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**Robbins' on the stationary state:
an early attempt to distinguish idealization from abstraction**

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Abstract

In 1930, Lionel Robbins warned economists not to confuse the Classical stationary state of an economy with fixed endowments of an economy as conceived by John Bates Clark. The first way of modelling assumes that economies produce a stationary state as a result of balancing forces, the second way of modelling assumes that endowments are stable *by hypothesis* and aims to show that equilibrium can be attained under such conditions. Robbins said (1930, pp.206-7) that '[b]oth rule out inventions and fundamental changes in nature and human beings. But the one admits the possibility of variations of labour and capital, the other excludes these by definition'.

With Heinz Kurz, I hold that Knut Wicksell (1954) used the Clarkian and not the Classical approach in his interest- and distribution theory. For his theory of capital and distribution in the long run Wicksell dropped the assumption of a stationary state and used a comparative static economic model of what actually is a long run dynamic situation.

I shall first show that the two strategies used in Wicksell's (1954) and (1951) are in fact opposed in precisely the way Robbins indicated. Secondly, I shall claim that these two strategies essentially make the difference between idealization and abstraction. As also this distinction is often misunderstood, we can see how a rereading of Robbins' methodological publications helps clarify economists' reasoning strategies even today.

1. Introduction

In 1893 Knut Wicksell published his *Über Wert, Kapital und Rente* (see his (1954) for the English translation *Value, Capital and Rent*) in which he formalised and extended Böhm-Bawerk's interest theory. He praised Böhm-Bawerk's insightful concept of interest, which allows for a positive rate of interest even in a stationary economy. According to Wicksell, Walras had said that

in order to determine the level of interest, it is necessary to turn from the investigation of a stationary economy to the investigation of a progressive one, where new interest-bearing capital goods are produced, whose capital can be determined from the production costs. (Wicksell 1954, 167)

However, Wicksell continues:

In the stationary economy too [...] a rate of interest of the circulating capital will undoubtedly establish itself, precisely because the lengthier methods of production prove more profitable. [...] The merit of having taken the decisive step forward belongs in this field to Jevons and, above all, to Böhm-Bawerk. (Wicksell 1954, 167-8.)

Indeed, the Austrian Böhm-Bawerk is the first economist to perceive the importance of time as a productive factor in a complete theory of distribution. In this theory, interest does *not* come forth as the surplus of the value of new capital over old.

Wicksell wrote his work on value, capital and rent (henceforth VCR) as both a summary and an improvement of Böhm-Bawerk's magnum opus. It is a mathematical interpretation of Böhm-Bawerk's theory in which capital is conceived as a wages fund enabling entrepreneurs to invest in more roundabout – i.e. more capital intensive – ways of production. In the course of intermediary goods' maturing into finished consumer goods in production, they gain in value as a consequence of the technical datum that 'past' goods satisfy more needs than 'present' goods (or, as Böhm-Bawerk alternatively put it, that present goods satisfy more needs than future goods; known as Böhm-Bawerk's 'third cause'). This technical fact has nothing to do with issues of liquidity preference (which comes close to Böhm-Bawerk's 'first cause') or with irrationality of agents on the market (i.e. the underestimation of future needs, the 'second cause'). It has to do with the phenomenon that fishing rods help catch more fish than our bare hands do and that the more sophisticated ways of fishing require investment and, hence, more total labour input. We could say that the wage workers receive is a present good, but the revenues for the investor, who lends from the wages fund to sustain the workers, are future goods. The mere fact that labour hours mature in the production process gives rise to interest, which is the marginal productivity of round-

about production. It is this Böhm-Bawerkian concept of interest that Wicksell fitted into a very brief general theory of production, income, and distribution in a stationary economy. It constitutes the body of VCR.

This work, from 1893, marks what I would call Wicksell's first phase. In his later work, Wicksell tried to develop a broader theory with growing capital, laid down in his *Lectures on political economy* (Swedish original 1901). The stationary state of Böhm-Bawerk had instead assumed inelastic supply of steadily circulating capital. Thus, Lionel Robbins wrote in his introduction to Wicksell's *Lectures on Political Economy* was

able to present an account of equilibrium of capitalistic production which combined all the best features of these apparently divergent theories, and, by invoking the methods of Walrasian analysis, he was able to present it in a much more general setting [...] It is true that this theory itself is not complete. It was fully developed in the *Lectures* only for the case of circulating capital. (Wicksell 1951, xvi.)

Below I shall depict the project Wicksell engaged in, after VCR, when he tried to extend the model to capture a growing economy (section 2). Next, I shall point to the importance for such an enterprise of understanding the differences between a static analysis and a theory of the stationary state and discuss the distinction as Robbins demarcated the two (section 3). In order to apply the concepts of idealization and abstraction to the distinction, I first have to explain the structure of idealizational reasoning strategies in comparison with abstraction in economic theory making (in section 4). It will be maintained that Robbins' concept clarification can be applied for an understanding of Wicksell's strategies in his first and second phase. With the help also of my distinction between abstraction and idealization we can see that Wicksell's strategies turn out to be orthogonal to each other (section 5).

