

UNIFORMITY ASSUMPTIONS AND THE MUSGRAVE MÄKI-HINDRIKS DEBATE

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Abstract

In this paper I address the problem of the unrealisticness of assumptions in neoclassical economics². Being accused of using highly unrealistic assumptions in its models, neoclassical economics replied through what later was called the *F-twist*. Shortly stated, it was claimed that descriptively unrealistic assumptions are ubiquitous in other sciences also, and that economics should be concerned with its predictions instead of its assumptions. The immediate implication of this statement was that all unrealistic assumptions are the same – they are harmlessly unrealistic. Philosophers of economics vivaciously debated this claim and argued that economics made use of several kinds of assumptions which "*had better be true*"³. Building on this debate I introduce the notion of uniformity assumptions and I argue that in certain conditions they "*had better be true*".

Keywords: unrealistic assumptions, uniformity assumptions, neoclassical economics.

Unrealisticness of assumptions

Neoclassical economics faced a long list of charges in its history from "*then*" to "*now*". Leontiev (1971) argued that it became a "*mathematical model building industry*" (Leontief, 1971: 2). In the same respect, Solow (1997) noted that it displays a tendency of theorizing at the expense of data, because "*theory is cheap, data are expensive*" (Solow, 1997: 57). Coase (1992) labelled it as "*a system which lives in the minds of economists but not on Earth*" (Coase, 1992: 714) or as "*blackboard economics*" (Coase, 1992: 714). Blaug (2002) diagnosed it as "*being*

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² This includes also neoclassical economics extensions into other domains as mentioned by Stigler (1984): the economics of law, the new economic history, the economic analysis of social structure and behavior and the economic analysis of politics or public choice theory. (Stigler, 1984: 303)

³ These are Musgrave's (1981) words.

sick" (and)⁴ *increasingly becoming an intellectual game played for its own sake and not for its practical consequences*". (Blaug in Maki, 2002: 36). All these accusations could be translated into one problem, the unrealisticness of the assumptions of the neoclassical economics.

This problem had already received a much celebrated answer from Milton Friedman. In (1966, 1966) Friedman talked about the "*realism of [...] assumptions*" (Friedman, 1966: p.14) and argued that economics should be concerned with its predictions not with the descriptive unrealisticness of its assumptions because "*they never are*"; (they should only be) "*sufficiently good approximations for the purpose in hand*" (Friedman, 1966: 15). So models should be tested regardless the degree of unrealisticness of some of their particular assumptions. To argue for this idea, Friedman gave an example from physics, the law of falling bodies. The accepted hypothesis that the acceleration of a body dropped in a vacuum is a constant g , could be applied to various real life situations, let's say, dropping a cannon ball from the Pisa tower. The ball would fall as *if* there was a vacuum. From this, Friedman argued that "*the formula is accepted because it works, not because we live in an approximate vacuum - whatever that means.*" (Friedman, 1966: 18) So the unrealisticness of an assumption did not impede with the models prediction quality. As mentioned in the abstract, that implied that unrealistic assumptions were harmless – they were just useful fictions and we should focus on the outputs of our models not on their inputs.

In (1981) Musgrave noted that in the *F-twist* "*[...] Friedman defended the apparently absurd view that `unreal assumptions` are not a vice but virtue.*" (Musgrave, 1981: 377)" This argument "*sparked a vigorous controversy which has continued right up to the present day*" (Musgrave, 1981: 377). Starting from Friedman's argument Musgrave argued that Friedman failed to "*distinguish between three different types of assumptions, each of which makes a different type of assertion and therefore plays a different role in a theory*" (Musgrave, 1981: 378) So, starting from the different functions that assumptions could hold in a model, Musgrave identified the negligibility assumptions, the domain assumptions and the heuristic assumptions. The first type, the negligibility assumptions concern the impact of a factor F "*which might be expected to affect (a) phenomenon (but) actually has no effect upon it, or at least no detectable effect.*" (Musgrave, 1981: 378). The vacuum assumption from Friedman's example of falling bodies could be an example of such assumptions: "*the effect of air resistance is negligible*" (Musgrave, 1981: 378) Of course not all the factors not stated in a model are in fact negligibility assumptions. Musgrave assumed that we did not

⁴ The paranthesis is mine. I will use this rule from now on.

