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Explanation, understanding, and unrealistic models

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ABSTRACT

How can false models be explanatory? And how can they help us to understand the way the world works? Sometimes scientists have little hope of building models that approximate the world they observe. Even in such cases, I argue, the models they build can have explanatory import. The basic idea is that scientists provide causal explanations of why the regularity entailed by an abstract and idealized model *fails* to obtain. They do so by relaxing some of its unrealistic assumptions. This method of ‘explanation by relaxation’ captures the explanatory import of some important models in economics. I contrast this method with the accounts that Daniel Hausman and Nancy Cartwright have provided of explanation in economics. Their accounts are unsatisfactory because they require that the economic model regularities obtain, which is rarely the case. I go on to argue that counterfactual regularities play a central role in achieving ‘understanding by relaxation.’ This has a surprising implication for the relation between explanation and understanding: Achieving scientific understanding does not require the ability to explain observed regularities.

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Highly unrealistic models often entail regularities that are not observed in practice. Reliance on such highly unrealistic models is perhaps not much of a problem for explanations that concern laboratory settings in which the conditions stipulated in the model can be approximated. However, it does present a problem for many explanations concerning phenomena outside of laboratories. Regularities play a crucial role in a wide variety of accounts of explanation, either as explananda or as explanantia. On these accounts of explanation, models that entail regularities that do not obtain fail to be explanatory. Drawing on the contrastive account of explanation, I present a method of explanation on which highly abstract and idealized models do have explanatory import even outside the laboratory. The basic idea is that scientists sometimes provide causal explanations of why regularities entailed by particular models *fail* to obtain.

Scientists often construct very unrealistic models and proceed to develop a more realistic alternative model that is more concrete

or less idealized. This process of constructing more realistic models provides the key to what I call ‘the method of explanation by relaxation.’¹ Starting from an unrealistic model, a new model is constructed by relaxing one of the assumptions of the original model. When the central implications of these models differ from one another, the factor that figures in the assumption that has been relaxed can be invoked in order to explain why the model implication of the original model does *not* obtain in the actual world. This method of explanation works even if the central implication of the more realistic model does not obtain in the actual world either.

The first claim that I defend in this paper is that the method of explanation by relaxation reveals how highly abstract and idealized models, “models that are descriptively false,” can play a central role in successful explanations (Section 1.2). I use an economic theory, the well-known Modigliani–Miller theorem, to illustrate the method (Section 1.3).² And I argue that explanation by relaxation does a better job at capturing the explanatory practices in

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¹ I now prefer ‘explanation by relaxation’ to ‘explanation by concretization,’ which I used in my 2008. The latter could be taken to imply that only decreasing the level of abstraction is important for explanation, whereas this also holds for decreasing the extent to which it is idealized.

² Explanation by relaxation is especially relevant to sciences or disciplines in which laboratory work plays a relatively insignificant role including substantial parts of the social sciences and biology (cf. Morrison, 2009 on the Hardy–Weinberg law in population genetics).

economics than the accounts that Daniel Hausman and Nancy Cartwright have offered (Sections 1.1 and 2.1).³ Subsequently I discuss what Cartwright says about understanding (Section 2.2), which sets the stage for the second claim of this paper: the insights that explanation by relaxation provides can be used to develop an account of scientific understanding (Section 3.1). Again the point of departure is an unrealistic model, one that captures a mechanism. The process of relaxing unrealistic assumptions serves to reveal how the mechanism operates in different contexts. The resulting sequence of models provides the means for understanding the mechanism. I thus provide an answer to a question that Cartwright (2004, p. 242) regards as a pressing one: When does “a false model” provide understanding?⁴

My proposal sheds new light on the relation between explanation and understanding. What is explained when scientists employ the method of explanation by relaxation is the absence of certain regularities. Understanding by relaxation requires one to grasp (only) what regularities would be observed if many of the factors that actually interfere with the functioning of the mechanism were absent. So regularities that are actually observed do not play a role in either explanation by relaxation or understanding by relaxation. This has the surprising implication that explanations of actually observed regularities are neither necessary nor sufficient for understanding (Section 3.2).

1. Regularities and explanation

According to nomological accounts of explanation, including the familiar Deductive-Nomological model, the explanans is a law-statement and it logically entails the explanandum (Hempel, 1965). This means that the relation between (descriptions of) the explanans and the explanandum is one of derivation. In their accounts of explanatory unification, Friedman (1974) and Kitcher (1981) also require that the explanandum be derived from the explanans. Not all of these accounts of explanation are nomological, as some of them do not require law-statements at all. However, regularities play an important role in all of them. Either they do the explaining or they constitute what is being explained.

Regularities play a central role in the accounts of explanation in economics that Hausman (1990, 1992) and Cartwright (1999, 2001) have proposed. Both ascribe an important role to the central implications of the models that scientists employ, which are statements of regularities. They take them to be *ceteris paribus* laws. These laws are supposed to be causal laws. Given that other things are usually not equal, the models that represent them tend to be highly simplified representations of reality. In the process of formulating a model, scientists exclude factors or distort the values of the variables that represent them, i.e. they abstract or idealize. In spite of being unrealistic in one or both of these senses, such models supply the general statements on the basis of which explanations are formulated. As I shall argue in more detail below, this creates a problem for cases in which such statements fail to capture what is actually observed.

