

POLEMIKA

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ARE THE CONCEPT OF CAPACITIES AND *CETERIS NORMALIBUS* CLAUSE REDUNDANT?

ABSTRACT

The text is supposed to be a critical response to Łukasz Hardt's paper on *ceteris normalibus* laws. Author especially criticises three main Hardt's theses: 1. Economic laws do not describe regularities, but refer to capacities and powers; 2. Economic laws are only true in theoretical models; 3. Economic laws are valid *ceteris normalibus*, rather than *ceteris paribus*. Based on several examples of theoretical models in economics Author argues that: 1. We cannot abandon the requirement of regularities being the necessary component of any scientific law, economics including. The concept of capacities, even if helpful in reasoning on causes and outcomes, is methodologically redundant; 2. Economic laws cannot be true only in theoretical models. They must be (at least within the range assumed by the researcher) true in the domain represented by the particular model. Otherwise, the notion of "laws true only in a model" refers to the inherent tautologies, which truth value are checked exclusively by assumptions and adopted inference rules; 3. The term *ceteris normalibus* in Hardt's account is redundant because it simply represents a more general set of assumptions, including *ceteris paribus*, *ceteris rectis*, *ceteris absentibus*, *ceteris constantibus*. As long as the "normal" circumstances are defined in a model, the clause does not improve our understanding of models.

Keywords: economic models, economic laws, regularities, capacities, *ceteris paribus*, *ceteris normalibus*.

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This paper was inspired by Łukasz Hardt's text, published in the same special edition of "Studia Ekonomiczne", and is meant to be a critical response to at least some of the crucial Hardt's theses, namely:

1. Economic laws do not describe regularities, but refer to capacities and powers;
2. Economic laws are only true in theoretical models;
3. Economic laws are valid *ceteris normalibus*, rather than *ceteris paribus*.

I must admit at the beginning that I strongly appreciate the depth of Hardt's insight into the problem of the ontology of economic laws, and I respect his stance as a scientific realist (though mine is different). Nevertheless, after reading his text, I have an impression that either there is a deep misunderstanding about very fundamental terms and concepts, or his theses, as indicated above and explained in the paper, are indefensible, even in the light of scientific realism. It would probably be easier to critically review his text from instrumentalist's or constructivist's point of view, but it would not contribute much to the current philosophical debate, as the arguments on both sides are pretty well known. Besides, it seems that the contemporary realism, especially presented by Uskali Mäki, who takes into account the purpose of the theory, its language and its possible perception by the audience, does not fall much away from the moderate instrumentalist's view. My point here is that those three theses, as they are put forward and argued by Hardt are not defensible regardless of the ontological stance we occupy, and at least in reference to the first thesis, it seems to contradict other statements found in the paper.

ECONOMIC LAWS, REGULARITIES AND CAPACITIES

Intuitively, the fact that economic laws (assuming they exist, as Hardt also questions their existence (Hardt, 2017)) do not describe regularities, or even that they have nothing in common with regularities sounds odd. Each scientist who investigates the realm of her / his studies with empirical methods knows very well that to formulate a law or a law-like statement requires the demonstration of the certain repeatable connection between the investigated factors or variables. Econometricians know the most about it. But even the classical and neoclassical economists, who constructed their theories on the deemed rational *homo economicus*, based on regularities, which were repeatable behavioral patterns of a rational agent on the market. They were falsely assumed, but they were. So could it be that Hardt detaches the regularities from the concept of economic law? Reading the paper, one cannot conclude anything different. We read among others:

Here comes second understanding of economic laws, precisely they do not describe regularities, but they refer to capacities and powers.

(...) we should not be surprised that our attempts at understanding cp-laws in economics in terms of some regularities usually fail. What we should focus on is the real domain of economic reality where such entities as the following ones are present, namely powers, mechanisms, tendencies, and structures.

He even goes further and claims that we may have a (valid?) economic law, based on tendencies, where the tendency is dormant, and we do not observe even one occurrence of the expected outcome.

