economists rely on a more stringent requirement when it comes to measurement. In a case study concerning the measurement of economic performance indicators (Hindriks 2005), I observe that economists try to relax unrealistic assumptions if this is or when it becomes feasible (i.e. when it becomes possible or if the benefits of doing so are not outweighed by new costs incurred elsewhere). I use the term 'tractability assumption' for assumptions that are imposed because it is not feasible to construct the ideal model (see also Hindriks 2006).

> Theoretical laws derived from the model give an approximate fit with empirical laws reporting on observation. It is precisely this lack of perfect fit that sets in motion the processes of self-correction and imaginative extension described above. If a model is a good one, these processes are not ad hoc; they are suggested by the model itself. (1985, 264)

The idea is that in order to confirm a theory one has to concretize its basic model and that good models come with suggestions for appropriate concretizations. Choosing the right kinds of idealizations (and abstractions), then, is of vital importance for the quality of a theory, as well as for the creative process an unrealistic model can instigate. McMullin is mostly concerned with confirmation. Below we shall see that such a dynamics of self-correction also indicates that the theory has a large amount of explanatory power.

2. Unrealistic Assumptions, True Theories, and Explanatory Power

2.1 False Models and True Theories

The claim to be defended in this section is that a theory can be true even though its model(s) is (are) false. Before turning to the argument I should explain what this is supposed to mean. According to the most widely accepted conception of theories, the semantic view, a theory is a set or family of models combined with a theoretical hypothesis about the relation between the model and reality (Van Fraassen 1980, Giere 1988). On this conception only theoretical hypotheses can be false, not models. Giere (1988), for instance, claims that models are definitions, and that they are trivially true because of this. Nevertheless, the hypothesis that the way a model represents certain phenomena corresponds to the way things are in the real world can be false. And whether a theory is true depends on whether its theoretical hypothesis is true. Given this approach to models and theories, talking about false models is

inappropriate. As using this manner of speech makes it much easier to express the claims presented below, I shall rely on it nevertheless. Here, then, is my terminological proposal. Whenever models are said to be true or false in this paper, this should be read as shorthand for the following claim: the hypothesis that the model representation corresponds to reality is false.

So, how can a theory be true even though its only model is false? As was suggested in the introduction, the claim the truth of which should be evaluated in the case of Galileo's theory is this: if there were hardly any air-resistance, all bodies would fall at approximately the same speed. This claim is recognized as true until this very day, even though the model implication is false in the world as we know it (i.e. the world outside of laboratories). The idea, then, is that at least in some cases we should evaluate a model in combination with its assumptions. Doing so can reveal that a theory can be made true by a false model. This suggestion is not new (although many reject it because they do not like to invoke counterfactuals in this context; see Giere 1988 and Hausman 1992). It is worth elaborating on this nevertheless in order to clarify how exactly model implications and assumptions should be combined in order to get the desired result.

The way in which I combined the vacuum assumption with the model implication or the prediction of the model can be used as a template for other theories as well. The thing to do is to formulate a counterfactual in which the assumptions figure in the antecedent, and the model implication in the consequent. In effect, the unrealistic assumptions serve to select the world in which the model implication holds. The fact that the assumptions are unrealistic implies that this world is not the actual one, but another possible world (or set of possible worlds). The general form of the proposal, then, is to formulate a counterfactual of the following form: if the world were to fit the assumptions of the theory, the model implication would hold. This counterfactual may well be true even if both the assumptions and the model

prediction considered in isolation are false (cf. Niiniluoto 1989).[4] All this reveals that concretization is not needed in order to arrive at a (potentially) true claim.[5] Thus, it turns out to be mistaken to think that realism of assumptions is necessary for a model to be true, and it can be misguiding to engage in the process of relaxing unrealistic assumptions in order to move from a false model to a true model. Instead, the proposed perspective suggests that there might be more truth in the theory and its original model than meets the eye.

2.2 Contrastive Questions and Explanatory Power

Suppose that a model is true, or regarded as true when evaluated in combination with its assumptions in the way just proposed. Why would economists bother to propose new versions of the model by relaxing some of its assumptions? I shall now argue that the quest for explanations offers a good reason for engaging in this activity. Unrealistic assumptions often provide opportunities for explanation. In fact, we shall see that the extent to which a model is unrealistic can, under certain conditions, be seen as an indication of its explanatory power. Thus, pace Wimsatt (1987), false models can have productive uses even if their predictions

---

[4] Relative to the semantic conception of theories, the proposal amounts to regarding the counterfactual as the theoretical hypothesis of the theory. Note that, given a standard Lewis-Stalnaker semantics, a counterfactual is true exactly if the consequent is true in the (set of) world(s) closest to the actual world in which the antecedent is true.

[5] Mäki has argued that '[t]he truth of a model is not reducible to the truth of its assumptions nor to the truth of its predictions' (2006, 16). Given my story about how false models can make a theory true, I am committed to this as well. As Mäki has indicated in personal conversation, however, he is reluctant to appeal to counterfactuals in the way I do. So, it would be too quick to say that my argument supports his view.

are incorrect. In this section, I shall present the theoretical machinery needed for formulating these claims in a precise way, in particular the contrastive approach to explanation and three conceptions of explanatory power. The proposition that the way economists explain corroborates the proposed view shall be defended in section 3.2.

The explanatory role of assumptions can best be understood in terms of the contrastive approach to explanation. According to this approach, explanations are answers to why-questions. They are not, however, answers to simple why-questions of the form 'Why *p*?' Instead, they are answers to complex or contrastive questions of the form 'Why *p* rather than *q*?' In such questions, *p* is a proposition stating a fact, and *q* is a proposition that specifies the contrast, benchmark, or 'foil' relative to which an explanation of *p* is sought. Contrastive questions make it apparent that we only explain aspects of events rather than events simpiciter. This allows us to single out particular causal factors at the expense of others (Lipton 1990, 249-50).

Garfinkel illustrates the need for contrastive explanation in terms of a story about Willie Sutton, the bank robber: 'When Willie Sutton was in prison, a priest who was trying to reform him asked him why he robbed banks. "Well," Sutton replied, "that's where the money is."' (1981, 21). Obviously, this is not the answer the priest was looking for. The reason for the miscommunication between the priest and Willie Sutton is that they relied implicitly on different contrasts to the fact mentioned in the simple question: 'For the priest, what stands in need of explanation is the decision to rob at all. He does not really care what. But for Sutton, that is the whole question. What is problematic is the choice of what to rob.' (Ibid.) So, for the priest the complete explanatory question is 'Why did Willie Sutton rob the bank rather than not robbing anything at all?' whereas the question on Sutton's mind is 'Why rob the bank rather than robbing something else?'