usually have explicitly stated assumptions about the negligibility of factors like the day of the week on which the experiment was performed or about the experimenter's eyes color. "*Negligibility assumptions are stated only for factors which might be expected to have some effect but which, we claim, will not.*" (Musgrave, 1981: 378) The second class, the domain assumptions specify the domain of a theory. Musgrave formulated this as a second step assumption: "*What begins as a negligibility assumption, when it gets refuted, turn into a domain assumption*" (Musgrave, 1981: 381). So if a negligibility assumption that a factor *F* has negligible effect is proved to be false, the domain of the theory may be restricted. The researcher "*may retain the assumption that F is absent, but now use it to specify the domain of applicability of his theory*" (Musgrave, 1981: 381) Musgrave argued further that this radical change in a given theory could "*go unnoticed because the same form of words is used to express both assumptions. An economist who says 'assume the government has a balanced budget' may mean that any actual budget imbalance can be ignored because its effects on the phenomena he is investigating are negligible. But he may also mean precisely the opposite: that budget imbalance would have significant effects, so that his theory will only apply where such an imbalance does not exist*" (Musgrave, 1981: 381). Finally, the third class, the heuristic assumptions, designates a two stage approach. "*When a scientist finds that his domain assumption are never true (if it is to have a testable theory) [...] he must take into account the factor F whose presence he took first to be negligible and then to limit the domain of applicability of his theory. But he may wish to develop such a theory in two stages: in the first stage he takes no account of factor F, or assumes that it is negligible; in the second stage he takes account of it and says what difference it makes to his results. Here the assumption that factor F is negligible is merely a heuristic device, a way of simplifying the logical development of the theory*". (Musgrave, 1981: 382,383).

In (2000) Mäki labeled Musgrave's (1981) taxonomy of assumptions as successful "*because it clarifies certain aspects of the assumptions controversy*" (Mäki, 2000: 317) and argued that further clarifications were needed. First of them concerned the negligibility assumptions and Musgrave's formulation of it. Mäki noted that what was surprising about Musgrave's definition "*is that it does not mention the idea of negligibility at all; it talks about detectability*" (Mäki, 2000: 319). Further, Mäki pointed out that "*a negligible effect does not also have to be undetectable. [...] negligibility presupposes detectability: in order to rationally judge whether an effect is negligible, one has to be able to measure it! [...] This means that [...] Musgrave has actually characterized a separate type of assumption*" (Mäki, 2000: 320). Another problem was

that a factor which had no effect was not a factor with a negligible effect. Mäki argued that “*while negligibility is a context-specific notion [...] there is no context-specific [...] about a factor having no effect*” (Mäki, 2000: 322). So, a no-effect factor was irrelevant not negligible. Therefore we got an adequate definition: “*A negligibility assumption is the hypothesis that some factor F that might be expected to affect the phenomenon under investigation actually has an effect upon it small enough to be neglected relative to a given purpose*” (Mäki, 2000: 322). Starting from this new definition Mäki argued like Musgrave that “*It is good for the theory – including its predictive abilities – if they (the negligibility assumptions) are true*” (Mäki, 2000: 322). So the F -twist must be untwisted in the case of negligibility assumptions. The second clarification concerns Musgrave’s domain assumption. Mäki argued that there was a problem with Musgrave’s domain assumptions because their identity was not clear. “*It is not quite clear whether the relevant domain assumption is intended to be the non-paraphrased statement DB : ‘the government has a balanced budget’ or the meta level paraphrase DB^* : ‘the theory only applies where budget imbalance is absent’*” (Mäki, 2000: 324). Reading between Musgrave’s lines, Mäki decided that he intended to talk about DB . Starting from this observation, Mäki suggested that actually it was DB^* that would better express a domain assumption. From this, in order to have a domain assumption in Musgrave’s spirit (but not in its form) we must have a statement about reality (*the government has a balanced budget*) and a meta-statement (*the theory applies only where ...*). At this point Mäki makes a distinction between domain assumption (first kind of statement) and applicability assumption (the second kind). For example: “*the assumption that the agents’ behavior is solely calculative and self-seeking may serve as a domain assumption, while the assumption that that assumption applies to market behavior but not – or alternatively: as well as – to political or family behavior, is an applicability assumption*” (Mäki, 2000: 325). Finally, Mäki analyzes the third of Musgrave’s assumptions, the heuristic ones. They should be understood in a dynamic context and could be labeled as first step assumptions or early step assumptions. Actually what we have is an assumption and a promise of forthcoming relaxation the form of the following sentences: “*(B): ‘The government has a balanced budget’ (and) E - SB : ‘(B) is an element in an early formulation of the theory and will be removed as the theory is developed’*” (Mäki, 2000: 326). So what we need is to acknowledge the necessity of a meta-statement as in the case of the domain-applicability assumptions. From the preceding clarifications, Mäki critically argues that even though the first two kinds of assumptions „*had better be true*”, in the case of early step assumptions, Musgrave’s conclusion does not hold. His argument goes something like the following: first, an early step assumption