As a given tendency may be dormant and thus it is not to produce a particular (or anticipated) result, in a similar vein we may have type-causation between A and B without any manifestations of tokens a and b.

The above quotations do not leave any room for doubts. According to Hardt, in economic laws regularities are of no importance, are redundant. This strong claim is contradictory to the accounts of philosophers, including realists referred to in the text. It also contradicts this part of the text where Hardt tries to explain how we know about capacities. Let us start from the *argumentum ab auctoritate*. This argument is so commonly mocked as commonly used, and commented paper has a lot of quotations from undisputable authorities in the domain.

The concept of capacities comes from British philosopher of science, Nancy Cartwright. Though she claims that it originally goes back to Mill and even Aristotle, her account is the most comprehensive. Cartwright's idea is much like Hardt's, but not exactly the same. She would probably never defended the view that in a scientific law, the regularities do not count, even in reference to economic laws. Her concept of a scientific law is based on the so-called "nomological machine" which is

(...) fixed (enough) arrangements of components, or factors, with stable (enough) capacities that in the right sort of stable (enough) environment, give rise to the kind of regular behaviour that we represent in our scientific laws (Cartwright, 1999, p. 50).

Regular behavior is a necessary feature of the nomological machine and a necessary feature of a scientific law. Nomological machines can be artificially constructed or can be observed in a particular environment in nature. Their main purpose is to let us change the environment, namely to intervene, to compose causes to produce the target effect. Without regularities, it would be impossible. What distinguishes Cartwright from other philosophers, especially those inclined on the one side towards scientific constructivism and on the other side towards scientific fundamentalism, is that she claims that "necessary regular association between properties" is not enough. We need to understand the arrangements of capacities that give rise to them, and those capacities are real. Moreover, laws are not universal. They work only in the special settings which are the nomological machines.

Laws of nature hold only ceteris paribus – they hold only relative to the successful repeated operation of a nomological machine.

The example of economic laws, where the capacities are dormant and the expected outcome is not produced although the setting is just right, is an example of the nomological machine not running properly, what may imply that the laws are not observed, and the underlying capacities were misidentified. This statement seems to be pretty obvious for most economists. If the monetary mechanism does not work as it is supposed to work according to the commonly accepted laws, e.g. if lowering interest rates does not lead to higher investments, and it does not lead to them even in one singular occurrence, it does not mean that the tendency is “dormant” (whatever it means). In terms of Cartwright, it means that we are unable to construct the running nomological machine according to our previous blueprint, and it further implies that the assumptions for the machine blueprint might have been false, and the deemed capacities were misread or they changed.

Hardt knows it too. But he seems to detach the ontological claims from the epistemological ones. In a different section of his paper he tries to answer the question, how we know about capacities. Out of our empirical experience, of course. It reads:

First, we need special arrangements where capacities can show up. Second, measurement of their effects is necessary. Third, capacities can be deduced from probabilities, or, to say more precisely, probabilities can offer us hypothesis concerning capacities' existence.

In this passage the contradiction between the ontological and epistemological claim is clear. The capacities must show up, and even more, they must show themselves so that we can measure them. Dormant capacities are unobservable and even less measurable. If we deduce capacities from probabilities, then providing that we are talking about classical probability calculus and rules of inferences embedded therein, we clearly need regularities to deduce anything from within. Taking the above into account, it seems that eliminating the expected outcome occurrence and the regularities from the concept of economic law is impossible (unless we assume that no such thing exists like economic law, which is another Hardt's disputable claim expressed elsewhere (Hardt, 2017)).

