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## A Pragmatic Reading of Friedman's Methodological Essay and What it Tells us for the Discussion of ABMs

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## **A Pragmatic Reading of Friedman's Methodological Essay and What it Tells us for the Discussion of ABMs**

Abstract: For the recent methodological discussion in agent-based modelling Friedman's points are a helpful starting point by broadening the methodological perspective: Setting up ABMs allows the modellers to get rid of the restrictive assumptions characterizing traditional modelling approaches in economics. However, does this necessarily mean, that the use of assumptions to reduce the complexity of real world economic processes is no longer allowed? Or do we overstress the capacity of ABMs by requiring as much empirical foundations as possible?

Building on the pragmatic interpretation of Friedman's essay, we strongly support the building of abductive models. However, in our view this does not necessarily mean to include as much data as possible when setting up the assumptions, as this surely leads to a loss in generality and hence reduces the ability of an ABM to explain. When the explication of basic economic structures is the aim, ABMs can deliver their full strength only when they are relatively simple, so that the mechanics of the model can be fully understood.

Keywords: KISS, KIDS, Agent-based modelling, validation, assumption  
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### **1. Introduction**

In the most recent literature on agent-based modelling the issues of empirical calibration of parameter values and functional relationships describing the interactions between the various actors plays an increasing

important role. Agent-based models (ABMs) range from purely theoretical exercises focussing on the patterns in the dynamics of interactions processes to modelling frameworks which are oriented closely at the replication of empirical cases (e.g. history friendly models (Malerba, Nelson, Orsenigo and Winter, 1999)). In this discussion Brenner and Werker (2007) proposed a taxonomy suggesting to classify ABMs in terms of their generality and their use of empirical data. In their conclusion they recommend aiming at maximizing both criteria by building what they call “abductive models”. Interestingly, they explicitly recommend to “include as much data as possible when setting up the assumptions”.<sup>1</sup>

This is almost the direct opposite of Milton Friedman’s famous and provocative methodological credo “the more significant a theory, the more unrealistic the assumptions”.<sup>2</sup> Most methodologists and philosophers of science have harshly criticised Friedman’s essay as inconsistent, wrong and misleading. By presenting arguments for a pragmatic reinterpretation of Friedman’s essay, we will show why most of the philosophical critique misses the point. After that, we will use the developed arguments for contesting the claim that good simulations have to rely on descriptively adequate assumptions.

## 2. A pragmatic reinterpretation of Friedman’s methodology

Even more than 55 years after its original publication Friedman’s methodological essay still is *the* classic among all methodological texts for economists. As Daniel Hausman has stated, it is probably the only methodological work that a fair amount of economists has ever read.<sup>3</sup> More philosophically minded readers have usually rejected it as inconsistent,

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<sup>1</sup> Brenner/Werker (2007), p.242.

<sup>2</sup> Friedman (1953), p.14.

<sup>3</sup> See Hausman (1992a), p.162.

vague or false.<sup>4</sup> The most commonly held view reduces Friedman's essay to the point that the assumptions of a theory do not matter because all we should expect from economics is good predictions. This is a grossly misleading interpretation, as we will show.

Before constructively developing our pragmatic interpretation of Friedman's methodology, we will deal with some of the best-known criticisms of his approach.

Daniel Hausman's essay "Why look under the hood" is a paradigmatic example of critique of Friedman's essay.<sup>5</sup> His main argument against Friedman's approach goes like this:<sup>6</sup> Friedman claims, that the assumptions underlying a model are irrelevant and all that *is* relevant is predictive success. Hausman tries to spot an error in this claim by providing an analogy: Suppose you want to buy a used car. According to Hausman, Friedman would say that the only relevant test was checking whether the car drives safely, economically and comfortably. Looking under the hood and checking the status of the components is not necessary. Now it is obvious, that no one would buy a car without looking under the hood. In analogy to that, we should check the assumptions of theories as well and not merely rely on predictive success as the only criterion, Hausman concludes. He takes this to be an argument against Friedman's position.