1.1. Hausman's inexact deductive method

Hausman (1992) maintains that neoclassical economists, in particular economists who employ general or partial equilibrium models, follow what he calls ‘the inexact deductive method.’ This method consists in part of an account of explanation that is very

similar to the Deductive-Nomological model. The main difference is that Hausman allows for law statements that have exceptions. The first two steps of the inexact deductive method consist of the formulation of a potential explanation. An economist first formulates a law statement. Then she derives a prediction of a particular event from the law statement and a statement concerning initial conditions. The method is called ‘deductive’ because it takes explanation to be a matter of deduction. The last two steps of the method concern confirmation. Confirmation is needed in order to determine whether a potential explanation is successful.

The most distinctive feature of the inexact deductive method is its first step, which Hausman describes as follows: ‘Formulate credible (*ceteris paribus*) and pragmatically convenient generalizations concerning the operation of relevant causal factors’ (Ibid., p. 222). The generalizations should be law statements. Rather than universal generalizations, however, they are restricted in their scope: they are *ceteris paribus* laws, or tendency statements. Furthermore, the *ceteris paribus* clauses are not fully specified. Some causal factors are left implicit, and it is not known precisely which factors they are. In other words, the extension of the *ceteris paribus* predicate is unknown. This is why the method is called ‘inexact.’ Note that Hausman does maintain that scientists should know the most significant interfering factors if they are to use a qualified law statement for the purposes of explanation (ibid., p. 141).

Hausman succeeds in capturing several important features of the economics he discusses. Economists often make *ceteris paribus* claims, and, as we shall also see below, they tend to have strong opinions about the major factors that might interfere. In spite of this, I think his account of explanation in economics is flawed. It does not provide a very charitable interpretation of their practice.⁵

To see why, we have to consider some of the assumptions economists often make. These include perfect information, the absence of transaction costs, and constant returns to scale—assumptions which we have good reason to believe are (frequently) false. These assumptions often figure in the *ceteris paribus* clauses economists rely on. The generalizations are formulated on the basis of models that depend on these assumptions. Often those generalizations cannot be derived from models in which some of them are relaxed. In other words, the model implications can be quite sensitive to the abstractions and idealizations the models involve. This means that the *ceteris paribus* statements economists formulate tend to be of little use for the purpose of explanation, at least if it is conceived of as a matter of deduction in the way Hausman does. There is little reason to believe that the predictions that can be formulated on the basis of the relevant models will be correct: it is very likely that one or more of the factors that figure in the *ceteris paribus* clause disrupts the regularity entailed by the model.

Hausman believes that many of the generalizations economists propose are false (ibid., pp. 207 and 209; 2009, p. 37). However, he defends their continued acceptance arguing that the *ceteris paribus* clauses may be the culprit (ibid.). It may be that the predictions are indeed unsuccessful because these clauses are not satisfied. If this is the case, however, Hausman's account of explanation faces a problem. The falsity of the generalizations entails that, more often than not, what is actually observed cannot be derived from the model employed. This in turn means that the model cannot be used as a basis for successful explanations. What is more, it cannot even be used in order to formulate a potential explanation: it does not capture an observed regularity of which it might uncover the causal process that generates it. Hausman's views, then, imply that large

³ See Alexandrova (2008) for another critique of Hausman and Cartwright's views on explanation.

⁴ Wimsatt (1987) has played an important role in getting questions concerning the function of “false models” on the philosophical agenda.

⁵ Hausman (2009) has recently abandoned his inexact deductive method as an account of explanation in economics. He has come to agree with Woodward (2000, 2003) that preserving the deductive method by adopting a weaker notion of a law is not very promising (Hausman, 2009, p. 48). Hausman now believes that scientific explanation should be understood in terms of causation rather than laws.

parts of economics are explanatorily irrelevant. Perhaps he does not see this because he is too optimistic about the extent to which relatively realistic economic models approximate observed reality.

Now it may in fact be that few of the models that economists have proposed provide the basis for successful explanations. It is, however, rather implausible that by their own lights most of the models they have proposed cannot even be used as the point of departure for potential explanations. The only way to make sense of this idea would be to say that this is why economists put so much effort into relaxing assumptions. The idea would be that they develop less abstract and idealized, or more realistic models in order to derive generalizations that do fit with what is observed. The problem with this idea is that, as illustrated below, the implications of these more realistic models often fail to hold as well.⁶ This means that those models will also be explanatorily irrelevant. If possible at all, we should try to avoid this conclusion. As Hausman recognizes, many economists take explanation to be an important goal of their discipline. In order to make sense of this, an account of explanation is needed that does not require that the phenomenon to be explained be entailed by the model used to formulate the explanation. As I argue in the next section, the method of explanation by relaxation meets this condition.

1.2. Explanation by relaxation

Explanation by relaxation requires two models, one more realistic than the other. The more realistic model should be formulated by relaxing an assumption of the relatively unrealistic model. The central implication of the original model or the regularity it entails often fails to hold in practice. The more realistic model frequently has implications that conflict with the central implication of the original model. However, it may well be that its own model implication is not observed either. At this point one might despair as to how two such models can be used to explain anything. A solution comes into view once we let go of the idea, to which Hausman subscribes, that an explanans should consist of a model implication. Instead, the factor that figures in the assumption that is relaxed can be regarded as the explanans. The explanandum can then consist of the fact that the implication of the original model *fails to hold*. Let me elaborate.