The explanatory power of a theory is usually taken to depend on its explanatory scope and its explanatory depth. The explanatory scope of a theory is a matter of the number of phenomena it can explain. This idea has been developed in several ways. Explanatory scope can be measured in terms of the number of laws that can be derived from a more basic law (Friedman 1974), or in terms of the number of phenomena that can be explained by using one and the same argumentative structure (Kitcher 1981). In the context of economics, Mäki has proposed a conception of explanatory scope on which it is a matter of the number of 'apparently independent and diverse phenomena' that can be re-described 'as manifestations (outcomes, phases, forms, aspects) of one and the same small number of entities, powers, and processes' (2001, 498). The depth of an explanation depends on the amount of detail it provides about the causal process that explains the phenomenon at issue (see, for instance, Marchionni 2006).[6]

It appears that the notion of explanatory depth does not really require adaptation in order for it to be suitable for use in relation to the contrastive approach to explanation. It is just a matter of the amount of detail or articulation provided about the underlying causal processes and mechanisms in an answer to a contrastive why-question. In any case, the explanations to be discussed below are fairly shallow, so there is no need to pursue this issue any further. Explanatory scope can in this context be conceived of in terms of the number of contrastive questions a theory can answer.[7]

---

[6] Many believe that explanatory scope and depth are 'competing' explanatory virtues (see, for instance, Sober 1999). In her contribution to this issue, Marchionni (2007) argues that explanations can have a large scope and be deep at the same time.

[7] Marchionni (2006) proposes a typology that is more fine-grained than this. She introduces the term 'explanatory variety' for the number of contrastive questions answered <u>in relation to a given phenomenon</u>, and reserves the term 'explanatory scope' for the number of phenomena

Another dimension of explanatory power, one that seems to have gone unnoticed thus far, is what I shall call 'explanatory breadth'. Often a single phenomenon can be explained in terms of one of several factors. All those factors contribute to the phenomenon to be explained, but each of them suffices for explaining it. Each of them is causally relevant, while none of them is necessary for the phenomenon to occur.[8] In terms of the contrastive account of explanation, explanatory breadth is a matter of the number of answers a theory can provide to a particular explanatory question. Such answers need not be deep in the sense of being articulated in detail. In section 3.3, I argue that the Modigliani-Miller theory has a fair amount of explanatory breadth, as it can be used for providing at least three answers to one and the same contrastive question. I also argue there that the theory has a significant amount of explanatory scope, because it can be used for answering at least four different contrastive questions.

2.3 Unrealism of Assumptions as an Explanatory Virtue

---

explained (irrespective of the number of questions that are answered in relation to those phenomena).

[8] As Caterina Marchionni pointed out to me, the term 'breadth' is sometimes used instead of scope (see, for instance, Sober 1999). As I use the term, explanatory breadth is an explanatory virtue that is distinct from scope. Explanatory breadth pertains to the breadth of the explanatory basis, i.e. the number of explanantia. Note that often several factors need to be in place in order for a phenomenon to occur. In such cases, explanatory breadth is to be achieved by identifying several <u>sets</u> of factors each of which is sufficient for the phenomenon to occur (an INUS-condition represents a member of such a set).

How can unrealistic assumptions be used for explaining contrasts, or answering contrastive why-questions? Unrealistic assumptions can be used for determining the foil, *q*, the contrast relative to which a certain fact is to be explained (see Marchionni 2006). Consider Galileo's theory of falling bodies once more. One of its predictions presented earlier is that all bodies fall at the same speed. This is not what we actually observe (outside of laboratories). As a consequence, it can be used as a contrast relative to which a phenomenon that we do observe can be explained. The explanatory question would then be: Why do bodies with different weights fall at different speeds rather than at the same speed? The way in which such a question can be answered is by relaxing one of the unrealistic assumptions, in this case the vacuum assumption. So, relaxing an unrealistic assumption can be a means to explanation. It facilitates explanation if the new set of assumptions generates a result the original model did not contain that is reflected in the world outside of the model. If the new model implies *q*, and the old model entails *p*, the factor that figures in the unrealistic assumption that is relaxed can be used for answering the question 'Why *p* rather than *q*?' As the explanation is constructed by relaxing an unrealistic assumption, I shall call this way of explaining facts <u>explanation by concretization</u>.[9]

[9] Marchionni (2006, 428-29) argues that unrealistic assumptions sometimes serve to determine a causal background that the fact and the foil have in common. The explanatory factor(s) is (are) present only in the model that implies the fact. One reason why I say instead that the unrealistic assumptions determine the foil is that this enables us to see that the explanatory factor(s) can be brought into the model by relaxing an unrealistic assumption. So, unrealistic assumptions play both a passive role (in determining the background), and an active role (by revealing the explanatory factor(s)). Another reason is that this facilitates the discussion of why certain contrasts are more fruitful than others in section 3.3.

Given this perspective on explanation, the explanatory power of a theory turns out to be intimately related to the extent of the unrealism of its basic model. The explanatory scope of a theory depends on the number of questions that can be answered by relaxing some of its assumptions. The explanatory breadth of a theory depends on the number of assumptions that can explain a particular contrast. Whether or not a theory has a large degree of explanatory power in either or both of these senses, then, depends on the choice of unrealistic assumptions. If they are ill chosen, only a few relevant questions can be answered by relaxing some of them. If they are well chosen – something that is of course hard to determine in advance – many questions can be answered in this way. As we shall see in section 3.3, it may also happen that providing an answer to one explanatory question prompts new interesting questions that can be answered by relaxing several assumptions at the same time. In this way, unrealistic assumptions can also have significant heuristic value. Relative to the purpose of explanation, then, it can be a virtue of a model that it has many unrealistic assumptions, the reason being that it might have a large amount of explanatory power because of those assumptions.