This critique is typical for a class of accusations made against Friedman's case. The accusations are however attacking a straw-man, because a more thorough reading of Friedman's essay easily shows, that he does *not* think the assumptions of a theory to be irrelevant at all. The error in Hausman's argumentation can be made clear by the following comparison: Hausman grants later in the text, that modelling always involves

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<sup>4</sup> We do not deliver an introductory summary of Friedman's essay as it is hard to summarize it in a neutral way due to the huge amount of different interpretations available in the literature. The pragmatic interpretation that we develop in this text is therefore not the only one consistent with the text, but in our view, it is the one that fits best Friedman's general point, even if it has to live with some inconsistencies.

<sup>5</sup> See **Hausman** (1992b), p.70-73.

<sup>6</sup> See **Hausman** (1992b), p.71.

simplification, which is why the assumptions do not need to be perfectly true, but can be “adequate approximations [...] for particular purposes”.<sup>7</sup> Ironically, in Friedman’s essay one passage states just that: “the relevant question to ask about the “assumptions” of a theory is not whether they are descriptively “realistic,” for they never are, but whether they are sufficiently good approximations for the purpose in hand.”<sup>8</sup> This should make clear, that Friedman does not think the assumptions of a theory to be irrelevant. His aim is rather pointing to the deficits of a wholesale demand for more realistic assumptions in economics. Friedman takes the contrary position: Model building requires simplification and good models can “explain much by little”.<sup>9</sup> This is why interesting models must rely on assumptions, which are descriptively false and making them more realistic does not lead automatically to better models. Modelling is different from mere abstraction, it necessarily involves construction and is not just extracting parts from reality. These are all well accepted arguments supporting the view that it can be all right to use unrealistic abstractions. What neither the above arguments nor Friedman are implying, is the view that all unrealistic arguments lead to good models.<sup>10</sup> His point is rather, that some unrealism is necessary and even a plus if it is unrealism of the right kind.

Friedman’s talk about unrealistic assumptions has led to much contradiction and heavy confusion. It is indeed Friedman’s fault that he did not formulate his thesis very carefully. He talks very generally of “unrealistic assumptions”, which is problematic, because both terms “unrealistic” and “assumption” can be understood in many different ways.

In a famous article Alan Musgrave criticised Friedman’s position by distinguishing three kinds of assumptions and trying to show that in all

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<sup>7</sup> Hausman (1992b), p.72.

<sup>8</sup> Friedman (1953), p.15.

<sup>9</sup> Friedman (1953), p.14.

<sup>10</sup> Sometimes it seems that this view is attributed to Friedman even if it is obviously absurd. Such critics seem to forget that Friedman is accepting only those assumptions that lead to correct predictions. Besides that, it is a simple logical error to conclude from Friedman’s “the more significant a theory, the more unrealistic the assumptions” to the statement that unrealistic assumptions lead to significant theories.

three cases assumptions must be rather realistic than unrealistic.<sup>11</sup> With negligibility and domain assumptions Musgrave's argument seems convincing at first sight: The colours of the traders' eye *are* negligible, at least in the strict economic domain of analysing the stock-market. In this sense, a model that "assumes away" the influence of eyes' colours to stock-prices can be called realistic rather than unrealistic. However, Musgrave's argument is more a twist with words than a real refutation of Friedman's position. The negligibility of eyes' colours *can* be called realistic, because it *really* has no effect on the stock market, or unrealistic, because the traders in reality *do* have coloured eyes. Friedman tends to the latter view, but stresses the point, that it is not the "realisticness" of the assumptions that matters, but the implications they yield. From this perspective, it does not matter if a model that assumes away eyes' colours is called realistic or unrealistic, because, again, it is not the realisticness<sup>12</sup> of the assumption that counts, but if they are the *adequate* assumptions for the modelling purpose. This is probably the main point of misunderstanding between Friedman and many critics (take Hausman again): Friedman recommends evaluating the assumptions only for specific purposes, whereas his critics aim at *broad* predictive success. The question is however, if broad predictive success is achievable at all. Friedman holds the view that it is not – there is no "theory of everything", so narrowing the domain of theories is always necessary.