The method of explanation by relaxation takes the contrastive approach to explanation as its point of departure (Garfinkel, 1981; Lipton, 1990; Van Fraassen, 1980; Woodward, 2003). On the contrastive approach the relation between the explanans and the explanandum is not one of deduction. Instead, an explanation consists of an answer to a question, more specifically an answer to a contrastive why-question. Such a question has the following form (using ‘*f*’ for facts and ‘*c*’ for contrasts or foils): Why *f* rather than *c*? The underlying idea is that we never explain facts *simpliciter*. The famous example is that of Willy Sutton the bank robber. When asked by a priest why he robs banks, he answers by pointing out that that is where the money is. This indicates that he has not properly understood the contrast implicit in the priest’s question. Whereas Willy took the question to be ‘Why rob banks rather than rob something else?’ the intended question was ‘Why rob banks rather than not rob anything?’

The novelty of explanation by relaxation resides in the way the explanans and the explanandum are to be specified. The fact to be

explained is inspired by the most unrealistic model of the two models that are used to generate the explanation: it is the fact that the implication of this model *fails to obtain* (even though it could obtain). The contrast is that this model implication does obtain. The explanans consists of the factor that occurs in the assumption that is relaxed in order to construct the more realistic model. That model has a model implication of its own, which differs from that of the original model. It entails that the regularity described by the original model implication is not observed. This reveals what is distinctive about the method of explanation by relaxation. It relies on unrealistic models, models that are general in that they surpass the particularities of specific cases. In spite of this, *regularities are neither what does the explaining nor what is being explained*. Explanation by relaxation is concerned with *explaining the absence of regularities rather than their presence*.⁷

The method of explanation by relaxation consists of six steps. (1) The first step consists of the formulation of a very abstract and highly idealized model: model M_o . (2) The next step is a matter of identifying one of the model’s implication as its key implication in the context at issue, I_o . Usually, this is a (statement of a) regularity. (3) Subsequently, a new model is formulated by relaxing one of the assumptions of M_o . This is done by adding a factor F to the original model, or by increasing the accuracy of its value. The former makes the model less abstract; the latter makes it less idealized. The model that is constructed in this way is model M_r . (4) The fourth step of explanation by relaxation consists of determining the central implication of the relatively realistic model, I_r . (5) If the implications of the two models differ from one another, they can be used for the purpose of explanation by relaxation. If I_r is indeed incompatible with I_o (in the sense that if the former occurs the latter does not), the explanandum can be formulated as follows: Why I_o rather than I_r ? So the fifth step consists of formulating a question concerning the fact that I_o fails to obtain, which it does in M_r , rather than that it does obtain, as it does in M_o . (6) The sixth and final step consists of invoking (the value of) F in order to propose a (potential) explanation of the contrastive fact, or formulate a (potentially true) answer to the contrastive question.

The method of explanation by relaxation differs in important ways from what I shall call ‘the method of approximation,’ which encompasses the methods of de-idealization (McMullin, 1985), concretization (Nowak, 1989), and that of decreasing abstraction (Lindenberg, 1992). Proponents of the method of approximation grant that highly unrealistic models can have a lot of analytic power. However, they maintain that an unrealistic model can only be used for formulating a genuine explanation if it is used as a point of departure for constructing increasingly realistic models. What is more, this process of decreasing the extent to which a model is abstract or idealized has to be continued until the model approximates the way things are. In contrast, the method of explanation by relaxation reveals why explanation does not require an extended process aimed at constructing a realistic model. Just one step of decreasing the extent to which a model is abstract or idealized can be sufficient for the purpose of explanation. Neither one of two models used for explanation has to be realistic in the sense that it approximates the way things are. This implies that neither one of the two models entails regularities that (approximately) obtain. The method of approximation does insist on this.⁸

⁶ In a critique of Cartwright, Elgin & Sober (2002) argue that explanantia can involve idealizations that have distorting effects on the values of the predictions as long as these distortions are negligible. Although there may be nothing wrong with this move in principle, it cannot save Hausman’s account of explanation in economics. Many economic models involve significant abstractions and idealizations with non-negligible effects.

⁷ Marchionni (2006) argues that being precise about explanatory questions is important because it can serve to reveal that different theories that appear to have the same explanandum often have different explananda. My argument is complementary to that of Marchionni: it is also important because models that might appear to be explanatorily useless can turn out to have explanatory value.

⁸ The method of relaxation does combine well with the method of isolation (Mäki, 1992, 1994). Its central contention is that the fact that a model is (very) unrealistic need not be inconsistent with its being true. I argue that this fact need not be inconsistent with the model featuring in a true explanation, and I leave open whether the model as such is true or false.

The method of relaxation is particularly useful in situations in which the method of approximation cannot be applied. Some scientific models are so unrealistic that it is simply not feasible to develop a version of it that approximates the way things are. For this reason the explanandum of the method of relaxation differs from that of the method of approximation. Whereas the method of approximation explains an event or a regularity that actually obtains, the method of relaxation explains why a regularity does not obtain. A related difference is that the method of relaxation requires at least two models for an explanation, whereas in principle one model suffices for applying the method of approximation. The explanandum of an explanation that proceeds by approximation is an implication of a particular model. Only that model is needed in order to formulate the explanation. In contrast, explanation by relaxation requires one model in which a regularity obtains, and one in which it does not. The assumption that has been relaxed in order to formulate the relatively realistic model pertains to the factor that explains why the original regularity does not obtain. Note that approximation requires relaxing assumptions, whereas relaxing assumptions does not require approximation. Hence, the method of approximation is more demanding in what it tries to achieve. The method of relaxation complements the method of approximation because it facilitates the formulation of explanations in situations in which the method of approximation does not work.