The upshot is that an economic theory that has a large amount of explanatory power usually owes at least some of that power to the high number of unrealistic assumptions on which its basic model is built. Recall that Friedman argued that we should not fault a theory for its unrealistic assumptions, but only for faulty predictions. This defense of unrealistic assumptions is often not available, because the predictions of unrealistic models are frequently inaccurate. The preceding line of argument, however, suggests a variant of this claim that may hold for such models and the theories to which they belong: <u>we should not fault a theory for its unrealistic assumptions if it has a lot of explanatory power</u>. As we shall see in section 3.2, economists have taken Friedman's defense of unrealistic assumptions to heart often without paying a lot of attention to the presumed link to prediction. The way in

which their theories develop, however, is often compatible with the justification of unrealistic assumptions in terms of explanation just presented. All this means that Friedman's (1953, 14) dictum 'the more significant a theory, the more unrealistic the assumptions' might be true for other reasons than he had in mind.[10]

3. The Explanatory Power of the Modigliani-Miller Propositions

3.1 The Modigliani-Miller Propositions

Franco Modigliani and Merton Miller (1958) rely on the Fisherian view of the firm that abstracts from production technology, takes firms to be black boxes, and considers only the cash flows a firm generates.[11] The question that Modigliani and Miller are concerned with is

[10] In order to have confidence in an explanation, the theory used for providing it needs to have received confirmation. And confirmation requires prediction. So, an explanation can be regarded as adequate only to the extent that the theory on which it relies has turned out to be empirically successful. As indicated in section 1, this can be taken to require nothing more than that concretization improves the empirical accuracy of a theory. This implies that it is not necessary that each explanation be confirmed separately. Having said that, I should note that more work needs to be done concerning confirmation in relation to abstract and idealized models that cannot or can hardly be tested in laboratories. Laymon's criterion will appear to be too weak to some, while it can also be argued that it is too strong: it is not inconceivable that a more realistic model performs worse than a less realistic one even if the theory to which these models belong is true.

[11] Those who are familiar with the Modigliani-Miller irrelevance theorem may wish to skip to section 3.2.

whether, and, if so, how, capital structure influences the value of a firm. Firms put a lot of effort in determining the extent to which they depend on debt versus equity. Furthermore, changes in capital structure often have great repercussions for the value of a firm on the stock market. These observations suggest that capital structure is of great significance to the value of a firm. Modigliani and Miller argue against this and maintain that the capital structure is in fact irrelevant to the value of a firm. This proposition is what has become known as the Modigliani-Miller irrelevance theorem.

At the time they wrote their article, the common opinion was that the value of a firm that is financed by equity only could be increased by introducing debt in its financial structure. The motivation for this view was that debt is cheaper than equity, because the rate of return on bonds is relatively low as they hardly carry any risk, and that relying on debt thereby reduces the cost of capital. The idea was that, as it reduces cost, the introduction of debt increases the value of a firm. Against this background, Modigliani and Miller put forward the following two propositions: (I) The financial structure of a firm is irrelevant to its value (1958); (II) The dividend policy of a firm is irrelevant to the value of a firm (1961). The first proposition, to which I also refer as 'the irrelevance theorem', can also be formulated in terms of the average cost of capital and the cost of equity: the average cost of capital to any firm is independent of its capital structure. It is supported by a proposition about the cost of equity, which is said to be a linear function of the debt to equity ratio. These propositions are the model implications that Modigliani and Miller have derived. Let me elaborate on the first proposition and its companion.

According to proposition I, the value of an unlevered firm is equal to that of another firm that is identical to it in all relevant respects except that it is partially financed by debt. It is equal to the expected returns discounted at the rate of return for uncertain streams (the return on equity). Using V_U and V_L for the value of an unlevered and a levered firm

respectively, $\overline{X}$ for expected returns, and ρ for the rate of return on sure streams (the interest payments on debt), proposition I can be expressed as follows:

(6) $$V_U = V_L = \frac{\overline{X}}{\rho}.$$

Modigliani and Miller support this theorem by an arbitrage argument. They argue that if levered and unlevered firms were to differ in value without a concomitant difference between their cash flows, investors could make a profit either by selling their share of the unlevered firm and buying a part of the levered firm or vice versa. And such transactions would serve to restore the equality between the values of the firms, because the investors would influence the values of the shares of the firms at issue by exploiting the arbitrage opportunities. Modigliani and Miller (1958, 271n13) call this 'the arbitrage mechanism'.

Modigliani and Miller consider both the case in which the value of the levered firm is larger than that of the unlevered firm and the case in which this inequality is reversed. I shall limit my discussion of the arbitrage proof to the former (ibid., 269-70). Consider two firms that have the same expected return, $\overline{X}$.[12] Suppose that firm 1 is financed entirely by common stock, while firm 2 is financed partially by debt. The value of the levered firm, V_2 is larger than that of the unlevered one, V_1. Now, a particular investor holds s_2 dollars' worth of the shares of company 2, representing a fraction α of the income available for the stockholders of company 2: the total return X_2 less the interest charge rD_2, r being the rate of return on debt and D being the market value of the debt. As the expected return of the two firms is the same, we can use X as a symbol instead of both X_1 and X_2. Hence, the return on the initial portfolio, Y_2, can be written as:

[12] Modigliani and Miller (1958, 266-67) assumed that the firms belong to the same risk class. This assumption was later shown to be redundant, and I will abstract from it for ease of exposition.

(7) $Y_2 = \alpha(X - rD_2)$.

Now suppose that the investor sells his αS_2 worth of shares of firm 2, and uses it for buying a part of firm 1. She could buy an amount $s_1 = \alpha(S_2 + D_2)$ of the shares of firm 1 by utilizing the amount αS_2 realized from the sale of her initial holding and borrowing an additional amount αD_2 on her own credit (the new holdings in firm 1 could serve as a collateral). She would thus secure for herself a fraction $s_1/S_1 = \alpha(S_2 + D_2)/S_1$ of the shares and earnings of firm 1. As she has to pay interest payments on her personal debt αD_2, the return from the new portfolio, Y_1, is given by:

(8) $$Y_1 = \frac{\alpha(S_2 + D_2)}{S_1} X - r\alpha D_2.$$

As this can be rewritten as $Y_1 = \frac{\alpha(S_2 + D_2)}{S_1} X - r\alpha D_2 = \alpha \frac{V_2}{V_1} X - r\alpha D_2$, these equations imply that $Y_1 > Y_2$ as long as $V_2 > V_1$. So, it is profitable for the investor to sell her shares of firm 2 and acquire shares of firm 1. The former transaction will lower the market value of the shares of firm 2, S_2, while the latter transaction will raise the value of the shares of firm 1, S_1. As the value of a firm is equal to the sum of its equity and its debt, $V \equiv S + D$, these transactions lower V_2 and increase V_1 respectively. In equilibrium, Y_1 should equal Y_2, which implies: $\alpha(X - rD_2) = \alpha \frac{V_2}{V_1}(X - rD_2)$. This holds exactly if $V_1 = V_2$. Q.E.D.