However, instead of pondering over the ontological components of the concept of economic law-like regularities, we may focus exclusively on its correct expression or description in language. From this point of view, we may benevolently read Hardt's thesis as referring to the mode of expression and not to the necessary components of economic law. In this understanding, very close to Cartwright's account, laws are about capacities, not because they do not refer to any regularities, but because those regularities occur exclusively in nomological machines and beyond them, they are rarely met. Thus laws are not universal, and whenever we need to take our observed associations beyond the setting of a particular nomological machine, we need a concept of capacities. Instrumentalist would probably say that it is purely the problem of our language whether we express laws in terms of capacities or regularities, which occur with the different

ratio in different environments, and in certain environments, they do not occur at all. Both expressions are in any case replaceable. Other philosophers, however, would not agree, rightly indicating that to intervene in nature effectively, regularities are not enough. We need the concept of causation or/and mechanism. I would even assume that they might be right. But do we need capacities and are they ontologically valid?

Our philosophical generalizations depend very often on the examples we deploy to illustrate them. Cartwright's account, as well as Hardt's, is very rich in examples. However while Cartwright uses examples of scientific theories or nomological machines which are well recognized and work good, like Newton's laws of motion and the solar system, Hardt uses examples of economic laws which seem to fail (especially after the recent financial crisis), like monetary mechanism and deemed relation between the interests rate and investments. Therefore, Cartwright requires the repeatable functioning of the nomological machine to deduce on capacities, while Hardt does not. But both agree that capacities are true and valid. Let us, however, focus for a moment on different examples of models in economics, also connected with monetary flows and recent financial crisis. Economists (at least some of them) rightly concluded that if contemporary models were unable to spot the significant signs of the coming crisis, and even less to instruct the policymakers on how to struggle with it, we should work out better models. Some of them are quite peculiar and seems to be constructed on the entirely different ontological assumptions. In the paper from 2010, Billio et al. (2012, p. 535) proposed a model of measuring the so-called systemic risk. The concept of systemic risk in such models is itself an interesting issue. Instead of investigating the linear, causal relations between the quantifiable variables, systemic risk is embedded in the complex, numerous interconnections between market agents. Those interconnections evolve over time according to the evolutionary logic (i.e. mutation, imitation and adaptation to the dynamically changing environment being the most important factors), and they may, or may not enter the dangerous, irreversible path leading directly to the global financial crisis. The task is to spot the moment (or pattern) signifying the coming disaster. Billio et al. focused their research on the financial agents, namely hedge funds, banks, brokers and insurance companies and they reconstructed the map of their connections, taking into account their monthly returns. They claim to reveal certain causal relations among them, and that their model "can also identify and quantify financial crisis periods, and seem to contain predictive power for the current financial crisis."

Another interesting example, not based on the evolutionary logic, is the model constructed by Nyman et al. (Nyman, et al., 2018). Contrary to the mainstream economists, they decided to study the possible relation between the broadcasted news and certain macroeconomic indices (like consumer sentiment index, economic policy uncertainty index, PMI and others). They built their model on the social-psychological theory of "conviction narratives", which broadly assumes that narrative drives human action. They investigated three

sources of news, i.e. Bank of England's daily reports, Reuters, and brokers' reports to measure the so-called "relative sentiment shift" (RSS). They claim to discover a strong correlation between the sentiment measures and financial crisis, where RSS falls in advance of the crisis and even in advance of the other macroeconomic variables, which seems to lag it rather than lead. It may imply the causal relation so that broadcasted information can be one of the essential causes of the financial crisis.