One might be inclined to think that explanation by relaxation concerns a model world rather than the actual world. The contrastive questions at issue are indeed motivated by models, and answering one of them requires two models. However, what is being explained is a fact about the world, the fact that a certain regularity fails to obtain even though it *could obtain*. This qualification, which I presented just in passing earlier, deserves to be emphasized. After all, it is easy to construct models that imply regularities that are not observed. Many of them, however, will not be of any explanatory value. The original model should of course capture something that could occur. A particularly fruitful way in which this minimal requirement can be given more substance invokes the notion of a mechanism. The idea is that the proposed way of specifying the explanandum makes sense in particular if the original model adequately represents a mechanism as it operates in the absence of (particular) interfering factors. This means that in the absence of interfering factors I_o would obtain.

The method of explanation by relaxation is not wedded to a particular account of confirmation. The central questions of this paper are how “false models” can be used in a way that is explanatory and, as discussed in Section 3, how they can be conducive to understanding. Even so, it is worthwhile to point out that the notion of a mechanism can also be used to answer the question why anyone would believe in an explanation provided by means of relaxation. The literature on the method of approximation offers some ideas that can also be used for gathering support for applications of the method of relaxation. McMullin (1985) argues that de-idealizing a model provides evidence for the structure postulated by the model if new empirical discoveries are made and a theoretical justification can be provided for the de-idealization (see Bokulich, 2011 for more on this). Mechanisms can play an important role in providing theoretical justifications, or so I propose.

Evidence for explanations by relaxation will often be indirect. Frequently a significant number of model assumptions will not match the way things are. Furthermore, the regularity implied by

the model will rarely approximate what is observed. Sometimes, however, independent information is available concerning a mechanism, its composition, and the way it functions. This information can be used for justifying particular steps in the process of de-idealizing and concretizing assumptions. When a relaxation is motivated by information about the underlying mechanism, one may well be in a position to form reasonable expectations about how its functioning is affected. Anna Alexandrova suggests that in such situations scientists often ‘extrapolate...on the basis of knowledge of capacities and disturbing factors present’ (2008, p. 401). This means that, even if the resulting model is still highly unrealistic, it may be possible to make sensible inferences from the world to the model. A number of philosophers have argued that mechanisms are important for making reliable causal inferences (Reiss, 2008; Steel, 2004; Weber, 2007). In the kind of case at hand, this could be a matter of combining knowledge concerning the (parts of a) mechanism.⁹

1.3. An illustration: the Modigliani–Miller theorem

This six-step method of explanation by relaxation can be illustrated using the Modigliani–Miller theorem in financial economics, one of the most well entrenched theorems in economics at large.¹⁰ According to this theorem, the value of a firm is independent of its financial structure, the debt to equity ratio. I refer to this theorem as ‘ I_{MM} .’ I_{MM} is the key implication of the model Modigliani and Miller formulate, M_{MM} . Since the introduction of their model in 1958, many models have been formulated in which this implication fails to hold. Modigliani and Miller (1963) showed that the implication breaks down when the assumption that taxes are absent is relaxed. This is the first complicating factor that has been added to the original model, F_1 . A differential tax treatment of bonds versus shares that favors the former over the latter is quite common in practice. It implies that it is optimal to finance firms with debt only. Kraus and Litzenberger (1973) proved that if, bankruptcy costs are assumed to be present as well, some mix of equity and debt will be optimal. This is I_{KL} . Jensen and Meckling (1976) in turn have argued that this optimum is influenced by agency costs due to the presence of asymmetries in information. They maintain that the value of a firm is influenced by the extent to which shares are manager-owned (I_{JM}). Bankruptcy costs and agency costs are factors F_2 and F_3 , respectively.

How can these models be used to explain anything? The first thing to do is to identify the fact and the contrast. The fact is that the Modigliani–Miller theorem fails to hold in practice, i.e. I_{MM} . The contrast is that it does hold. So the contrastive why-question to be answered is this: Why does the value of a firm depend on its financial structure rather than being independent of it? The motivation for asking this question is that when the underlying mechanism, which Modigliani and Miller call ‘the arbitrage mechanism,’ operates without interference, the value of a firm would indeed be independent of the debt to equity ratio. The idea underlying the arbitrage mechanism is that agents will not let an opportunity to make a profit go to waste. Any agent who sees such an opportunity will seize it immediately. Modigliani and Miller do not question the idea that this mechanism might not be in place, or that it might not function optimally. Given that so many people try to make money in this market, it is not very surprising that Modigliani and Miller took it for granted. Even so, the fact that they do not provide any evidence in favor of it makes them vulnerable to criticism on this score.

⁹ Sugden (2000, 2009) argues that models can sometimes be regarded as credible worlds that relate to the actual world in a very loose way. In spite of their indirect relation to the world, such models do provide the basis for inductive inferences from the model to the world, or so Sugden argues. Whereas Sugden takes the credible world strategy to be a rival of the isolation strategy, Mäki (2009) does not. To the extent that it fits the method of isolation (see Cartwright, 2009; Knuuttila, 2009 for critical discussions), the epistemic tools of the credible world strategy are also available to the method of explanation by relaxation.

¹⁰ See my 2008 for a more elaborate discussion.