According to the supporting proposition, the expected yield of a share of stock is an increasing function of the debt to equity ratio:

(9) $$i = \rho + (\rho - r) D/S.$$

The underlying idea is that the higher the amount of debt, the larger the risk for equity holders, because equity holders get what is left after interest has been paid. As a consequence, the second term of the equation can be seen as a risk-premium that is a function of the spread

between ρ and r, and the debt to equity ratio. This companion proposition can be derived from the main one in a straightforward way. By definition, the yield of a share of common stock is given by $i \equiv \frac{\overline{X} - rD}{S}$. Proposition I implies that $\overline{X} = \rho(S + D)$. Equation 9 follows from substituting the second equality in the first and simplifying.

The MM propositions, as I shall call them from this point onwards, are often motivated in terms of a cake-cutting metaphor. Just as the size of a cake is unaffected by the sizes and shapes of the pieces in which it is cut, so the value of a firm is independent of the way in which its earnings are divided between the holders of debt and equity. Miller describes the theorems informally as follows: '[T]he MM propositions assert only that the sum of the values of all the claims is independent of the number and the shapes of the separate partitions.' (1988, 111) Jean Tirole relates the metaphor explicitly to the thesis that supports proposition I as well, as is apparent from the following passage: 'In other words, decisions concerning the financial structure affect only how the "corporate pie" (the statistical distribution of income that the firm generates) is shared, but has no effect on the total size of the pie. Thus, an increase in debt or a dividend distribution dilutes the debtholders' claim and benefits the shareholders, but the latter's gain exactly offsets the former's loss.' (2006, 77-78; see also Milgrom and Roberts 1992, 459)

The MM propositions presuppose what has become known as an MM world. There are no taxes and no bankruptcy costs in an MM world, and it is possible for individual investors to borrow at the same rate as firms. In addition to this, it is assumed that markets operate efficiently, and firms only issue two types of claims: (riskless) debt and (risky) equity. Furthermore, cash flows remain constant over time. Implicitly, Modigliani and Miller also assumed that information is symmetric, that there are no agency costs and that contracts can also be enforced. Later on it was established that the theorems also hold in an Arrow-Debreu environment of complete markets (Tirole 2006, 1). An assumption such as that debt is

completely without risk could be dropped, and the remaining assumptions are usually summarized in terms of the claim that capital markets are complete and perfect.

There is no doubt that, relative to the prevailing opinion at the time, the MM propositions present an advance. They reveal that the idea that increasing the amount of debt relative to equity can increase the value of a firm is not as compelling as it might seem otherwise, and it provides for a diagnosis as to why it might be mistaken. Under certain conditions the effect of an increase in debt will be exactly offset by an increase in the cost of equity. As a historical fact, the propositions have indeed served to debunk the existing view, which is now known as the naïve view.

3.2 A True Theory Based on False Propositions

If the purpose of relaxing the unrealistic assumptions mentioned had been to replace false theorems by true ones, one would expect economists to have rejected the MM propositions. This, however, is not what has happened. In fact, the theorems are still heralded as cornerstones of corporate finance (Tirole 2006). And this is the case in spite of the fact that some take the propositions to have been refuted, while others regard their empirical validity as still an open question (Brealey and Myers 2000). Ross expresses his support for the theorems in especially strong terms: 'We are now so accustomed to the acceptance of the new paradigm that the older view that capital structure did matter has about it the flavour of phlogiston. Not only does it seem wrong, it is difficult to believe that sensible folk could have held such beliefs.' (1988, 127) In a similar vein, Modigliani (1988, 150) observes that now proposition I 'seems almost trivial', whereas at the time at which they introduced it academics and practitioners regarded it as 'plainly preposterous'.

At the same time, however, Miller has given up hope that the theorems can be empirically confirmed directly: 'Our hopes of settling the empirical issues by [empirical estimates of how closely real world market values approached those predicted by our model], however, have largely been disappointed.' (1988, 103) And, as we will see shortly, many explanations as to why capital structure does matter after all have been offered and accepted since Modigliani and Miller put forward their theorems. So, on the one hand the propositions are firmly established within economics, and on the other hand often the same economists, including for instance Miller, accept claims that contradict them. How can we make sense of this?

My proposal is that in order to understand their continuing importance we should evaluate the MM theorems not in isolation but in combination with their assumptions. The fact that the model implications do not obtain in the actual world – we do not observe the empirical regularities the propositions entail – has not stopped economists from regarding them as valuable. One reason is that they served to show that the view concerning the role of capital structure in relation to the value of a firm that prevailed in the fifties of the previous century could not just be taken for granted. More importantly, however, the propositions might be part of (approximately) true counterfactuals in which they figure as the consequents and that have the assumptions as their antecedents. The idea is that the propositions as formulated above need not be true, but that what matters is the truth of the claim 'If capital markets were complete and operated perfectly, the propositions would be true.' As I shall now argue, this reading derives ample support from what has happened since the theorems were proposed generally, and from Miller's (1988) interpretation of this history in particular.[13]

[13] This is not to deny that many economists have at least been tempted to simply regard the propositions as empirical claims that could be confirmed or disconfirmed as such. Even Modigliani and Miller were attracted to this idea initially, as is suggested by the empirical

Consider a comment Miller makes on a consideration that is in tension with the MM theorems:

> The dividend invariance proposition … highlights still another way in which the corporate form of organization, and especially the separation it permits between ownership and management, can have effects that at first sight at least seem to contradict the MM value-invariance predictions. Recall that the dividend invariance proposition takes the firm's investment decision as given, which is just a strong way of saying that the level of investment, whatever it might be, is set by management independently of the dividend. Without imposing such an "other-things-equal" condition, … the MM invariance proposition may seem to be failing, <u>but it is really not being put to the test</u>. (Ibid., 105-06; emphasis added)

This passage can be taken to suggest that we should not interpret the theorems as straightforward empirical claims. In another passage, which is concerned with data that apparently contradict MM proposition II, Miller is more explicit as to how we should interpret the theorem:

> But, as we suggested in the 1961 dividend paper, these price reactions to dividend announcements were not really refutations. They were better seen as failures of one of the key assumptions of both the leverage and dividend models, namely, that all capital market participants, inside managers and outside investors alike, have the same information about the firm's cash flows. (Ibid., 105)

This passage suggests that it is misguiding to think that the theorems can be tested empirically by directly comparing the real world to the regularities the propositions entail.