What lessons may philosophers learn from those models? They are undeniably economic models. They are far from being the blueprints for well-functioning nomological machines. But at least they are built on certain regularities, and they reveal a certain level of predictive power. Are we, however, able to identify in those models the searched capacities or tendencies or the nature of things? In the case of agent-based models constructed according to the evolutionary principles, I would say that it is impossible. One cannot say that in the nature of interconnectedness between the financial institution is financial crisis creation, because it is clearly both not true and not informative (too vague). The only nature (actually assumed in the model) is its evolutionary dynamic. In the case of RSS, it could be stated, that it is in the nature of the financial market that narrative drives the agent's behaviour. But instantly two questions emerge: First, "capacity" so identified does not seem to be compliant with a set of standard economic models, where the agent's behaviour is driven mainly by economic incentives (like interest rates). If capacities may have truth values, which one is true? Second, do we really need the ontological concept of capacities in such models? Is it not enough to conclude that we (most probably) have discovered the additional, hitherto disregarded causal factor, which seems to act stronger than others? It is not my intention, at this stage to go deeper into this hard, metaphysical dispute. But I claim that the concept of capacities, so apparent in models based on linear equations, recedes in more complex models, where the pattern emerges out of interconnectedness of unquantifiable variables. The attempt at answering the question why there are patterns at all, or why they produce any regularities is one of the most troublesome in the scientific ontology. Whether those are capacities which are responsible for them, or the mathematical structure of the world¹, or propensities² is another story, and all these hypotheses are ontologically very strong and undoubtedly interesting. Methodologically they are however redundant. We do not need this strong ontology to make good science. But we cannot make any good theory or construct any good model without the observed regularities or repeatable patterns, economics including. Models presented above are supposed to illustrate the vagueness of the concept of capacities and its speculative nature.

¹ The concept of mathemacity of the world is among others promoted by Michal Heller and is often presented as the justification of the effectiveness of inductive reasoning in science (See: Heller, 2006).

² The concept of propensities is one of the interpretations of the probability theory given by C.H. Pierce and developed by K. Popper. More can be found in: Zaluski, 2008.

TRUTH IN THEORETICAL MODELS

Whenever Hardt writes about theoretical models in economics, I have an impression that under the term “model” we understand something different. According to some passages in his text, his view on models seems to be based on the typical realistic approach. Defining what model is he quotes Mäki:

*Agent A
uses object M as
a representative of some target system R
for purpose P,
addressing audience E,
prompting genuine issues of resemblance to arise;
and applies commentary C to identify and align these components (Mäki, 2009).*

In some sections, he seems to stress the importance of the above-underlined features of model’s representativeness and resemblance to the target system. We read:

(...) the closer a given empirical domain to the model’s structure is, the higher probability that the model’s insights (i.e., economic laws) are to correctly explain the workings of such a domain. Nevertheless, isomorphism between models and empirical domains is never perfect and thus economic laws only describe tendencies in economic realm.

(...)

I am to show however that discussing laws without referring to models and empirical phenomena is simply impossible.

So, one might have an impression that the genuine tester of the hypothetical truth value of the model is a relation, so far unspecified, between the model and the said empirical domain. However, reading his further text one may deduce that his models lack any reference to the outer world. He writes:

Now, let me recapitulate the main findings of our study into the very meaning of ceteris paribus clause. It seems that the only uncontroversial way to successfully defend such laws is just to claim that a given cp statement is only true in a model used for its “production” or, in a second case, if one have a cp law, no matter of its origin, then one can always construct a model in which such a statement is to be true.

(...)

Having in mind what has been said above, one should agree that economic laws (usually stated with ceteris paribus clauses) can be understood as the laws always true in economic models, and hence ceteris normalibus is just to be conceptualized as being synonymous to “in a model” phrase. So, for instance, saying that ceteris paribus lower interest rates are to stimulate investments can be rephrased that lower interest rates always stimulate invest-

ments only in theoretical models where such a relation holds or, in other words, *ceteris normalibus*, lower interest rates stimulates investments.

If one can always construct (a good?) model in which a *ceteris paribus* statement (any given cp statement?) is true, then one can “produce” any law. The example with interests rates always stimulating the investments only in theoretical models is especially worrying because if the stimulating effect works always only in a theoretical model, it does not work always in the target system. Consequently, the important causal connection is detached from the empirical domain. Either we have here another inconsistency in the text, or we have to assume that the postulated closeness or resemblance to the given empirical domain does not refer to economic laws.