This issue, however, need not be settled in order to appreciate the logic of their explanatory strategy. The explanatory question mentioned above can be answered by means of relaxation if there is a more realistic model in which the contrast does not obtain. As we saw, this holds for the model that incorporates a differential tax treatment of bonds versus shares. The fact that this factor is present in practice provides a potential answer to the question just formulated. The results of Kraus and Litzenberger on the one hand and of Jensen and Meckling on the other reveal that the same holds for bankruptcy costs and information asymmetries. This implies that our explanatory question has no fewer than three potentially correct answers (all of which can be true at the same time): one for each of the three factors F_1 , F_2 , F_3 that can be invoked to explain (or at least to provide a potential explanation of) why I_{MM} rather than I_{MM} .¹¹

It is no accident that the first two assumptions to be relaxed were those of the absence of taxes and bankruptcy costs. Modigliani and Miller had already singled these out as being particularly important. Although initially they hoped their theorem would capture the actual relation between (i.e. independence of) two factors, it is now conventional wisdom that it did not and that this is what needs to be explained. 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, p. 458). In a similar vein, Jean 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, p. 1; see also MacKenzie, 2006, p. 89).

These passages confirm my claim that explanation by relaxation provides a more accurate interpretation of the explanatory practices of partial equilibrium theorists than accounts such as Hausman’s that insist on regularities as explananda. Many economists are aware of the fact that the central implications of their models fail to hold in practice. Nevertheless, they continue to accept them not only because they might be true of situations only realized in possible worlds other than the actual one, but also because they can be used to explain features of the actual world. All this reveals that highly unrealistic models can be used successfully for explanatory purposes even if their implications do not obtain. As said before, what is being explained is the absence of a particular regularity. The question that I shall address in the remaining sections is how such models can be used for achieving scientific understanding.

Before turning to understanding I should remark on how my analysis reflects on economics. What I have said about explanation and what I shall say about understanding by and large reflects positively on economics insofar as it provides a rational reconstruction of their explanatory practices. The reconstruction, however, also provides angles from which they can be criticized. In this paper I take for granted, for instance, that the models used for explanation have somehow received confirmation (see Section 1.2 for more on confirmation). This need not be the case in fact. Furthermore, the emphasis I put on mechanisms can also be used to develop a critique of the modeling practices in large parts of economics. A criticism worth exploring is that many economists put too little effort into investigating mechanisms, their components, and the way they operate.

2. Cartwright on explanation and understanding

2.1. Explanation

My main criticism of Hausman also applies to Cartwright’s account of explanation in economics. Whereas Hausman took regularities to be the things that do the explaining, Cartwright takes their occurrence to be what needs to be explained. This also has the unattractive consequence that many of the models economists construct will be of little explanatory relevance, even by their own lights. And it does not fit with the self-understanding of economists. Furthermore, an alternative account of their explanatory practices is available, which is supported by the way in which many prominent economists talk about their models (Section 1.3). The method of explanation by relaxation enables us to see how highly unrealistic models can be explanatorily relevant outside of laboratories, and puts us in a position to appreciate that such practices may be valid (which is not to say they always are).

The notion of a mechanism plays an important role in Cartwright’s account of science. Appreciating the respects in which her account of explanation in economics is flawed provides a useful point of departure for investigating how the process of relaxing assumptions can be used to shed light on the role mechanisms play in economic modeling practices. I shall argue that it enables us to see that even if model regularities are sometimes of little explanatory value they can provide understanding. More specifically, the claim that I shall defend is that sequences of increasingly more realistic models can provide an understanding of the way mechanisms work.

At the beginning of Section 1, we saw that Cartwright insists that the law statements that scientists employ be causal. She regards *ceteris paribus* laws as claims about causal powers or capacities (1989, 1999, 2002). More specifically, she maintains that such laws only hold ‘relative to the successful repeated operation’ of a mechanism, a notion that she partly explicates in terms of laws. For this and other reasons, Cartwright uses the term ‘nomological machine,’ which she defines as follows: A nomological machine ‘is a fixed (enough) arrangement of components, or factors, with stable (enough) capacities that in the right sort of stable (enough) environment will, with repeated operation, give rise to the kind of regular behaviour that we represent in our scientific laws’ (1999, p. 50). A nomological machine, then, is an entity that has parts and that is located in an environment. Furthermore, it has several capacities that warrant a characterization of the way it operates in terms of laws, as long as certain stability conditions are met.¹²

Cartwright rejects the Deductive-Nomological model of explanation and covering-law accounts in general. On her view, the things that explain are (representations of) capacities or nomological machines rather than (mere) laws or law statements (ibid., pp. 58 and 138). So she regards capacities or nomological machines or representations of them as explanantia. Cartwright says little about how exactly this is supposed to work, but we can reconstruct her view by investigating what she says about models and nomological machines. She takes models to be blueprints of nomological machines, by which she means that models reveal which components such machines have, how they are arranged, which capacities they have, and what behavior, or which regularity results from their joint operation. Cartwright maintains that ‘[w]here there is a nomological machine, there is lawlike behaviour’ (ibid.,

¹¹ In my 2008 I point out that each of the more realistic models mentioned has in effect itself been used as the point of departure for another explanation that has the same format. Thus, this account of explanation also sheds light on the heuristic value of the process of relaxing assumptions.