The third class of assumptions Musgrave distinguishes he calls "heuristic assumptions" which are employed when there is *no* domain where a factor that is assumed away is negligible indeed. The "heuristic assumptions" are rules for simplifications that guide researchers and tell them how to proceed, if a theory does not fit to the data. Musgrave states "At any rate, his central thesis 'the more significant the theory, the more unrealistic

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<sup>11</sup> See **Musgrave** (1981).

<sup>12</sup> "Realisticness" is a term introduced by Uskali Mäki in order to distinguish descriptively accurate assumptions from the philosophical position of realism. See e.g. **Mäki** (1998).

*the assumptions' is not true of 'heuristic assumptions' either."*<sup>13</sup> It is hard to see how Musgrave wants to judge the realitsticness of a heuristic assumption, if he accepts that they are untestable. Heuristic assumptions are rules for simplification, they are *intentionally* unrealistic, they *define* the focus of a research programme. From this perspective it seems hard to decide if they are realistic, *before* looking at the specific models (and their implications) that are based on them. With heuristic assumptions, the question is not whether they are true or false, but whether they are able to generate fruitful lines of research.<sup>14</sup> Musgrave's distinction between three classes of assumptions is certainly a brilliant addition to the assumption-debate, but it does not refute Friedman's position, as it fails to show why seemingly implausible heuristic assumptions such as the rationality assumption are always nothing but an error of a theory.<sup>15</sup> As the two other types of assumptions go, Musgrave seems to attack rather a straw-man than Friedman's position (at last in the pragmatic interpretation): Nowhere does Friedman say, that making false negligibility or domain assumptions helps generating significant theories. He says that significant theories are mostly based on unrealistic assumptions, not that any unrealistic assumption creates a significant theory! When Musgrave stresses that wrong negligibility and domain assumptions usually lead to bad theories, this can be interpreted as stressing Friedman's point that the empirical correctness of the implications is relevant for judging assumptions: As soon as wrong negligibility and domain assumptions lead to wrong predictions (which they most probably do quite fast), they are immediately ruled out. If one takes the ability to predict serious, one cannot come up with wrong negligibility or

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<sup>13</sup> **Musgrave** (1981), p.385.

<sup>14</sup> See **Mäki** (2000), p.326.

<sup>15</sup> Of course, it is *sometimes* not fruitful to assume rational behaviour. E.g. modelling situations that involve decisions under uncertainty or trying to analyse innovation processes that require creativity are not likely to be fruitfully reconstructed by rational choice models. However, as there *are* situations that *can* be reconstructed by rational choice models, we cannot judge this methodological principle as such on grounds of its implausibility, but only the aptness of a concrete application of the principle.

domain assumptions.<sup>16</sup> The case is different with heuristic assumptions, as they are neither directly comparable to reality nor do they lead directly to empirical implications at all. Here it seems still correct, to allow for assumptions that seem *prima facie* implausible or unrealistic. Musgrave has done a great job at pointing out what Friedman *cannot* mean by unrealistic assumptions, but as we see it, he has failed to refute him. At no point in his essay, Musgrave delivers an argument why it should be false to claim that significant theories often rely on seemingly unrealistic heuristic assumptions.<sup>17</sup>

After having discussed the term “assumption” at some length, let us turn to the other term “realistic”. As we have seen, Friedman claims that unrealistic assumptions are not a disadvantage of a theory *per se*. So there must be assumptions that are unrealistic in some sense, but still good ones for the purpose in hand. The following three cases show different interpretations of “unrealistic” that meet this criterion:

1. In a trivial sense *all* assumptions are wrong, because they are necessary incomplete. It is not possible to deliver an objective and complete description of the observable world.
2. Apart from the incompleteness, assumptions can be “unrealistic” in a different sense: As models propose hypotheses of causality, they must contain more than what is directly observable, as causal relations are not. This is why assumptions cannot be descriptively realistic in the sense of photographic depictions of the observable world.<sup>18</sup>

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<sup>16</sup> Marcel Boumans adds that Friedman encourages empirically exploring the domain of a negligibility assumption. See **Boumans** (2003), p.320.