In fact, at some point Miller explicitly claims that the theorems should not be interpreted as statements that apply in 'the real world': 'But the view that capital structure is literally irrelevant or that "nothing matters" in corporate finance, though still sometimes

---

evidence they presented in their initial paper on the topic. Instead, this is to explain the continued widespread acceptance of the propositions in the face of their empirical refutation.

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.' (Ibid., 100)[14] All in all, Miller's retrospective statements are best read as implying that we should regard the theorems as (part of possibly) true counterfactuals rather than as empirical statements. As this fits with how the economics' profession has received them, this perspective on the propositions would retain its value even if Miller's statements were largely a matter of post hoc rationalization.

So, the MM theorems should be treated in the same way as Galileo's law of falling bodies discussed in section 3.1: rather than as empirical claims, they should be regarded as claims about other possible worlds than the actual one.[15] Economists themselves sometimes appear to regard these issues as analogous to physics. In this respect, it is interesting to note that Miller (1988, 100 and 112) talks about the MM world as a "frictionless" world. The MM theorems hold in this world. The fact that they are lacking in predictive accuracy did not deter Modigliani and Miller from defending them (although at first they thought they performed

---

[14] In a similar vein, Modigliani maintains that the 1958 paper 'was addressed to finance specialists and it was written tongue-in-cheek, <u>not</u> really to demonstrate that leverage could not possibly affect market values in <u>the actual world</u> but to shock those who accepted the then-current naïve view that some debt in the capital structure had to reduce the cost of capital even in the absence of taxes simply because the interest rate was lower than the earnings-price ratio on equity.' (1988, 150; emphasis added)

[15] It is worth stressing that the analogy is limited to this point: the fact that the prediction of the model does not obtain in the actual world does not entail that the theory to which it belongs is false. I am not committed to the claim that the Galilean model played a role similar to that of the model that Modigliani and Miller proposed insofar as explanation is concerned as is discussed in section 3.3.

well empirically). They did not regard the fact that their assumptions are unrealistic as a valid criticism. This view was quite common because of Friedman's defense of unrealistic assumptions. Friedman's concomitant claims that all that matters is the predictive accuracy of a model and that a hypothesis should be 'rejected if its predictions are contradicted ('frequently' or more often than predictions from an alternative hypothesis)' was not quite as well remembered (1953, 9). They did not care so much about the accuracy of a theory's implications but about their 'acceptability' (MacKenzie 2006, 55). Why a highly unrealistic implication might be acceptable nevertheless is an issue that shall be addressed in the next section.

3.3 The Modigliani-Miller Theory as an Explanatory Engine

In spite of the fact that the propositions may well be (regarded as) true when evaluated in combination with their assumptions, economists have put a lot of effort into relaxing them. In the preceding section, we saw that this is <u>not</u> done in order to arrive at a true theory. So, the question arises as to why economists engage in concretization. As we saw in section 1, concretization can serve the purpose of confirmation, because it can increase the empirical accuracy of a model. However, this does not exhaust the purposes for which concretization is used. In order to get a good grasp of how and why this project was undertaken relative to the MM model, I shall discuss three assumptions in particular, those of the absence of taxes, bankruptcy costs, and agency costs. We shall see that they play an important role in the explanation of several contrasts. They contribute significantly to the explanatory power of the Modigliani-Miller theory.

Modigliani and Miller relaxed the assumption concerning taxes themselves in an article that appeared in 1963 (which corrected the claims they had made about this in their

1958 paper). Although taxes per se do not influence their results, a differential tax treatment of debt versus equity does. And firms can in fact deduct interest payments before calculating taxes, whereas they cannot deduct dividend payments. Thus, debt is advantaged over equity. Modigliani and Miller argue that, because of this, financing a firm with debt only is the optimal strategy for maximizing its value. This follows from introducing taxes into the model presented above. Proposition I implies that the capitalization rate of uncertain streams can be expressed as follows: $\rho = \frac{\overline{X}}{V_U}$. This needs to be adjusted in case of corporate taxes: $\rho_\tau = \frac{(1-\tau)\overline{X}}{V_U}$, which implies that the value of an unlevered firm in the presence of corporate taxes is this: $V_U = \frac{(1-\tau)\overline{X}}{\rho_\tau}$. Consider now a levered firm. Firms can deduct the interest paid on debt, *R* or *rD*. So, a levered firm can deduct τR. Modigliani and Miller (1963, 436) go on to suggest that the value of a levered firm is given by: $V_L = \frac{(1-\tau)\overline{X}}{\rho_\tau} + \frac{\tau R}{r} = V_U + \tau D$. The second term of this equation reflects the tax advantage that a levered firm enjoys over unlevered firms. The tax-adjusted version of proposition I implies that the way to maximize the value of a firm it by relying on debt financing only. It shares with the received wisdom at the time the idea that increasing the amount of debt can increase the value of a firm, though the underlying reasoning is very different. But it takes this thought to its extreme by maintaining that the maximal value is achieved by relying on debt only, rather than an intermediate value of the debt to equity ratio being optimal, as the naïve view has it.

In addition to the no-taxes assumption, Alan Kraus and Robert Litzenberger (1973) relax the assumption that there are no bankruptcy costs (as they do so in a different formal framework, the state-preference approach, I discuss this relaxation informally only). The risk of bankruptcy increases with the amount of debt, as bankruptcy is a consequence of a firm's

not being able to fulfill its obligations to its debt holders. Given that bankruptcy involves costs, there might be a trade-off between the tax advantage of increasing the amount of debt and the disadvantage associated to the increase in the risk of bankruptcy. This is exactly what Kraus and Litzenberger argue:

> The market value of a levered firm is shown to equal the unlevered market value, plus the corporate tax rate times the market value of the firm's debt, less the complement of the corporate tax rate times the present value of bankruptcy costs. Contrary to the traditional net income approach to valuation, if the firm's debt obligation exceeds its earnings in some states the firm's market value is <u>not</u> necessarily a concave function of its debt obligation. (Ibid., 918)

In other words, it may well be that maximizing the value of a firm requires it to retain a substantial amount of its equity.

As it turns out, both the no-taxes assumption and the no-bankruptcy assumption serve to answer the question why leverage is relevant rather than being irrelevant:

[Q1] Why is leverage relevant to the value of a firm rather than being irrelevant?