¹² In relation to economics, Cartwright uses the term ‘socio-economic machine’ for what is basically the same idea. I use the term ‘nomological machine’ instead, because the role that laws play in Cartwright’s conception of a mechanism is of crucial importance to my argument. I use the term ‘mechanism’ in a way that differs from Cartwright. She uses the term for parts of nomological machines, or for capacities (Cartwright, 1999, pp. 142–144). On my view, a mechanism is (at least) a stable configuration of capacities or causal powers (see also note 16). When the term is used in this way, nomological machines are a kind of mechanism.

p. 57), and that ‘it takes a nomological machine to get a regularity’ (ibid., p. 73; see also p. 59). For the case of economics, she makes the point as follows: ‘Socio-economic laws are created by [nomological] machines’ (ibid., p. 149). This suggests that, according to Cartwright, we explain regularities rather than singular events. She does indeed talk of the explanation of regularities (ibid., p. 58). So, regularities are the explananda rather than the explanantia, as Hausman has it. And they are explained in terms of models of mechanisms or nomological machines that entail those regularities.¹³

The basic question I want to pose in response, at least insofar as economics is concerned, is: Which regularities? Economic models usually do not imply regularities that are observed in practice. Moreover, economists rarely construct laboratory situations in which they might be observed. So economic models rarely capture the actual behavior of economic entities. Cartwright’s own criticisms of covering-law accounts in fact rely on the claim that scientific models rarely save the phenomena. More often than not, models pertain to regularities that could be observed if circumstances were different. This in turn implies that they cannot be used for the purpose of explanation as conceived by Cartwright. Explanation is, after all, *factive*: in order for an explanation to be successful it must be true in the sense that it must pertain to phenomena that are actually observed. Relative to Cartwright’s account of explanation this means that the relevant regularities must actually obtain. As they often do not, the kind of explanations Cartwright attributes to economists frequently fail. At this point, the argument presented in Section 1.1 can be used again in support of the claim that the method of explanation by relaxation is to be preferred as an account of explanation in (significant parts of) economics.¹⁴

2.2. Towards understanding

There is, however, a lot to be learned from Cartwright’s discussion of these issues. The striking thing is that, just as Hausman, she is well aware of the fact that few implications of economic models hold in practice, as is implied by her claim that ‘the most immediate problem with regularities is that, as John Stuart Mill observed, they are few and far between’ (ibid., p. 141). Cartwright makes basically the same point in her discussion of a game-theoretic model about debt contracts proposed by Oliver Hart and John Moore. She notes that regularities are deduced from the model and goes on to make the following observation:

The cost is that the rules of the games that allow these strict deductions may seem to be very unrealistic as representations of real life situations in which the derived regularities occur... The kind of precise conclusions that are so highly valued in contemporary economics can be rigorously derived only when very special assumptions are made. *But the very special assumptions do not fit very much of the economy around us.* (ibid., pp. 148–149; emphasis added)

What Cartwright seems to have in mind here is that the assumptions are ‘very special’ in that the regularities will only be observed in the extraordinary event that the assumptions do fit the economy. She does not seem to be worried, however, by the fact

that economic models often pertain to regularities that occur only in conditions that are very different from those in the actual world. She writes, for instance, that a model in economics is best represented ‘as a design for a [nomological] machine which, if implemented, should give rise to the behaviour to be explained’ (ibid., p. 139; emphasis added). This fits with her talk of models as blueprints of nomological machines. The fact that they are blueprints suggests that they do not (need to) represent the way things actually are. Nevertheless, it creates a puzzle: How can Cartwright’s admission that few of the regularities implied by the relevant models are observed be squared with the significance she ascribes to those models, given that on her own account of explanation the fact that the regularities are not observed undermines their explanatory value.

The solution to this riddle lies in the fact that Cartwright pays little attention to explanation in her account of economics and focuses instead on understanding. She has in fact said little about explanation since 1983. Furthermore, she is well aware of the fact that economic models rarely imply regularities that obtain. In order to avoid the conclusion that it has no point, I suggest interpreting her claims about nomological machines in the context of economics in terms of understanding rather than explanation. This reading is confirmed by the kind of knowledge economists are after according to Cartwright. Having discussed the problem that regularities are ‘few and far between’ she argues that economists are ultimately concerned with something other than observable regularities:

The second problem with regularities is that, as in physics, most of the ones there are do not reflect the kind of fundamental knowledge we want, and indeed sometimes have. We want, as Mill and Haavelmo point out, to understand the functioning of certain basic rearrangeable components... What we need to know is about the capacities of the distinct parts. (ibid., pp. 141–142)

I think this is exactly right. Fundamental regularities reveal something about how capacities or causal powers and the mechanisms they constitute work. The models that postulate such regularities may provide understanding of mechanisms, even though they do not explain anything. At least in the context of economics, what Cartwright says about nomological machines can only be made sense of if it is meant to gesture at how models can provide understanding of mechanisms. Her account of nomological machines can be seen as a major step towards an answer to the pressing question mentioned in the introduction: When does “a false model” provide understanding?¹⁵

3. Mechanisms and understanding

3.1. Understanding by relaxation

The (or at least a) way to achieve understanding of a mechanism by means of models, I propose, is to combine the process of deriving regularities from models with that of relaxing some of their assumptions in order to construct more realistic models. The first step is to develop a model that isolates the mechanism. I take a mechanism to be a stable configuration of causal

¹³ The reconstruction I give here of Cartwright’s views on explanation is based on her recent work in which she characterizes models as blueprints (of nomological machines). When she presented her simulacrum account of explanation in her 1983 she described them as fictions.