2. Long run dynamics: capital accumulation

A stationary economy can be conceived as one with long run inelastic supply of land and labour. It is feasible to model capital as a homogeneous fund of purchasing power, as Böhm-Bawerk had done. But a growing economy feeds on a growing stock of capital, and to produce capital goods, capital is needed. There is, then, a loop structure of investment. Capital has to be modelled as heterogeneous machines and unfinished products and, hence, cannot be valued in its own technical units. The valuation of capital requires prior information about the interest rate. The so-called Cambridge controversies arose out of the problem that the determination of interest by the marginal product of capital induced the economist to reason in a circle: marginal productivity theory cannot explain the rate of interest without mixing up explanandum and explanans.

Böhm-Bawerk's assumption, meanwhile, had been that land could be treated as a special case of durable capital goods. He hoped that this would simplify the model; something he was in need of because he was incapable of doing what Wicksell managed to do: developing the model mathematically. However, even in a stationary economy, the loop structure of investment already enters at the level of land itself. Böhm-Bawerk had defined capital as piled up (or frozen) land and labour¹, but land, in turn, was taken to be a sort of perpetual capital good. He had calculated the value of land simply as the sum of an infinite geometric series of its future product. This, then, implies that land partly consists of land, contradicting another of Böhm-Bawerk's claims, i.e. that land is an *original factor* together with labour. Due to Wicksell land got its autonomous status in the general theory. He proposed to treat durable capital as a rent-earning good.

The assumption of a stationary economy had allowed Böhm-Bawerk to calculate the average investment period as a measure of the quantity of capital: in PTK more capital is synonymous with longer roundaboutness. But with two original factors, land and labour, some capital may consist of more frozen land, other of more frozen labour. The heterogeneity of capital then props up more pressingly. If the project is to also allow long run growth, tracking down the quantity of capital becomes a real problem. This is the challenge Wicksell faced.

Heinz Kurz discusses the attempts by economists to adjust Wicksell's model, which have turned into the debate on Wicksell's 'missing equation' (Kurz 2000). I am interested Kurz' analysis for his treatment of the difference between being in a stationary state (a property of the world) and the method of a static analysis (a property of our strategies to know about the world), based as it is on Robbins' much earlier distinction. Some passages from Wicksell's work seem to imply strictly stationary conditions, numerous others cause 'this impression [to be] quickly dispelled'². Heinz Kurz concludes that Wicksell did not want to describe stationary economies.

There is additional evidence that Wicksell did not intend to study the problem of distribution in terms of a strictly stationary state of the economy.³

But does the quote really prove that Wicksell was not interested in the stationary state at all? I shall deny this below. Kurz indicates two ways for Wicksell to choose from: *either* capital is measured by some value unit, *or* capital – heterogeneous as it is – is represented by the average period of production in the economy (which is in fact Böhm-Bawerk's solution), or $\frac{1}{2}t$. Kurz says that Wicksell

¹ Note that this definition applies both to durable capital goods with their key role in the capital theory and for the subsistence fund that was assumed in the interest theory.

² Kurz (2000), p.774.

³ *Ibidem*, p.776.

started off using the average period of production as an indicator for the capital intensity of the economy. Allegedly, Kurz goes on, Wicksell wanted to explain the distribution of factor incomes in a static framework, even assuming fixed factor supply. [ZOEK OP WIE DIT BEWEERT](#) But it requires calculating compound, not simple interest to deal with growing capital stock. So Wicksell allegedly did not see the importance of the use of compound interest for the analysis he ultimately was after.

The whole idea was not to stick to the analysis of a stationary state, Kurz claims, but to a comparative static analysis. In that case the use of simple interest is not harmless at all, let alone *essentially* harmless. He says:

While he saw that compound interest was necessitated by the assumption of free competition, he seemed to think that using simple interest involved an admissible simplification and no 'essential alteration'. As we know, this presumption cannot be sustained.⁴

But this, I believe, is not true! Another quote can show this. Wicksell contended that assuming a stationary state makes the use of simple interest harmless, even 'essentially' so:

The Product ϵt , that is to say, the investment period of the capital, can here, however, be conceived as a single variable, so that the expressions undergo no essential alteration, at least when calculating simple interest.⁵

Clearly, *given simple interest*, and only under that condition, the quoted factor ϵt can as well be taken as exogenous. Wicksell did not claim at all that the use of simple interest is of no matter for the essence of the analysis. He knew that the use of simple interest was not harmless *in general*, i.e. in case one wants to study growing economies. In his VCR, Wicksell takes into account compound interest and provides the appropriate equation⁶. Both in VCR and in the major part of his (1951) he explicitly assumes the stationary state. (It must be admitted that, if the investment period and the rate of interest both are large, the deviation between calculating simple compared to *compound* interest becomes very large too. But the simplification is not so serious *in principle* and this is what Wicksell contended as he judged it as admissible.)