Differential taxation explains what makes debt attractive for firms, while bankruptcy costs explain what makes equity so appealing. [Q1] reveals that the first MM proposition functions as the benchmark or foil in explanations of the effects of leverage on the value of a firm. It is a fact that leverage has an effect on the value of a firm, and this fact is explained relative to the contrasting situation in which leverage is irrelevant. By selecting this as a foil, Modigliani and Miller have in effect abstracted from all factors that have an impact on the relation of leverage on the value of a firm. Thus, they prepare the way for explanations of why leverage is relevant. This reading is supported by Miller's claim that 'showing what <u>doesn't</u> matter can also show, by implication, what <u>does</u>' (1988, 100). Modigliani and Miller have shown that the

fact that the rate of return on debt is lower than that on equity does not explain why capital structure matters. At the same time, the assumptions they need to support their propositions provide directions as to where to look for alternative explanations. The factors that are assumed to be absent in an MM world may well serve to explain why leverage often is relevant. What is more, it appears that by selecting the MM world as a benchmark or foil, Modigliani and Miller in effect maximized the explanatory breadth of their theory. Let me elaborate.

We have seen that two of the unrealistic assumptions of the original MM model can be used for explaining the contrast. This implies that the MM theory has more than a minimal amount of explanatory breadth. Jensen and Meckling (1976) have argued that the effect leverage in fact has on the value of a firm can also be explained in terms of a failure of the assumption of symmetric information, which implies the existence of agency costs. As taxes, bankruptcy costs and information asymmetries undoubtedly exist, this suggests that the MM theory has a substantial amount of explanatory breadth. In fact, the theory can be used for formulating several additional potential explanations, as is implied by Miller's claim that '[e]ven in a no-tax world the "no gain from leverage" implication of the original MM invariance proposition might fail if the new debt was not made junior in status to the old, if the old bond covenant was "open ended," as many still are, and if the new bonds were issued under it.' (1988, 114) The factors mentioned in this passage will not be relevant to all firms, so in that sense the suggested answers are less generic than the three discussed earlier. However, in cases where these factors do play a role, the explanatory breadth of the MM theory may be even larger than we already had reason to believe before.

Since all factors that influence the relation between the capital structure of a firm and its value are assumed to be absent in the original MM model, it appears that it can be used for generating all possible answers to [Q1]. This suggests that the foil provided by the irrelevance

theorem serves to maximize the explanatory breadth of the MM theory. But what determines whether or not an explanation should be ascribed to a particular theory? This depends, inter alia, on how the boundaries of that theory are to be drawn. And no way of individuating a theory exists that is both objective and determinate. Some progress can be made by considering the tax explanation, as there is no doubt that this explanation can be ascribed to the MM theory. First, Modigliani and Miller have proposed the explanation themselves and relate it explicitly to their irrelevance theorem. Second, [Q1] is answered by formulating a model that does include taxes, and comparing its prediction to that of the original model. The model is derived from the original model by relaxing one of its unrealistic assumptions, in this case the no-taxes assumption. Thus, the explanation proceeds by way of concretization.[16] A third feature of the tax explanation that will turn out to be relevant to the issue is the mechanism on which it relies, which is the arbitrage mechanism.

Consider next the bankruptcy-cost explanation provided by Kraus and Litzenberger (1973). Although this explanation is not offered by Modigliani and Miller, it is clear that the model Kraus and Litzenberger use is a descendant of the original model in the sense that it would not have existed if it were not for the MM model. The widely accepted claim that theories are families of models suggests that relations of descent matter to the individuation of a theory (see section 2.1). And the MM model can be regarded as the parent of the model proposed by Kraus and Litzenberger.[17] Furthermore, although their explanation is based on a

[16] See Mäki (2004) for a discussion of how Williamson uses concretization in order to increase the explanatory scope of the theory of the firm. An important difference between his approach and mine is that I use the contrastive model of explanation in order to shed more light on how exactly concretization can be used for the purpose of explanation.

[17] In this connection it is interesting to note that Modigliani and Miller explicitly invited others to contribute to the concretization of their model: 'These and other drastic

different formal framework, the state-preference approach mentioned above, its structure is identical. Just as the tax explanation, the bankruptcy costs explanation proceeds by way of concretization. The impact of bankruptcy costs can be determined by comparing the most unrealistic model with the model in which the no-bankruptcy-cost assumption is relaxed (the model without bankruptcy costs is of course a special case of the model that includes such costs). Leverage is irrelevant to the value of the firm in the former model, while it has an impact on its value when bankruptcy costs are positive. Finally, the explanation relies on the same mechanism as the tax explanation, the arbitrage mechanism. These considerations, which also apply to the agency-costs explanation offered by Jensen and Meckling (1976) and discussed below, suggest that it is reasonable to hold that several of the answers to [Q1] rely on the MM theory.

What about the explanatory scope of the MM theory? Recall that in the context of the contrastive account of explanation explanatory scope is a matter of the number of questions a theory is capable of answering. The MM theory can be and has in fact been used for answering a variety of questions. Whereas the irrelevance theorem provided the foil for [Q1], subsequent concretizations suggested new contrasts. Kraus and Litzenberg take the implication of the model that includes taxes as their point of departure. As we saw above, this

---

simplifications have been necessary in order to come to grips with the problem at all. Having served their purpose they can now be relaxed in the direction of greater realism and relevance, a task in which we hope others interested in this area will wish to share.' (Modigliani and Miller 1958, 296) This suggests that the fact that the bankruptcy cost explanation is offered by other economists is of little relevance to the individuation of the MM theory.

model entails that financing a firm by debt only it optimal.[18] In effect, Kraus and Litzenberg seek to answer the following question:

[Q2] Why do firms retain some of their equity rather than relying on debt only?

Kraus and Litzenberg argue there is a trade-off between the effect of taxes and the effect of bankruptcy costs. The optimal capital structure of a firm depends on the relative sizes of these effects. Thus, they also provide an answer to the following question:

[Q3] Why do firms choose for this particular equity-debt ratio rather than another one?

Jensen and Meckling (1976, 332-33) accept the answer Kraus and Litzenberg provide to [Q2] but argue that there must be other determinants of capital structure in addition to taxes and bankruptcy costs. Debt has not always enjoyed a tax advantage. Nevertheless, firms relied on debt even when it did not. The model that Kraus and Litzenberg present, however, implies that financing a firm by equity only is optimal when there are no taxes but bankruptcy costs are positive. One of the (many) questions Jensen and Meckling seek to answer, then, is this:

[18] Thus, Modigliani and Miller provide what is known as a how-possibly explanation (Dray 1968). Consider the following passage: 'In fact, under conditions which can by no means be dismissed out of hands as implausible, we showed that the value of the private claims [i.e. the shares] might well have no well-defined interior maximum. The optimal capital structure <u>might be</u> all debt.' (Miller 1988, 112; emphasis added) The underlying question is this: How could all-debt ever be optimal? Incidentally, the how-possibly explanation turned to be less far-fetched than it has often been taken to be. More than two decades after the possibility was suggested, capital structures coming close to all-debt financing started to appear (ibid.).