¹⁴ Alexandrova (2008) presents an alternative to Hausman’s and Cartwright’s views on explanation in terms of causation rather than laws. She argues that models can be used to formulate open formulae by picking and choosing what assumptions of the model feature in the causal hypothesis to be tested. Although this view is much more flexible than those of Hausman and Cartwright, it suffers from the same problem. The causal hypothesis that a scientist formulates has to be confirmed ‘by finding a *material realization* of the causal relation in the hypothesis’ (ibid., p. 400). See Section 1.2 for a discussion of an alternative suggestion she makes.

¹⁵ Strictly speaking, models are either true by definition, or they are neither true nor false (at least on the semantic conception of theories, which Cartwright employs). Presumably, Cartwright (2004) uses ‘false model’ as shorthand for ‘a model that is descriptively inaccurate’ (just as I do). In other words, a model is false when the theoretical hypothesis that its central implication obtains in reality is mistaken.

powers.¹⁶ So modeling a mechanism is a matter of modeling its causal powers and the way they interact. Isolating a mechanism is a matter of depicting the way it works in a situation in which no interfering factors are present. This is achieved by means of abstraction and idealization.¹⁷ As a consequence, the model will be (very) unrealistic. If it succeeds in isolating the mechanism, its central implication will provide a description of the way the mechanism as such functions. Knowing how the mechanism operates without interference is the *first condition* for understanding the way it works. I call a model that aims at isolating a mechanism in this way ‘a basic model.’ Building such a basic model requires knowledge of the capacities or causal powers involved in the relevant mechanism and how they interact. Some such knowledge is surely required to understand the way a mechanism works.¹⁸

The next thing to do is to develop several more realistic alternatives to the basic model. The reason for doing so is that one needs to grasp how the mechanism is affected by some major interfering factors. Mechanisms tend to operate in very complex environments. This suggests that only knowing how they operate in the most unrealistic context of all can never add up to genuine understanding. At the same time, however, knowing exactly how they actually operate in particular situations seems to be too much to ask. Such knowledge will only be available after extensive investigation of those situations. A theoretical scientist need not possess such applied knowledge in order to understand the workings of a mechanism. Knowing how the mechanism is affected by some major interfering factors, then, is the *second and last condition* for this kind of understanding of the way in which a mechanism works.

These two conditions of understanding are directly related to the discussion of Cartwright’s views on explanation discussed in Section 2.1. The problem identified there was that, on Cartwright’s account, the explanations that economists provide are almost always unsuccessful because their models involve regularities that do not obtain. Note that such models would be explanatory if the regularities they imply did obtain. This means that they provide what might be called ‘would-be explanations.’ Would-be explanations are by definition inadequate as explanations, because they are not factive. The thing to see is that such would-be explanations are vital for achieving an understanding of mechanisms.¹⁹ In order to formulate a would-be explanation one needs to construct a model of a mechanism that would produce a regularity if the appropriate conditions were to obtain (which they do not). I suggest calling the mechanism as it occurs in a context without interfering factors, the mechanism as it operates on its own, ‘the basic mechanism.’ A basic model, then, is a model of a basic mechanism. On the conception I propose and defend in this paper, understanding a mechanism requires understanding the causal powers of which the basic mechanism is composed as well as their configuration. In addition to this, it involves knowing how the basic mechanism functions and how its functioning is affected by some major sources of interference. These conditions come down to grasping some would-be explanations pertaining to the mechanism in different contexts.

3.2. In defense of understanding by relaxation

Why should we accept understanding by relaxation as an adequate conception of understanding in relation to mechanisms? First, as discussed above and clarified further below, it provides an answer to the question of how “false models” can be used to provide understanding (see note 15). As a consequence, we need not dismiss offhand all those models that can easily appear to be too unrealistic to have genuine scientific significance. Second, the account fits well with an increasingly popular account of scientific understanding proposed by de Regt and Dieks (2005). They defend the following criterion for understanding (CU)²⁰:

A phenomenon *P* is understood by scientists (in context *C*) if an adequate theory *T* of *P* exists of which they can recognize qualitatively characteristic consequences without performing exact calculations.

Although this requires some unpacking, which I provide shortly, there is a clear analogy between CU and the condition that a scientist knows how the mechanism functions in various contexts.

Let me illustrate these two points using the Modigliani–Miller theorem. As we saw in Section 1.3, the mechanism underlying this theorem is the arbitrage mechanism. If it is not interfered with, the outcome of this mechanism as conceptualized by Modigliani and Miller is that the value of a firm is independent of its financial structure. This is a qualitative consequence of the model of the basic mechanism in the sense that de Regt and Dieks have in mind. Significant interfering factors include taxes, bankruptcy costs, and information asymmetries. No exact calculations are needed to recognize the relevant model implications (I_{MM} , I_{KL} , and I_{JM} were expressed in purely qualitative terms in Section 1.3).

This discussion prepares the way for formulating a criterion for understanding the way mechanisms work (CU_M):

The way a mechanism *M* works is understood by scientists (in context *C*) if there is an adequate theory *T* about mechanism *M* that consists of a sequence of models of *M*, a basic model and some of its more realistic alternatives, of which they can recognize qualitatively characteristic consequences without performing exact calculations.