Kurz' claim, that Wicksell was not really interested in the stationary state, is untenable if it is taken to describe all phases of Wicksell's his career. He was in-

⁴ Kurz (2000), p.781.

⁵ Wicksell (1954), pp.125-6. He refers here to the fact that the average production period need not be Böhm-Bawerk's $\frac{1}{2}t$, as the distribution of labour and land over the process of production can be subject to choice by the entrepreneur, or to technical conditions, giving rise to a period ϵt instead. The coefficient ϵ supposedly had to be empirically determined.

⁶ Wicksell (1954), p.123.

terested in the properties of Böhm-Bawerk's idealized model with time as the essential factor, even if it described a stationary state. In the last chapter of his (1951) on capital accumulation, the stationary state is finally abandoned. But this precisely marks the next step in Wicksell's research.

3. Robbins on the difference between static analysis and the stationary state

There is a potential confusion over the terms 'stationary' and 'static'. It is possible in principle to treat a stationary state dynamically, viz. to highlight short run market events in an economy at long run standstill. Also, it is viable to discuss growth by a static view if one is interested in the properties of a growing economy *synchronically* and not *diachronically*. Kurz' project is meant to show that much of the research on Wicksell's failure to develop a complete distribution theory for the case of modern, growing, economies is misguided due to its confusing the two concepts. This has induced the researchers to look for some missing equation in Wicksell's work, which is, he says, not missing at all. He draws on the clarification of (and the warning to observe) the distinction of the concepts already expressed by Lionel Robbins (1930):

we must recognise not one general class of "static states" and "static laws", but two: the classical conception in which the condition of stationariness is the resultant of the balancing forces tending to change, and the Clarkian in which the factors of production are stationary by hypothesis, and equilibrium is attained within these conditions. Both rule out inventions and fundamental changes in nature and human beings. But the one admits the possibility of variations of labour and capital, the other excludes these by definition. [...] The modern economist [...] will recognise in the two constructions we have been examining, *not competing abstractions, but successive stages of exposition.*⁷

The Classical economists held a view of the economy as a system that produces long run equilibrium and, hence, of a stationary state until interventions or spontaneous changes in initial conditions reset it; and they wondered about the conditions for such a stationary end state. John Bates Clark looked upon it as a system that produces long run equilibrium *only* under conditions of fixed endowments: he studied the economy under the given axiomatic condition of a stationary state. Thus, Clark's approach is a limiting case of the picture seen by the Classical economists. Kurz, then, notes that Wicksell did not assume a strictly stationary economy in the sense of the Classics, but maintained a static point of view *'designed to throw some light on the actual, growing economy in terms of a comparative static analysis of consecutive states of the economy characterized, inter alia, by*

⁷ Robbins (1930), pp.206-7. Italics in the original.

*different "quantities of capital" in existence'*⁸. Wicksell did ultimately not aim for describing the limiting case. Above I have made clear that I agree insofar as this is concerned with Wicksell's second phase.

It is important to note that it is not useful, perhaps not even intelligible, to speak of 'a static economy'. This is not just because economies are essentially dynamic systems, even if in a stationary state, but because it is only *the method of analysis by which we try to explain* such an economy, which can be static in kind. In other words, 'static' is a property of a method, not of an economic system. The latter can be 'stationary', at least if we refer to a hypothetical economic system.

4. Abstraction and idealization

The distinction between the assumption of stationary conditions and a static mode of analysis is not futile. No economy is in stationary state and, hence, it may be asked what any model economy resting on such a false assumption has to do with real economies. A static analysis of a growing economy, conversely, does not impose any deformation on the model that alienates it from our ultimate interest, the functioning of real economies. A static approach only affects the mode of analysis, not the model economy itself. The distinction has consequences for the issue of realism and truth.

The ubiquitous use of the infamous *ceteris paribus* clause in economics has triggered a fountain of literature discussing how economists could pretend to truthfully explain an aspect of reality that is to form the domain of economic scrutiny, while making use of unrealistic assumptions such as those packed in the *ceteris paribus* clause. The most widely known literature on this subject forms the debate around Milton Friedman's essay 'The methodology of Positive Economics' (Friedman, 1953). In my (Rol, 2008) I have set out to explain the different consequences for the function of scientific claims of on the one hand idealization and on the other abstraction. The use of a clause is a case of idealization. I qualify *idealization* as an epistemic mode by which true propositions can be formulated even though it is by the insertion of a clause which, taken as an (approximate) description of reality, is false. Meanwhile, *abstraction* is a mode of analysis due to which true theoretical hypotheses may be generated without the deliberate insertion of a false claim, like a *ceteris paribus* clause. For the assessment of Robbins' distinction, it is worth while to briefly highlight the core idea of the distinction between idealization and abstraction.