involves a false negligibility assumption. Second, this negligibility assumption must be supplemented with a promise that the falseness will be removed in a later step. From this, Mäki argues against Musgrave's conclusion that negligibility assumption needs to be true. If the first step needs to be true then it would be incoherent to say that in a later step the false negligibility assumption will be relaxed to a true one. I believe this argument aims to a potential problem with Musgrave's conclusion, rather than to an actual one. As Mäki acknowledged, Musgrave's statement of the heuristic assumptions was not very clear. Another reading of this type (other than Mäki's) of assumption could be that if there is an assumption with heuristic value, then in the first stage we could employ a false negligibility assumption, while in the second stage we could not make another false negligibility assumption. I believe this is the correct reading of Musgrave's intentions. Two stages of false negligibility assumptions do not make much sense, and Musgrave does not seem to be unable to understand that.

In (2005), (2006), Hindriks analyzed the Musgrave-Mäki *battle of assumptions* and made several corrections. Hindriks (2005) noted that Mäki's suggestions and changes in Musgrave's typology must be seen as improvements in need for further amendments. First, Hindriks distinguished between first order and second order assumptions. A first order assumption is, for example, one that assumes that "*a certain factor F has no effect on the phenomenon under investigation*" (Hindriks, 2005: 391). A second order assumption on the other hand, "provides reasons for imposing first-order assumptions" (Hindriks, 2005: 391). In a later (2006) formulation, these assumptions are described as explaining "*the purpose for which or the reason why an (a first order) assumption is imposed [...] such assumptions elucidate the roles that particular first-order assumptions play*". (Hindriks, 2006: 407). Starting from these definitions, Hindriks argues that they describe a new framework for analyzing the assumption issue. This allegation is not entirely true though. Mäki made a similar distinction regarding applicability assumptions and early step assumptions (as noted above). He contended that these types of assumptions were meta-statements about the applicability and about the future development of an assumption. That implied that Mäki also implicitly formulated a (restricted at two types of assumption) two-order approach to assumptions. In this view even though Hindriks' statement about the novelty of his approach is somewhat exaggerated, the framework he described is useful for a better understanding of the assumptions issue. In this framework he advocated that "*assumptions such as negligibility and applicability are second-order assumptions*" (Hindriks, 2006: 407). This move leaves us with a first order assumption and two different reasons (which should be

explicitly stated) for assuming it. Further, Hindriks did a second amendment to the Musgrave-Mäki *battle of assumptions*. He replaced the heuristic assumptions with what he called tractability assumptions. The latter are statements “*about the tractability of a problem. The idea is that a problem would be intractable if it were not for a particular first-order assumption*” (Hindriks, 2006: 410). Stated in more of Hindriks’ words, “*tractability is a matter of solubility or of the efficiency of a solution. A problem is intractable if it cannot be solved; a problem is more tractable with a certain assumption than without it if it can be solved more easily or efficiently in that case*”. (Hindriks, 2005: 392). Hindriks further divides tractability assumptions into theoretical and empirical. “*Problem P may be the problem of how to apply a certain theory T to a particular situation. An assumption that is imposed in order to solve such a problem is an empirical tractability problem. An assumption that is needed for solving a problem that is independent from the application of the theory is a theoretical tractability assumption*” (Hindriks, 2005: 392). His argument for the replacement of heuristic assumptions with tractability assumptions is that of rationalizing after the fact: “*The process of theory development can rarely be planned this way*” (as a second step) [...] *The claim that a first-order assumption will turn out to have heuristic value can at most be an educated guess at the time it is imposed. In such cases it will often be more convincing to state that the main reason for imposing the assumption was tractability, perhaps combined with a hope of being able to do without it one day*”. (Hindriks, 2006: 415,416). Regarding Mäki’s early step assumptions, Hindriks contention was that they missed the heuristic point originally given by Musgrave and that it did not make sense to impose a first order assumption and remove it later. In his words, “*it simply does not make sense to say that a first-order assumption is made so that it can be removed later [...] if the assumption is to be relaxed later on, why impose it in the first place?*” (Hindriks, 2006: 417). Hindriks’ conclusion was that the purpose of explaining why economists imposed false assumptions was better served by his notion of tractability assumptions. In his view, this notion should replace heuristic/early step assumptions. Regarding the problem of falsity from definition, tractability assumptions are forced steps in model building. If Newton had the mathematical apparatus needed for his theory, then he could have done without his “*one planet*” assumption. So a tractability assumption is a false assumption we hope to remove in the future. Being forced false assumptions, we cannot expect them to be true (as we can with negligibility and applicability assumptions).