To cope with this claim, I need to make a few remarks on models. Regardless of the vast and unfinished discussion in philosophy what they exactly are, two features seem to be common in most accounts, Mäki’s including.

First, models model something. There should always be a reference to a kind of universe which is supposed to be modelled. This universe is often called (as above) the target system (Mäki, 2009). In economics, this target system is the economy or its selected part. Models without the target system are either not models in this meaning or can be at most the interesting, entertaining game for mathematicians, and until they do not find its target system in the human economy, they are useless for economists. Special emphasis should be placed on the phrase ‘target system in the human economy’, because we may figure out the model for which the target system is the imaginary economy in Hobbiton, based on the special set of Hobbits’ preferences, who first and foremost wished to make other Hobbits happy by donating them gifts. The equilibrium is reached when for each Hobbit-agent the value of gifts given, equals the value of gifts received. Is it an economic model? We may say it is. Is there truth in this model?

This question leads us to the second important feature of models. Models cannot be true or false. If we interpret the model mechanically (like Phillip’s hydraulic model of economy) we may say that the model accurately or inaccurately represents its target (Reiss, 2013). If we interpret a model as a set of sentences (expressed in the natural language or in mathematical equations, what is the case in most of the economic models), we may ascribe, at most, the truth value to the particular sentences. The components of such models may have different functions. In purely mathematical models we have constants and variables, sentences representing assumptions (axioms), inference rules (law-like sentences or specific causal connections including) and last but not least, the sentences representing the outcomes of applied inference (predictions). Mäki supplements it by the model’s purpose audience and commentaries. Each of those components may be the bearer of the truth value.

But what is the correct method of ascribing the truth value to the sentences constructing the model? Usually, we test the correspondence level with the rel-

evant sentences describing the given target domain. But some sentences may also be tautologies deduced upon the given assumptions and accepted inference rules.

In Hobbiton sentences expressing the assumptions and causal connections (law-like sentences) are fictitious, so their truth value is 0. They do not correspond to any known human economy. Therefore we may only check if our model is internally complete and consistent, i.e. to test if given assumptions and causal connections can provide an outcome for each value of a variable and if outcomes are consistent (non-contradictory). If the model is internally complete and consistent, we may say that the fictitious economic law is “true” in a model that means that it provides in each iteration non-contradictory outcomes. The sentences in such model which use the fictitious economic law as the inference rule are tautologies. This is the only model of which I can say that “economic law is true only in it”. Therefore, it looks like Hardt’s claim that “economic laws are true only in theoretical models” sends us to Hobbiton.

Another, widely accepted method of testing models’ truth value, is setting the sentences composing the model against sentences describing the corresponding observed phenomena³. We are not able to test directly the law-like sentences, as they are usually hypotheses constructed upon the set of assumptions and disclosed regularities. But we can do that in reference to the statements representing model’s assumptions and to the statements representing outcomes (predictions). Which one are more important in the evaluation of the economic models, is a subject to a dispute referred to, among others by Milton Freedman in his famous essay (Friedman, 2008). Empirical constructivists might say, that both assumptions and law-like statements are of no relevance in respect to their truthfulness, only the accuracy of the predictions counts. If the level of this accuracy is satisfactory for the model’s constructor, taking into account her/his purposes, the model works, period. If it does not, we are free to test any other combination of assumptions and rules. In this account, the truth of assumptions and law-like statements is equally irrelevant, both in a model and in a target system. Others might say that the assumptions are the most important, and if the model does not work, we need to make them more real, to “factualize” them. And realists would rather place the emphasis on the truth (meaning its correspondence to the target system) of the model’s inherent mechanism transforming the input into output, regardless how we call it (causal connection, causal mechanism, economic laws etc.). According to them the economic laws in a given model are true, that means that they sufficiently well represent a certain aspect of a given economic realm.

The phrase “truth in the model” is indeed often used by the philosophers of economics. Hardt quotes Rodrik stating that “Models are never true: but there is truth in models”. And Cartwright writes:

³ For the sake of the current discussion let us for a moment forget about the strong constructivists’ view, that those observables are also constructs, burdened with an observer’s previously accepted theories, values and his social context.