[Q4] Why do firms rely on debt in addition to equity rather than on equity only?

As said above, their answer revolves around asymmetries in information. All this suggests that not only the explanatory breadth of the MM theory is substantial, but that it has a fair amount of explanatory scope as well. And this discussion of concretizations of the MM model is far from exhaustive). Hence, to the extent that it has indeed received confirmation (see note 10), the explanatory power of the MM theory is large in both of the dimensions analyzed here.[19]

The explanatory power of the MM theory is due to the fact that its original model is highly unrealistic and that, as history has proven, the choice of abstractions and idealizations was particularly good. As a consequence of its many unrealistic assumptions, it provides for a

[19] Cools, Hamminga and Kuipers (1994) argue that the model that Kraus and Litzenberger have proposed plays an important role in a process of truth-approximation that has the MM model as its point of departure. Roughly speaking, the idea is that the former model is closer to the way things are in the actual world than the latter model, as it contains fewer assumptions that do not hold in the actual world. This diagnosis is fully compatible to the one I have presented. It should not be taken to imply, however, that the original model does not contain an important insight. In fact, it can be argued that it captures an essential property of the workings of the capital market. Claims of this kind can be accommodated in the context of truth-approximation (Nowak 1989). As was argued in section 3.2, we can take a further step and maintain that when evaluated in combination with its assumptions the model may well be true (cf. Niiniluoto 1989; see Rol 2007 for the case of economics). So, the term 'truth-approximation' should not be taken to imply that the original model is false, but merely that, when considered in isolation, the model does not apply to the actual world.

striking foil that figures in explanations that prompt new explanatory questions, which can be answered by subsequent models that are part of the same theory. The idea that the value of the irrelevance theorem lies primarily in the directions it provides for developing explanations as to why the value of a firm often does depend on its capital structure appears to be well entrenched among economists. Paul Milgrom and John Roberts, for instance, write: 'Something else, besides the simple workings of classical markets, must account for the effect that financial structure seems to have on what investors are willing to pay. … The Modigliani-Miller (MM) theorem itself directly suggests several possibilities.' (1992, 458) In a similar vein, Tirole suggests that the theorem acted as 'a benchmark whose assumptions needed to be relaxed in order to investigate the determinants of financial structures' (2006, 1; see also MacKenzie 2006, 89).

The hard question is of course, what makes for a fertile foil. Economists appear to believe that perfect markets provide for contrasts that are fruitful insofar as explanation is concerned. They take this as far as saying that taxes and bankruptcy costs, factors without which a market economy is inconceivable, are 'market imperfections' (see, for instance, Kraus and Litzenberger 1973, 918). So, they have developed their own peculiar understanding of what makes markets perfect. In effect, then, a lot of explanatory questions in economics are of the following form: Why do markets operate in the way they do rather than as perfect markets? In order for a model of the perfect market case to be accepted, it has to meet other criteria as well. It has to be 'economically plausible, innovative, and analytically tractable', and it should not be 'obvious' (MacKenzie 2006, 245). Furthermore, it should be able to function as 'the foundation for further research that is itself tractable without being trivial' (ibid.). In an interview, Miller asserted that building a good model requires 'the capacity to

find "good abstractions"' (ibid.). The model characteristics mentioned go some way towards accounting for what that capacity involves.[20]

All in all, we can conclude that the MM theory is 'an explanatory engine' that is capable of providing many (potential) explanations (cf. Lipton 1990, 264).[21] What is more, the preceding suggests that the amount of explanatory potential is a function of the extent to

---

[20] In Hindriks (2008) I argue that whether abstractions and idealizations are adequate depends on whether they succeed in isolating a basic mechanism (and that, when they do, concretizations also facilitate understanding of the isolated mechanism). Whether the abstractions and idealizations made by Modigliani and Miller are adequate would then depend on whether the arbitrage mechanism as it operates in conditions of perfect competition is indeed a basic mechanism. This line of thought opens up an avenue for criticizing Modigliani and Miller for those who believe they fail to isolate a basic mechanism. A related point of critique could be that they do not develop their account of the arbitrage mechanism in sufficient detail (i.e. their theory lacks explanatory depth even though its explanatory scope and breadth is substantial). Furthermore, even though they present evidence for their theorem, they have not empirically investigated the underlying arbitrage mechanism as such.

[21] Milton Friedman and, before him, Alfred Marshall already used the metaphor of an engine in relation to theories, without however specifying what they are engines for. Rather than for producing explanations, Donald MacKenzie (2006) argues that theories in financial economics have served as an engine for producing financial markets that approximate the theories more fully than the markets that existed before the theories had been proposed. My claim that the MM theory might be true when construed in counterfactual terms even though the irrelevance theorem does not provide an accurate description of the relation between leverage and the value of a firm in the actual world fits well with MacKenzie's claim that the theories should not be regarded as cameras.

which the MM model is unrealistic. The least we can say is that some theories are powerful explanatory engines because of the fact that they contain a model that is based on a high number of unrealistic assumptions. As such models do not provide accurate representations of the way things are, then, the claims I have been defending can be summarized as the thesis that false models can be powerful explanatory engines.[22] It appears that, as long as the assumptions are well chosen, the MM case supports a stronger claim resembling Friedman's dictum discussed earlier: The higher the number of unrealistic assumptions, the greater the explanatory power of a theory.

4. Conclusion

The question from which we started was why theoretical economists engage in concretization. We have seen that in the MM case the process of relaxing unrealistic assumptions did not serve to get from a falsehood to a truth. When evaluated in combination with its assumptions, the original MM model may well be true. And it did not (just) serve to make the MM theorems (descriptively) relevant to the actual world. It also and perhaps primarily served as a means to providing explanations of a variety of aspects of financial structure. By starting from a model that represents the workings of a perfect market, Modigliani and Miller initiated the construction of a theory that has a large amount of explanatory power, which it owes at least in part to the high number of unrealistic assumptions on which the model is based.

[22] As was noted in section 1, strictly speaking it is not the models that are false, but the hypotheses according to which they provide accurate representations of reality. Furthermore, it is in fact the theory including its original model that is the explanatory engine and not the model.

References

Brealey, R.A. and S.C. Myers (2000) Principles of Corporate Finance, Boston, McGraw-Hill.