CU_M is basically an application of CU to the case of mechanisms. CU is formulated in terms of phenomena, CU_M in terms of mechanisms. Furthermore, CU_M employs the notion of a model, while CU is cast in terms of a theory only. CU_M encapsulates the two necessary and jointly sufficient conditions of the account of understanding presented in Section 3.1, and supports my claim that the account does indeed fit with the generic conception of scientific understanding that de Regt and Dieks have developed. Finally, CU_M explicates how “false models” provide an understanding of the way mechanisms operate. It reveals that such understanding requires the construction of a sequence of models some of which are more realistic than others. Note that this implies that understanding by relaxation

¹⁶ This characterization is consistent with Cartwright’s conception of nomological machines. It is also compatible with the conceptions of mechanisms defended by Machamer, Darden, & Craver (2000) and Bechtel & Abrahamsen (2005), although their explicit formulations are very different. Note that this characterization is meant to provide sufficient conditions only. In order to arrive at a definition in terms of necessary and sufficient conditions the least that needs to be added is something like ‘that is *ceteris paribus* productive of some regularity’ (see chapter 6 of Reiss, 2008 for a rather similar definition of the notion and a useful critical discussion of other definitions). Recall that Cartwright uses the term ‘mechanism’ differently (note 12).

¹⁷ I take abstraction to be a matter of omission, and idealization a matter of exaggeration (cf. Cartwright, 1989; Wimsatt, 1987). Mäki (1992, 1994) regards abstraction and idealization as kinds of isolation. In contrast to this, I regard abstraction and idealization as the means scientists employ in order to achieve the goal of isolation.

¹⁸ Note in this connection that Cartwright (1999, pp. 53–54) maintains that scientific understanding differs from everyday understanding in that scientists often know some explicit rules about how a capacity will combine with other capacities.

¹⁹ What I call ‘a would-be explanation’ is distinct from potential or how-possibly explanations. A would-be explanation pertains to a regularity that is not observed, although it could be in a situation different from the actual one. A how-possibly explanation concerns a phenomenon that is actually observed, but the explanatory factor that is invoked does (or may) not successfully explain the phenomenon in the situation under investigation (Craver, 2007: 112 and 139).

²⁰ CU results from combining the Criterion for Understanding Phenomena and the Criterion for the Intelligibility of Theories that de Regt & Dieks (2005, p. 150 and 151) propose. A theory is adequate, according to them, if it meets the usual logical, methodological and empirical requirements (the word ‘adequate’ is mine and facilitates a more compact formulation of the criterion). See also de Regt (2009a, 2009b).

depends on explanation by relaxation. In fact, genuine understanding of the mechanism usually requires several rounds of explanation by relaxation. After all, it involves knowing how several factors affect the way the mechanism operates. Once several explanations have been provided, they generate an understanding of why certain regularities fail to obtain. More importantly, they also provide understanding of the mechanism as such.

The notion of understanding by relaxation has significance that is independent from that of the method of explanation by relaxation. What is explained when the method is applied is why something does not work the way it would work in the absence of interfering factors. Suppose that, for one reason or another, one does not take such contrastive facts to be legitimate targets of explanation. This need not detract from the significance of understanding by relaxation. Given the account offered, there is no reason to insist that scientific understanding requires one to be able to explain any regularities that are actually observed. A grasp of the way the basic mechanism works and of the ways its functioning is influenced by interfering factors suffices. This line of reasoning leads towards a conception of *understanding without explanation*. Suppose, however, that one does accept explanation by relaxation as a sensible method to which scientists sometimes resort. In that case understanding is closely related to explanation. After all, information about counterfactual situations is of crucial importance for understanding the way a mechanism works. And such information facilitates the explanation of why certain regularities do not actually occur, at least if the assumptions of the models from which the information is gathered do not differ too much from one another. What remains true on this conception, however, is that *explanations of actually observed regularities are neither necessary nor sufficient for understanding a mechanism*. They are not sufficient, because understanding requires knowledge of how the mechanism operates not in one but in several circumstances. They are not necessary, because the explanations concern the absence of regularities and not their presence.²¹

4. Conclusion

The key observation around which the arguments in this paper revolve is that the regularities encapsulated in economic models are rarely observed in practice. As a consequence, many economic models, as well as some models in other sciences or disciplines, provide would-be explanations at best if explanation is explicated in the ways Hausman and Cartwright advocate. I have argued that the method of explanation by relaxation provides a better account of the explanatory practices in significant parts of economics than accounts that posit regularities as explanantia or explananda. On this method, the observation mentioned does not deprive the models at issue of all explanatory value. This is because a highly unrealistic model can be used in combination with a slightly more realistic model to answer why the key implication of the original model contrasts with what we see in practice. The basic model captures the behavior of a basic mechanism, one that is isolated from interfering factors. Those interfering factors can be invoked in order to explain why what is actually observed deviates from this. Even though no observed regularities are explained, the models serve to make sense of the absence of particular regularities that could be observed. Thus, explanation by relaxation allows us to see how “false models” can be used to provide true explanations.

In addition to this, I have argued that such models also provide the (or at least a) key to understanding the way in which mechanisms operate. Crucial to such an understanding is knowledge of

how a mechanism operates in the absence of interfering factors. What is needed in addition to this is knowledge of how its functioning is affected by some of those factors. This is where would-be explanations turn out to be of some use after all, as they account for the (unobserved) regularities that would arise if (only) those factors were to affect the way the mechanism functions. Thus, the process of constructing more realistic models by relaxing assumptions of a basic model not only has (some) explanatory import but also (substantially) contributes to our understanding of the way mechanisms work. What is more, such understanding is independent of the explanation of actually observed regularities.

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²¹ Khalifa (2012) argues that recent accounts of understanding do not add anything of significance to existing accounts of explanation. However, his argument works only for understanding that is provided by explanations. As I argue that understanding can be provided in situations in which explanations that fit these accounts cannot be provided, my proposal concerning understanding survives his criticisms.

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