(...) the laws of physics are true only of what we made. (...) I have argued that laws are true in the models, perhaps literally and precisely true, just as morals are true in their corresponding fable. (...) Even when the scientific model fit, they do not fit very exactly. This question bears on how true the theory is of the world. (...) This is the question that bears, not on the truth of the laws, but rather on their universality (Cartwright, 1999, p. 48).

The corresponding fable, which entails the truthfulness of morals, represents the set of assumptions and restrictions explicitly or tacitly imposed on a model. The truth of economic laws in a model may be evaluated only in combination with this fable, but not universally. But they are true both in a model and in a nomological machine built upon it.

If Hardt's intention was to present the same or similar view, his statements as quoted above are highly misleading as they suggest that the crucial component of a model, the most important bearer of the truth value (at least according to some realists) has no, or insignificant correspondence to the target system and remains in the impound of the theoretical model. They also suggest that if lowering interests rate does not stimulate investments, we have no reason to worry. The economic law is still valid in the theoretical model, and the implied capacity truly exists. On the contrary, we should worry. Using the words of Cartwright, in this case, we do not have any well-functioning nomological machine, so we do not have good reasons to claim that the model represents any economic domain and even fewer reasons to deduce any capacities.

CETERIS NORMALIBUS, RATHER THAN CETERIS PARIBUS

What is *ceteris paribus* clause and what problems it generates is extensively discussed in the philosophical literature (Mucha, 2016; Reiss, 2013; Reutlinger, et al., 2015; Schurz, 2014). Hardt's stance on that does not fall much away from other philosophers. He rightly noticed that especially in economics, the "other things being equal" may also mean "other things being absent" (*ceteris absentibus*) or "other things being fixed" (*ceteris constantibus*), or "other things being right" (*ceteris rectis*). So economists, more than other scientists, construct models in which they assume that certain factors (or variables) are absent or disregarded (have no impact on other variables), or they are fixed in advance at certain values and these values remain constant regardless of the changing values of other variables (exogenous- and endogenous). Those absent, disregarded or fixed variables are quite often confused and put under the collective term *ceteris paribus*. Philosophers of science try to be more precise, and they often distinguish between them and between the practical testability of the given model and (in terms of Popperian and post-Popperian philosophy of science) its corroboration or falsification, depending on the exact content and characteristics of those "other

things being equal” (Schurz, 2014). Hardt’s task, however, as he specified in the paper, is not to analyze the exact content of cp-clause in certain or exemplary economic models, but rather to argue that it should be in principle replaced by the *ceteris normalibus* clause. As the latter is neither commonly used in economics or philosophy, it requires definition. We find it in several fragments of the text. The proposed definition is combined with the theses, already criticized above.

Hardt claims that because economic laws can be understood as the laws always true in economic models so *ceteris normalibus* clause should be synonymous to “in a model” phrase. He further refers to the commonsensical notion of “normality” and admits that though law-statement should be conceptualized as only true in normal circumstances, those circumstances can be found only in a theoretical model. Yet, he does not define the normal conditions in any other way. To sum up, what is normal in economics is specified in the economic model.

There are at least two problems with such understanding of *ceteris normalibus* clause. First, as models quite often represent the selected part of economy in circumstances which are rather abnormal (like financial crisis, e.g. above mentioned model by Billio et al.), the concept of “normal circumstances” introduced by Hardt, is apparently different from concepts of other authors, who previously introduced the notion of *ceteris normalibus* clause to the philosophy of science (like Schurz, who is also referred to in Hardt’s paper). Second, as the “normal circumstances” are to be found in a model, they are supposed to be defined therein, and by definition, they include all the factors or variables composing those circumstances, or their assumed absence in a model. Thus, the notion of *ceteris normalibus* designates the set of subsets. The subsets are (supposedly) the assumptions, which has been previously mentioned, quite well recognized in the philosophy of science, being *ceteris paribus*, *ceteris absentibus*, *ceteris constantibus* and *ceteris rectis*. Is it a matter of terms, or does it have any important ontological or methodological implications? No implications can be found in the paper, so Occam’s razor suggests the additional notion is redundant.