Cartwright, N. (1989) Nature's Capacities and Their Measurement, Oxford, Oxford University Press.

Cools, K., B. Hamminga, and T.A.F. Kuipers (1994) 'Truth Approximation by Concretization in Capital Structure Theory', Poznan Studies in the Philosophy of the Sciences and the Humanities 38, 205-28.

Dray, W. (1968) 'On Explaining How-Possibly', Monist 52, 390-407.

Friedman, Milton (1953) 'The Methodology of Positive Economics', in: Essays in Positive Economics, Chicago, Chicago University Press, 3-43.

Friedman, Michael (1974) 'Explanation and Scientific Understanding', Journal of Philosophy 71, 5–19.

Frigg, R. and S. Hartmann (2006) 'Models in Science', The Stanford Encyclopedia of Philosophy *(Spring 2006 Edition)*, E.N. Zalta (ed.), URL = <http://plato.stanford.edu/archives/spr2006/entries/models-science/>.

Garfinkel, A. (1981) Forms of Explanation, New Haven, Yale University Press.

Giere, R. (1988) Explaining Science, Chicago, Chicago University Press.

Hausman, D. (1992) The Inexact and Separate Science of Economics. Cambridge, Cambridge University Press.

Hindriks, F.A. (2005) 'Unobservability, Tractability, and the Battle of Assumptions', Journal of Economic Methodology 12: 383-406.

Hindriks, F.A. (2006) 'Tractability Assumptions and the Musgrave-Mäki Typology', Journal of Economic Methodology 13, 401-23.

Hindriks, F.A. (2008) 'Idealization, Explanation, and Understanding', mimeo.

Jensen, M.C. and W.H. Meckling (1976) 'Theory of the Firm: Managerial Behavior, Agency Costs and Ownership Structure', Journal of Financial Economics 3, 305-60.

Kitcher, P. (1981) 'Explanatory Unification', Philosophy of Science 48, 507–531.

Krajewski, W. (1977) Correspondence Principle and the Growth of Knowledge, Dordrecht, Reidel.

Kraus, A. and R.H. Litzenberger (1973) 'A State-Preference Model of Optimal Financial Leverage', Journal of Finance 28, 911-22.

Kuipers, T.A.F. (2000) From Instrumentalism to Constructive Realism, Dordrecht, Kluwer.

Laymon, R. (1985) 'Idealization and the Testing of Theories by Experimentation', in: Observation, Experiment, and Hypothesis in Modern Physical Science, P. Achinstein and O. Hannaway (eds.), Cambridge (MA), MIT Press, 147-73.

Lipton, P. (1990) ´Contrastive explanations', in: Explanation and its Limits, D. Knowles D. (ed.), Cambridge, Cambridge University Press, 247-266.

MacKenzie, D. (2006) An Engine, Not A Camera: How Financial Models Shape Markets, Cambridge (MA), MIT Press.

Mäki, U. (1992) 'On the Method of Isolation in Economics', Poznan Studies in the Philosophy of the Sciences and the Humanities, 317-51.

Mäki, U. (1998) 'Aspects of Realism about Economics', Theoria 32, 301-19.

Mäki, U. (2001) 'Explanatory Unification: Double and Doubtful', Philosophy of the Social Sciences 31, 488-506.

Mäki, U. (2004) 'Theoretical isolation and explanatory progress: transaction cost economics and the dynamics of dispute', Cambridge Journal of Economics 28, 319-346.

Mäki, U. (2006) 'Remarks on Models and Their Truth', Storia del Pensiero Economico 1, 9-21.

Marchionni, C. (2006) 'Contrastive Explanation and Unrealistic Models: The Case of the New Economic Geography', Journal of Economic Methodology 13, 425-46.

Marchionni, C. (2007) 'Explanatory Pluralism and Complementarity. From Autonomy to Integration', Philosophy of the Social Sciences (this issue).

McMullin, E. (1985) 'Galilean Idealization', Studies in the History and Philosophy of Science 16, 3, 247-73.

Milgrom, P. and J. Roberts (1992) Economics, Organization and Management, New Jersey, Prentice-Hall.

Miller, M.H. (1988) 'The Modigliani-Miller Propositions After Thirty Years', Journal of Economic Perspectives 2, 99-120.

Modigliani, F. (1988) 'MM – Past, Present, Future', Journal of Economic Perspectives 2, 149-58.

Modigliani, F. and M. Miller (1958) 'The Cost of Capital, Corporation Finance and the Theory of Investment', American Economic Review 48, 261-97.

Modigliani, F. and M. Miller (1961) 'Dividend Policy, Growth, and the Valuation of Shares', Journal of Business 34, 411-33.

Modigliani, F. and M. Miller (1963) 'Corporate Income Taxes and the Cost of Capital: A Correction', American Economic Review 53, 433-43.

Niiniluoto, I. (1989) 'Theories, Approximations, and Idealizations', Poznan Studies in the Philosophy of the Sciences and the Humanities, 16, 9-57.

Nowak, L. (1989) 'On the (Idealizational) Structure of Economic Theories', Erkenntnis 30, 225-46.

Roll, M.E.G.M. (2007, forthcoming) 'Idealization, Abstraction, and the Policy Relevance of Economic Theories', Journal of Economic Methodology.

Ross, S.A. (1988) 'Comment on the Modigliani-Miller Propositions', Journal of Economic Perspectives 2, 127-33.

Sober, E. (1999) 'The Multiple Realizability Argument Against Reductionism', Philosophy of Science 66, 542-64.

Stiglitz, J.E. (1988) 'Why Financial Structure Matters', Journal of Economic Perspectives 2, 121-26.

Tirole, J. (2006) The Theory of Corporate Financing, Princeton, Princeton University Press.

Van Fraassen, B. (1980) The Scientific Image, Oxford, Oxford University Press.

Wimsatt, W.C. (1987) 'False Models as Means to Truer Theories', in: Neutral Models in Biology, M. Nitecki and A. Hoffman (eds.), London, Oxford University Press, 33-55.

Bionote

Frank Hindriks

f.a.hindriks@rug.nl

Frank Hindriks is a postdoctoral fellow of the Department of Ethics at the University of Groningen, the Netherlands. Before coming to Groningen, he was Assistant Director of the Erasmus Institute for Philosophy and Economics (EIPE). His research interests include economic methodology, social ontology, and political philosophy. His articles have appeared in journals such as the *Philosophical Quarterly*, *Philosophical Studies*, and *Dialectica*. He co-edited a special issue of *Philosophical Explorations* on collective intentionality.