<sup>17</sup> Musgrave takes Newton’s reduction of planets to mass-points as an example for a heuristic assumption, but instead of refuting Friedman’s claim this seems rather to confirm his view that significant theories are often based on unrealistic heuristic assumptions.

<sup>18</sup> This statement does not touch the philosophical position of scientific realism, which it locates in the realm of *theories* about causal connections. Correct causal assumptions would then be realistic theoretical entities. The above argument is headed against a more naïve form of realism which identifies realism with a one-to-one correspondence to observation.

3. In a third sense assumptions could be called unrealistic, when they contradict common sense. This is the case e.g. with economic models that make heuristic assumptions such as constant preferences.

Friedman does not see disadvantages for economic modelling in all three cases of “unrealisticness”. In the first case, unrealistic assumptions are unavoidable, in the second case going beyond observable reality is necessary for interesting models. The third case leaves open if the “unrealistic” heuristic assumptions are fruitful or not – the fact that an assumption seems implausible is however no good argument against it *before* its implications have been explored<sup>19</sup>, otherwise many scientific discoveries such as Galilei’s laws of falling bodies would have never been made.

All this shows, that there is indeed support for Friedman’s view, that most important thing about assumptions is not their (seeming) realism, but the predictive success of the models that rely on them, because it is hard to judge the adequacy of the assumptions before the implications have been checked.

Even if he emphasizes the relevance of predictive power, Friedman is fully conscious that prediction is unachievable with a high degree of precision in economics. When he stresses predictive success as quality criterion, he has rather pattern-prediction in mind than precisely forecasting stock prices.<sup>20</sup>

Besides that, he underlines more than once that science is about *explanation* which is why the predictions can be actually “retrodictions” which explain phenomena that have already happened in the past.<sup>21</sup> This is one reason why the standard instrumentalist interpretation, which assumes

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<sup>19</sup> The rational choice assumption often leads to false implications and cannot be fruitfully adapted to some economic problem fields such as innovation economics, but as there are many problems that *can* be tackled (if only to a certain degree) by the (implausible) rationality assumption (see e.g. the works of Gary Becker), it should be clear that this assumption cannot be rejected beforehand by calling it unrealistic.

<sup>20</sup> See **Friedman** (1953), p.40.

<sup>21</sup> See **Friedman** (1953), p.9.

that Friedman must recommend theory-free correlation-processing if this would lead to precise predictions, is inconsistent with the text.

The foregoing interpretation should have made clear, that Friedman is supporting a normative position that can be best characterized negatively at this stage of the argument: The wholesale demand for more realistic assumptions misses the point of many economic models and does not automatically lead to better modelling.

The central argumentative twist of Friedman's approach is accepting that economic models are made for solving certain problems and not for finding "the truth". This does not deprive economics from dealing with highly abstract and theoretical issues. However, if a specified problem is given, it is far more easy to discuss about the means for solving it, than to discuss about "normativity in general" as it was done in classic philosophy of science, which tried to demarcate science from non-science in general or aimed at developing universal criteria for progress. The analysis of aims and means, i.e. finding the right models for a given aim, does not lead to a loss of normativity or to relativism, rather the opposite: Only if aims are set that can be defined more precisely than general terms such as "true", "scientific" or "progressive" one can discuss in a meaningful way about theory evaluation.

Friedman discovers as early as 1953 that there are many competing aims in science and that predictive accuracy is not the only point of scientific enquiry: Depending on the problem even a less precise theory can be preferable, e.g. if it is easier to apply. Even theories that already have been (constructively) falsified such as Newtonian mechanics are still used even today for this reason. This shows that Friedman's argumentation is essentially an *economic* one when it comes to theory evaluation: He asks what we gain by a new theory or more realistic assumptions compared to its costs relative to a given problem.<sup>22</sup>

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<sup>22</sup> See **Friedman** (1953), p.17.