But does it mean that the concept of “normal circumstances” plays no part in economic models? They may play, and this is exactly the account of Schurz, but the understanding of the concept is different. Briefly, Schurz would probably never agree that all the conditions composing normal circumstances can be found in a model. The crucial problem is that they cannot because precisely we do not know what they are. To understand this, we need to follow Schurz in his distinction between cp (*ceteris paribus*) laws and cr (*ceteris rectis*) “laws”. The general form of the former is:

An increase in the value of a variable X leads to an increase (or decrease) in the value of another variable Y, provided that the value of all other variables Z, which are not effects of X (whatever these values are) remain unchanged (Schurz, 2014, p. 1802).

In this formulation it does not matter what the values of “all other variables” are, it matters only that they remain unchanged. Unlike Cr laws, which requires “all other variables” to take the “right” values, without explicitly stating what the right values are. The range of values which is “right” is beyond our current cognition. We only suspect that there is a certain range which is required to sustain the stable relation between variable X and Y, and we can indicate the exemplary values, within which the relation holds. According to Schurz this controversy cp laws and cr “laws” is especially noticeable in theoretical economics.

The truth of the law of demand requires not only a cp clause, but also a cr clause: the cp-demand-price relation holds only under the condition of an “ideal market”, which requires that the sellers and buyers are fully informed and free utility maximizers; irrational behaviour and government price regulation (etc.) have to be excluded (Schurz, 2014, p. 1803).

Due to this unknown values of “other variables”, the cr “laws” are not eligible to direct falsification. There is however a possibility of eliminating the cr clause (at least in reference to certain law-like formulations) via its replacement by a normality clause and thus construct the co-called *normic* cr law. In such formulation, the “right” values of the remainder variables are claimed to be the “normal” ones. But this normality is not defined in a model, but outside it, with reference to statistical normality, which further requires minimum regularity observed in the form of “Most As are Bs”. This statistical normality further emerges out of the evolutionary dynamic, which out of the complexity is able to create local orders with the help of self-regulatory, adaptation mechanism.

Let us again illustrate the difference with examples. In the above quotation, Schurz rightly notices that one of the cr requirement for law of demand to hold are free, rational, utility maximisers. As we know from various researches of behavioral economists, rational behavior is far from being normal in Schurz’s terms. So this formulation of the law of demand cannot be expressed with the use of cn clause. But in Hardt’s account, as the circumstances are defined in a model of supply and demand, they constitute the “normal” circumstances, and both cp and cr clauses belong to the cn set.

We may, however, take into account another, empirically tested regularity which can also be represented by the mathematical equation, namely tips earned by exotic dancers in relation to their ovulation cycle (Miller et al., 2007)⁴. This model does not require rational agents, but on the contrary, the agents seem at first sight to be quite irrational, spending more money on lap dancers who at the moment of dancing are at the peak fertility. But outside the model, there is a set of assumptions which refer to the situation which is statistically (evolutionary)

⁴ It is worth noting that the research described in the paper, was so peculiar that authors was awarded with Ig Nobel prize in 2008 in economics.

“normal”. Agents offering tips are heterosexual and are not asexual so that they respond relatively to the attractiveness of the exotic dancers, and their taste for the ladies’ beauty is relatively unified. If we placed the experiment in the “abnormal” situation, e.g. in the gays’ club, or if we engaged to the experiment men with a very exceptional (statistically rare) erotic taste, the regularity might disappear. However, all of those “normality” assumptions are not defined in a model. They are not even defined precisely at all. They are implied out of the normal circumstances, in which the regularity holds.