The role of assumptions' effects in building a typology

In this section I have two goals. First, I argue against Mäki-Hindriks' typology of assumptions that the second order assumptions are in most cases inoperable. Second, I argue that the reasons for using a certain assumption are not always specified and it could be more fruitful to concentrate on assumptions' effects in the model world.

Though I am not a trained philosopher, it is my belief that the philosophy of science should be concerned with the operability of its concepts. This is the ground on which I build my critique of Mäki-Hindriks' second order typology of assumptions. I think that concepts should display the property of operability. They should be able to be transformed in a way that allows us to use them in analyzing scientific theories. My contention is a mild one: I won't say that inoperable (or less operable) concepts are useless; I will only say that it is better to work with concepts we are able to use to make sense of scientific practice. I believe that scientific practice, in general, lacks meta-statements about the reason of employing certain assumptions. If we search only for these meta-statements we won't be able to conclude much about those reasons⁵. If we want that negligibility, applicability, domain, heuristic, early step or tractability assumptions to matter we must not think of them as only second order assumptions. Returning to Musgrave's example, "A: *The government has a balanced budget*", I think that there are two distinct ways to avoid the lack of operability of Mäki-Hindriks proposal. The first is returning to Musgrave original method of identifying the function an assumption have without a meta-statement the kind of "assumption A is made because...". The second, and I favour this approach here, is to focus on the effects that a certain assumption have in the model world. So, in the first case if we get lucky and find a meta-statement, then we are done. If the meta-statement is missing, we could employ Musgrave's method. Of course, this method has its shortcomings. Guessing about the reasons is not an easy and exact enterprise. In addition, even though Musgrave (1981), Mäki (1994), (2000), (2002), Hindriks (2005), (2006) talked (implicitly or explicitly) about the functions that assumptions have in a model, one thing was never clear (at least to me) in their analyses. They never talked (at least not explicitly enough) about the possibility of joint functions. An assumption can have more than one function in a model. One can buy apples for their taste and in the same times because of their vitamins. One can employ the same false

⁵ I think this is the reason Musgrave never imposed the meta-statement clause. His method of identifying the type of an assumption in a given model does not request a meta-statement and it is something resembling guessing about whatever functions assumptions might have in a certain model world.

assumption by more than one reason. This seems to be neglected by Musgrave-Mäki-Hindriks typologies. Taking an example, from the realm of a neoclassical political science⁶, Niskanen's (1975) budget maximizing assumption from its bureaucracy models, could be interpreted in several ways: First, we could have a function of negligibility: "*other reasons for action than budget maximizing can be neglected*". Second we could have a function of tractability: "*budget maximizing assumption makes the problem tractable*". Third we could have joint functions of negligibility and tractability (there is nothing to prevent us to have more than one reason for using a certain assumption). The same goes with the neoclassical principle of maximization. It has tractability functions, but in the same time it might be read as a negligibility assumption. The possibility of joint functions adds a little more mess to the job of identifying the reason for employing unrealistic assumptions. This is why I choose to focus on the obvious effects that assumptions have in a model world. By an effect of an assumption I mean a result that employing a certain assumption could have. This result could be deliberate or non-intentional. Suppose that the only reason to employ the budget maximizing assumption is tractability. In the same time, we will have the effect of negligibility because, for the reasons of tractability, we will disregard any causal power of other (than budget maximization) motivations for action that bureaucrats could have. Scientific practice may not offer enough information to identify functions, but the effects of making a certain assumption are always visible. The most obvious effect that some assumptions achieve in neoclassical models is that of uniformity. This effect is achieved by what I will label as uniformity assumptions. The reasons may be those of tractability or of negligibility or applicability, but the reasons are not my primary interest here.