Of course, such pragmatic elements can be found in Friedman's essay only in vague and unsystematic form, which is probably the reason why the "right" interpretation of his essay is still on debate today. Our pragmatic interpretation seems however convincing for several reasons:<sup>23</sup>

1. Friedman carefully avoids speaking of "truth". He tries to focus on solutions and explanations that work – whether they are ontologically true is not relevant for him and probably impossible to discover for everyone. His avoidance to speak about truth also shows that he is no instrumentalist who would claim that theories *cannot* be true but are mere tools. He just rejects the relevance of the truth-question for theory-evaluation.
2. Friedman's approach strictly focuses on problem-solving. He is far more radical in this than Popper, who used the term "problem-orientation" as well. In contrast to Popper Friedman allows employing already falsified theories, when they can be useful for a certain class of problems.
3. Friedman's talk of comparing costs and benefits of theories fits well the pragmatic/economic line of interpretation.
4. Friedman does not exclusively concentrate on predictive success as quality criterion but underlines the importance of pragmatic criteria such as explanatory power, fruitfulness and simplicity.

### **3. Application: Descriptive Assumptions yes or no?**

With the pragmatic interpretation of Friedman's methodology set out in quite some detail, how can we use it for the discussion of ABMs? In the

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<sup>23</sup> See **Hirsch/DeMarchi** (1990) for a detailed analysis of the pragmatic elements in Friedman's methodology. Particularly they argue for the thesis that Friedman was heavily influenced by John Dewey's views. Hence, the term "pragmatic" is to be understood in Dewey's sense in this text. However, it is not to be understood in the sense of a pragmatic theory of truth, but meant to shift the focus to the truth-independent usefulness of theories.

following, we shall use our interpretation of Friedman for delivering a critique of simulations that rely, in our opinion, too heavily on empirical data. Following a suggestion by Moss/Edmonds we distinguish two antagonist simulation approaches called KISS (Keep it simple, stupid!) and KIDS (Keep it descriptive, stupid!). For the sake of a more focused discussion we equate KISS with Friedman's view that the realism of the basic assumptions of a model is not a good criterion to judge it and KIDS with the opposite view, claiming that only models that are as descriptive as possible on the assumption side are likely to generate useful scientific insight.

One of the first examples of simulation in the social sciences are the checkerboard segregation models by Thomas Schelling and they constitute a paradigmatic example for KISS.<sup>24</sup> A short summary shall suffice here to introduce the main idea: In Schelling's segregation models, black and white stones are distributed on a checkerboard, symbolizing the black and white inhabitants of a (north-American) city. Now a certain threshold-share of stones in the neighbourhood that have a different colour from the stone under consideration is defined (e.g. 70% have a different colour). If this threshold-share is reached, the stone is said "to feel uncomfortable" and as a result it moves away from its original position to the next free spot available. The astonishing result of such a simplistic model was, that even if the threshold-share requires only 30% of stones in the neighbourhood having the same colour, complete segregation of the colours on the checkerboard is the result.

Now modelling in such a simplistic way is clearly against the KIDS suggestion to include as much empirical data as possible in the assumptions. However, in accordance with our interpretation of Friedman we think that descriptively false assumptions (such as checkerboards as representations of cities) are not necessarily a disadvantage of a model. In the

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<sup>24</sup> See **Schelling** (1971) for the locus classicus.

following, we will broaden Friedman's arguments by extending them to a defence of KISS modelling against the KIDS approach.

### **1. Simplification is necessary and inevitable.**

Full realism is neither achievable nor is it desirable.<sup>25</sup> Stressing the need to include as much data as possible seems to suggest that we can come close to full realism by using empirical data where it is available and keeping the model general where it is not. However, it remains largely unclear how a model can be "left general" at all. ABMs are by definition models that assume a certain behaviour for agents in a simulation. Leaving an aspect of behaviour completely general would imply the inclusion of theory-free aspects of agent behaviour, which means nothing more than introducing random elements in the model (or recurring to ad-hoc assumptions without being aware of it).