As Hardt rejects the concept of regularities as a component of laws, Schurz’s account differs from his also in this point. His *ceteris normalibus* laws are different from Schurz’s normic or laws because there are no underlying regularities which they may refer to, and all the variables which compose “normal circumstances” are defined in the model, and not outside it. The *ceteris normalibus* clause in his account seems to be a redundant term which simply stands for all the model’s assumptions.

CONCLUSIONS

1. If we accept that there is something like economic law and that it should have something in common with the empirically tested, observed realm, regardless of our ontological stance we cannot abandon the requirement of regularities being the necessary component of any scientific law, economics including. Especially we cannot replace the regularity requirement with the ontological concept of capacities. Capacities may help us explain why there are regularities in nature at all, and they help us in reasoning about the possible outcomes, whenever we go beyond the precisely constructed or described nomological machine in terms of Cartwright. Methodologically they are however redundant. We use the regular behavior of the nomological machine or any other setting to reason through an analogy about the possible outcome of other settings, which differs from the former in some points. But if there is no properly functioning nomological machine, any reasoning about capacities has no grounds.
2. If a model predicts certain outcomes, which do not occur in a particular setting, instead of consoling ourselves with the concept of dormant capacities we should rather assume that the model significantly misrepresents its target system. Significant misrepresentation, means, that it misrepresents its target system in reference to possible causal connections or in Hardt’s terms, in reference to alleged capacities.
3. Economic laws cannot be true only in theoretical models. They must be (at least within the range assumed by the researcher) true in the domain represented by the particular model. Otherwise, the notion of “laws true only

in a model” refers to the inherent tautologies, which truth value are checked exclusively by assumptions and adopted inference rules.

4. The term *ceteris normalibus* in Hardt’s account is redundant because it simply represents a more general set of assumptions, including *ceteris paribus*, *ceteris rectis*, *ceteris absentibus*, *ceteris constantibus*. As long as the “normal” circumstances are defined in a model, the clause does not improve our understanding of models and makes it even less falsifiable.

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CZY POJĘCIE ZDOLNOŚCI (CAPACITIES) I KLAUZULA *CETERIS NORMALIBUS* SĄ ZBĘDNE?

STRESZCZENIE

Tekst stanowi krytyczną odpowiedź na artykuł Łukasza Hardta dotyczący tzw. praw *ceteris normalibus*. Szczególnie poddaje krytyce trzy główne tezy: 1. Prawa ekonomiczne nie opisują regularności, ale odnoszą się do zdolności (*capacities*) i mocy (*powers*); 2. Prawa ekonomiczne są prawdziwe tylko w modelach ekonomicznych; 3. Prawa ekonomiczne są ważne raczej *ceteris normalibus* niż *ceteris paribus*. Opierając się na kilku przykładach teoretycznych modeli w ekonomii, autor twierdzi, że: 1. Nie można porzucić wymogu regularności, które stanowią konieczny element każdego prawa naukowego, włączając w to ekonomię. Pojęcie zdolności, nawet jeśli pomocne w rozumowaniu dotyczącym przyczyn i skutków, jest metodologicznie zbędne; 2. Prawa ekonomiczne nie mogą być prawdziwe wyłącznie w modelach teoretycznych. Co najmniej w zakresie założonym przez badacza, muszą one być prawdziwe w domenie reprezentowanej przez dany model; 3. Termin *ceteris paribus* w ujęciu Hardta jest zbędny, jako że reprezentuje on li tylko bardziej ogólny zbiór założeń obejmujący: *ceteris paribus*, *ceteris rectis*, *ceteris absentibus*, *ceteris constantibus*. Dopóki warunki „normalne” są zdefiniowane w modelu, dopóty klauzula ta nie poprawia jego rozumienia.

Słowa kluczowe: modele ekonomiczne, prawa ekonomiczne, regularności, zdolności, *ceteris paribus*, *ceteris normalibus*.