To further discuss the class of uniformity assumptions, it is necessary to mention another typology which started with Machlup's (1955) paper. This involves a hierarchization of assumptions by their importance. While in Musgrave's taxonomy all assumptions are at the same level of importance, in Machlup-Mäki's (and others) taxonomy, assumptions are ordered by their significance. Machlup distinguished between fundamental assumptions (for example Newton's three laws of motion, or neoclassical economics' rationality postulate) and specific assumptions (for example, that there is a quota for the importation of sugar which is fully utilized). A similar (hierarchical) approach was

⁶ Niskanen's work on bureaucracy is a part of the public choice theory research program. As Tollison argued „*public choice emerged from the maximizing paradigm of modern microeconomics, and it remains to this day within that approach*” (Tollison in Rowley, Schneider, 2008: 192).

proposed, more recently, by Mäki (1994), (2000). He wrote about types of assumptions and their function and distinguished between core assumptions and peripheral assumptions. Core assumptions “serve to sort out what is believed to be the most central force [...]” (Mäki, 1994: 244); for example, Galilei’s statement that bodies were attracted by the gravitational field of the Earth, measured by parameter g , or the assumption that agents maximized). Peripheral assumptions “serve to neutralize factors that are not regarded as central or essential to the phenomenon” (Mäki, 1994: 244); for example, Galilei’s vacuum assumption, or neoclassical economics’ perfect divisibility of goods assumption. In (2000) Mäki argued that Musgrave seemed to limit his analysis to peripheral assumptions and that that was not a legitimate move: “both core assumptions and peripheral assumption can in principle serve as negligibility, applicability and early step assumptions⁷” (Mäki, 2000: 329).

The notions of fundamental/core and peripheral/specific assumptions are useful here in two ways. The first issue is about Hindriks’ tractability assumptions. The second, and my main interest here, is about uniformity assumptions. I will discuss them consecutively. Hindriks’ (2005) (2006) papers are mainly concerned with explaining why economists use unrealistic assumptions. As I already noted, the reason Hindriks identified, was the new class of tractability assumptions, defined as legitimate forced, hopefully temporary, falsities. The first point I want to make is that if the concept of tractability has in its core the idea of manageability of a problem, then we could think about the possibility of realistic tractability assumptions. From the class of assumptions we could make by reasons of tractability, we should choose realistic ones. Of course if realistic tractability assumptions were the rule of scientific practice, there would have been no reason for Hindriks to write the two papers. My point is just that the concept of tractability is consistent with realisticness of assumptions. This being said, there are two other problems about tractability which I want to address. The first of these is about the meaning of tractability, while the second is about its domain. Regarding the meaning, my first reading of tractability assumptions was in terms of “*how to best solve a problem*”, with the path to solution left empty. My second reading was in terms of “*how to solve a problem in a certain way*”. The path to solution is, in this later reading, specified. I think the second reading is in Hindriks’ spirit. All of his examples are about tractability in a certain way. In (2010), Kuorikoski, Lehtinen and Marchionni, explicitly label Hindriks’ tractability assumptions as

⁷ Later on the same page Mäki weakens his statement: “*core assumptions seldom function as early-step assumptions in a sequence of models within a given framework of analysis: they are the stable ‘all-step’ assumptions*”. (Mäki, 2000: 329)

mathematical tractability assumptions: “*Some modeling assumptions are thus introduced only for reasons of mathematical tractability (see Hindriks [2006]).*” (Kuorikoski, Lehtinen, Marchionni, 2010: 547). So Hindriks’ tractability assumptions are a subset of what could be called methodological biased tractability. They specify a path to solution. The second problem, connected to the antecedent one, is with tractability assumptions’ domain. It is difficult to figure out if these falsities could appear (in Hindriks’ view) in the case of core assumptions. Hindriks’ definition of tractability and his examples seem to be about peripheral assumptions. I think that there is nothing to prevent us to talk about tractability of the core assumptions. If we make this step (which I am not sure Hindriks did) then we should have the new class of methodologically biased tractability core assumptions and a subset of this class, mathematical tractability core assumptions. By Hindriks’ definition, these must be false core assumptions made by reasons of mathematical tractability. The problem, as I see it, is what role these assumptions have in bringing the result. I will modify here Mäki’s definition of core assumption from “*what is believed to be the most central force*” to *what is pervasive to models from a discipline*. Mäki’s definition is clearly a realist one and the realist bias is not of great use here. If core assumptions are ubiquitous assumptions (without commitment to their causal force), then if they happen to be mathematical tractability assumptions also, then they must be totally robust. I will elaborate this idea later.