A possibility to deal with this issue is to calibrate the aspects where a model is "left general" by running the simulation numerous times and thereby varying the parameters by means of a Monte-Carlo approach. This implies the belief that the right assumptions can be found in a quasi-automated manner. It seems, however, far too optimistic to believe that descriptively adequate models can be generated in this way. No matter, if we use empirical data for setting up the assumptions or if they emerge after calibration: theoretical considerations heavily influence the process of modelling. First, every observation involves theoretical pre-assumptions, hence there is not such a thing as pure observation. Even worse, transmitting the observations into program-code as is done in ABM modelling involves even more decisions, which sets limits to an accurate reproduction of the observable reality. If the model is fitted to observation by comparing the results of several runs with the observed patterns in reality,

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<sup>25</sup> Note, that even the "toughest" sciences like physics make heavy use of idealisations or false assumptions. Just think of planets as "mass-points" or laws that apply only in vacuum. Without radical simplification, many of the basic laws of physics would have never been found.

there is even a third layer of theory involved, that is the standard by which the model results are compared with reality: Often the results of a model need heavy interpretation or statistical analysis so that a comparison to reality is far from straight forward.

All this shows that it can be misleading to claim highly descriptive assumptions, because a high degree of accuracy is never achievable, due to several inevitable restrictions of theory-ladenness. A really high level of descriptive accuracy in ABMs would require a thorough understanding of all the processes involved, so it becomes hard to see what there is left to be learned from actually building the model. In practice, it is more likely that we do *not* understand the processes under investigation to a high degree, which makes approximation and estimation inevitable. In a highly complex model this leads probably rather to potentiating the errors than to highly accurate predictions.

So when a high level of descriptive accuracy is suggested this gives no guideline at all for judging exactly *how* high this level should be. KISS modelling is clearer at this respect because it does not judge the assumptions in terms of descriptive accuracy at all. Besides, making “heroic” simplifications might be more honest than making some necessary simplifications while still claiming high descriptive accuracy.

## **2. Good models explain much by little**

Even if we grant that it might be possible to build descriptively highly accurate models, there are other arguments why the usage of false assumptions is sometimes preferable to highly accurate ones: Very fundamental and counterintuitive effects (such as complete segregation being caused by only mild preference for the own colour in the Schelling models) can get easily out of sight if a much more realistic and less schematic model is used. The main argument of the KISS-advocates points to the core of the idea of modelling: we make models in order to *reduce* the complexity of the real world, not to mirror it. Of course, good models do not

*neglect* the complexities of the systems they try to represent, but striving towards realism in every aspect means nothing else but the rejection of theorizing which can result in a mere collection of facts that may be descriptively highly accurate, but rarely helps explaining matters.<sup>26</sup> This is an argument why the call for more realism cannot be sustained as per-se-argument. It depends on the aim of the model, if the level of abstraction is rightly chosen. When the understanding of fundamental mechanisms is the aim, the KISS method still seems the approach of choice. Highly complex models may accurately *generate* output, but they do not enable scientists to understand how it comes about. Complex models often develop their own life and produce artefacts, which makes them difficult to interpret. A famous example is Sugarscape, which not even aims at accurately reproducing a real society, but is more like a sandbox toolkit for social scientists. In an elaborate version the Sugarscape worlds becomes unpredictable and can lead to greatly differing outcomes at the macro-level even when started with the same initial conditions.<sup>27</sup> A similar case makes a model of domestic water demand by Moss/Edmonds. Even if it is far away from being descriptively accurate, which once again shows how difficult this “standard” is to achieve in practice, it produces dynamics where the only observable regularities are caused by the external shocks that are programmed into the model, which is why it is not very helpful for explaining the observed dynamics.<sup>28</sup>

It seems hard to learn anything from models whose dynamics are not fully understood. The ability to explain much by little is therefore not only a pragmatic advantage, but has epistemic relevance as well.

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<sup>26</sup> The term “explanation” is itself under philosophical discussion. The covering-law model of explanation is generally considered outdated due to several difficulties. We do not enter in this philosophical discussion here, but stick to a common-sense interpretation of explanation which is approximated well enough by the covering-law model.

<sup>27</sup> See **Epstein/Axtell** (1996), p.92 footnote 32.

<sup>28</sup> See e.g. **Moss/Edmonds** (2004), p.141.

### 3. Simplicity is an economic value

Even from a much more down-to-earth point of view, there are advantages of keeping the assumptions simple instead of trying to make them descriptively adequate. Simpler models are not only easier to understand, but they are more tractable as well.

In a highly complex model errors are more difficult to trace, the model is easier and cheaper to validate, it is probably easier to adapt to new situations and it leads more quickly to solutions.<sup>29</sup>

From an economic/pragmatic point of view there is no such a thing as truth; models are tools for solving problems. Seen like this, simple models are clearly preferable to complex ones, if (and only if!) they achieve the same quality of solution for a given problem. Advocates of highly descriptive models are in charge for explaining the advantages of their models in terms of predictive and explanatory power when descriptive accuracy or truth are rejected as valid criteria due to a pragmatic methodology like Friedman's.

It is highly important to note, that this does not mean that all models should be as simple as possible. They clearly should not. As we stressed throughout the paper, models need the *right* level of complexity for the problems they tackle. So while simplicity is a value for models, it is surely secondary compared to the model's ability to contribute to a better understanding of the phenomena under scrutiny. Nonetheless, taking the value of simplicity seriously, means that starting with descriptive accuracy as first criterion is probably the wrong way for building helpful models.

## 4. Conclusions

1. The quarrel about the truth of assumptions is highly misleading. No theory and no model rest on true or descriptively accurate assumptions. Modelling is always centred on a specific problem.

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<sup>29</sup> See Chwif/Barretto/Paul (2000), p.452.

Whether the right level of complexity was chosen can be properly assessed only in retrospect.

2. When models aim at predictive accuracy, more refined assumptions are probably needed compared to models, which aim at reproducing stylised facts. The advocates of a more descriptive modelling approach are right to point at the difficulty of comparing simple models to reality. This task does not necessarily become easier when models are based on empirical data to a high degree, though.
3. For economic reasons it is more useful to try finding a simple model first and improve it when successful or replace it by a complex one when not.
4. Theories do not emerge out of empirical description. Modelling is necessarily a creative process that involves construction and hypothesizing. Therefore, theoretical elements must be included in ABMs as well, otherwise they are not likely to foster our understanding of the way the world works.

In accordance with authors emphasising a high empirical orientation, we hold that high generality is incompatible with models that make massive use of empirical data. We agree that abduction is the best way to characterise model building, but we contest the view that this requires the modeller to include as much data as possible when setting up the first version of the model.

As Brenner/Werker repeatedly underline, one advantage of simulations is the possibility to go easily back and forth between assumptions and their implications.<sup>30</sup> This is exactly the point of abductive modelling, but it does not lead to the conclusion to start with a highly complex model, but in our opinion rather the opposite: Start with a simple model and go on refining and calibrating it by going back and forth between assumptions and implications. In this way, models could be created that are based

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<sup>30</sup> See e.g. **Brenner/Werker** (2007), p.230.

on simple and abstract assumptions, but are nonetheless robust to various changes in parameter-settings. These models would be justified by a kind of reflective equilibrium: the assumptions are justified by balancing their prima facie plausibility and simplicity on the one hand and the implications they yield on the other. If such an equilibrium between simplicity and complexity is reached, the resulting model can surely count as the best explanation for the phenomena under scrutiny, which allows for the abductive step to accept it. Abduction, in the words of Charles Sanders Peirce as cited by Brenner/Werker means “*studying the facts and devising a theory to explain them.*”<sup>31</sup>

This shows that explanation *by theory* is the main point of abduction.

Using the empirical data everywhere it is available and keeping the model maximally general where it is not, will not lead to models that are simple enough to offer understandable mechanisms that can be called theoretical explanations of social processes. For meeting this goal, the usage of well-trained theoretical intuitions seems unavoidable.

Brenner and Werker surely agree with us on all this. The only difference seems to consist in the starting point of modeling: While Brenner and Werker recommend starting with descriptive assumptions, we recommend starting with simple assumptions and hence entering the process of going back and forth between assumptions and implications at a much earlier stage of model building. We also believe that this is the essential message of Friedman’s methodological essay: Assumptions should not be judged on their own, but by looking at their implications as early as possible, making model-building a process of continuous revision.

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<sup>31</sup> See **Peirce** (1867), 5, p. 145.

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