Turning back to uniformity assumptions, these are effects rather than reasons. When we look at neoclassical economics’ (and Public choice theory’s) model world, we see different kinds of uniformity. The reasons are less important (even though I think mathematical tractability is the main reason), the important thing is that uniformity is the rule. Building on Machlup-Mäki typology, uniformity may be local or global. By global uniformity assumptions, I have in mind a class of assumption which achieves uniformity across a discipline. My main example of global uniformity assumption is the principal behavioral assumption of neoclassical economics, *homo economicus*. By *homo economicus* I refer to an agent who maximizes (formal definition of rationality) his welfare (the self-interest operationalization of the formal definition). This assumption achieves behavioral uniformity across all discipline’s domain. By local uniformity assumptions I refer to assumptions that achieve uniformity at local level or at a certain model’s level. For example, the budget maximizing assumption achieves uniformity in Niskanen’s model world: all bureaucrats are the same. Similarly, the bilateral monopoly assumption achieves uniformity inside the model: the sponsor and the bureau chief are in the same relation across the entire

model world. The reasons may be those of negligibility or of tractability (there is no second step assumption in Niskanen's model), or both but the meta-statements needed to establish this are missing. What is sure is that we have assumptions which achieve uniformity. Global and local uniformity assumptions both achieve context invariance. No matter the context, the discipline world and the model world are uniform. This uniformity is not a bad thing per se, but it might be a problem if the real world is not really uniform. I think uniformity assumptions *had better be true*. If not, they *had better be* at least robust. I will develop this idea in the following section of the paper.

Uniformity assumptions: Discussion

Uniformity assumptions are not suspicious per se. To assume, for example, that objects fall *as if* there were a vacuum is to say that the nature is uniform in respect to this characteristic and it is not a problematic assumption. The same goes, for example, for assuming behavioral uniformity under the individualistic model of prey-predator described by Weisberg and Reisman in (2008): the model will still display the Volterra property. To assume, as neoclassical economics does, that all individuals are the same: instrumental, self-interested maximizers is, though, another thing. This obvious psychological unrealistic global uniformity assumption seems to be problematic at least in some circumstances which I will clarify in the following few lines.

In (2013) I performed robustness analysis on Niskanen's models of bureaucracy. I argued there, that those models were sensitive to some of their assumptions *i.e.* they were not robust with respect to those assumptions. I also argued that those assumptions had a uniformity effect in the model world but I did not fully explore the meaning of uniformity assumptions⁸. Shortly stated, by robustness analysis I meant a non-empirical procedure of building models by incrementally modifying a certain assumption. Starting from that notion, I argued that if the result was constant under different specifications of a certain assumption, then that assumption was in, what I called, the causal core of the model. With these in mind we could return to the problem of the utility maximization global uniformity assumption (and its uniformity operationalized assumptions) from neoclassical economics. If this assumption is in the causal core of model then it *had better be true* at least in a behavioral way⁹. So the circumstances mentioned above are connected to the

⁸ This task was assumed here.

⁹ The distinction made here is between psychological realistic assumptions and behavioral realistic assumptions. If utility maximization is interpreted as a psychological assumption then it is certainly unrealistic. If we only assume that people (no matter their

problem of robustness of results under different uniformity assumptions. If they are not in the causal core of the model then their unrealisticness is unproblematic. A good example is Becker's¹⁰ (1962) robustness analysis performed on the traditional model of household behavior. Replacing utility maximization with irrational (biased or random) preferences yields the same result. In my terms this means that the assumption was not in the causal core of the model and that its uniformity was not problematic. Concluding, my uniformity assumptions *had better be true* only when they have causal force in the model. This causal force could be traced by the help of robustness analysis.

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psychological processes) only behave *as if* they were utility maximizers then we might have a behavioral fairly realistic assumption.

¹⁰ Of course, in (1962) Becker did not use the concepts of robustness or causal core.

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