When idealizing, the scientist mentally or materially creates a model world where, for instance, friction has been reduced to zero or where countries' current accounts are balanced. In the first example it is materially possible to create an almost frictionless plane or a vacuum; in the second case it is not possible to

⁸ Kurz (2000), p.777. I endorse this interpretation, but I repeat that I differ in opinion as concerns Wicksell's first phase, the early period in which he wrote VCR.

minimize net trade surpluses in goods and factors. The notorious lack of laboratory situations in economics – and in social science in general – forces scientists to rely on some form of thought experiment: ‘suppose it were the case that this variable decreased to a limit of zero, then it would be the case that the other variable ...’. However, conditional sentences of this sort can be very instructive. In fact, many if not all claims underlying policy relevant talk have this fundamental form. The same ‘if-then’ form can be acknowledged in the everyday sentence ‘if I let go of my cup of tea it will drop’. It is the very truth of this sentence, which induces us to take care. Note that the sentence is not only of a conditional form, but that it has an antecedent which is false as long as I do not let go of my tea: it is a *counterfactual*. The debate over the use of deliberately false descriptions of reality in purportedly true theories can be clarified if it is noticed that idealizational propositions are potentially true counterfactuals – or so I wish to uphold.

Counterfactual sentences have an antecedent which is conditional for the consequent in a *subjunctive* way. This means that there is a reference to an as-if world. Some may think this is a long shot, but I think it isn’t. In daily life we regularly imagine such hypothetical worlds, as we imagine what might happen if we choose one out of two or more options. If the antecedent of the subjunctive conditional is false, which often is the case when they are formed by *ceteris paribus* clauses, we have a counterfactual. Idealizations are counterfactuals; the falsity involved in idealization lies with the idealizational clause in the antecedent of the counterfactuals; and the idealized claim is expressed by the consequent of the counterfactual. As indicated, counterfactuals – although it seems not to fit their name – can be true. Why? Because some claims of the form “if it were the case...” are allowed according to the lawlike behaviour of the world, and some other of these claims are not. To say that in some hypothetical world a particular phenomenon is present is to say that we know what goes on in such a world although it differs from the actual world. We are often well informed – even if perhaps only locally – about the relatively regular make-up of the world we live in. We understand which things will change into what direction and how much if some input variable changes. We imagine hypothetical worlds when we give policy recommendations. If we didn’t, we couldn’t predict the near future even approximately or give any advice.

For example, our awareness of gravity makes us believe that my tea will drop under the condition specified. Likewise, our awareness of the influence of substitutes and complements on our demand for particular goods makes us believe that price stability of substitutes and complements helps fix our individual demand for the good in question. Hence the textbook condition ‘no change in the price of substitutes and complements’ for demand functions.

Now, what I phrase (to some perhaps hyperbolically) as ‘the insertion of falsity’ is often coined ‘abstraction’, e.g. by Milton Friedman (1953). Wrongly so, I

believe. The mathematical idea of a circle is an abstraction of the circle I can draw in the sand in that it is somehow free of many concrete details, such as the thickness of the line or the imperfection of the curve. *Abstraction* removes *concrete* aspects mentioned in a proposition by definition – why else the name? The insertion of a finite clause that sums up which variables are to counterfactually remain unaltered, or zero, or infinite, involves a clear reference to those variables. And as the variables represent concrete aspects of the world, it follows that such a clause cannot amount to abstraction.

So what is a good example of abstraction? An everyday example of abstraction would be to infer that an apple drops from my bag from the assertion ‘a *red apple* drops from my bag’. Here we can see what leaving out concrete detail (redness) from propositions leads to higher levels of abstraction in a discourse. The second law of Newtonian mechanics is extremely abstract in this sense. Of course, lots of prior knowledge is employed in such abstract claims, as in ‘larger groups have a lower change of realization of a public good’ or, to refer to natural science again, ‘both burning and rusting are processes of oxidation’. In scientific abstraction, conjectures regarding structural aspects of a research object are proposed. Nevertheless in such cases concrete details, like the particular public good (is it a dike or is it trade union membership?) or phenomena pertaining to the two types of oxidation (heat and corrosion), are left out of the scientific description. As a result, abstract descriptions are logically weaker than their concrete counterparts. This is an important difference with idealization, which clearly bears on issues of truth.

I shall not dwell on further contemplations about realism and epistemic strategies here. Only the fundamental difference between the use of false clauses and of varying levels of abstraction are of interest for Robbins’ distinction. Idealization amounts to the insertion of false clauses into a proposition (an hypothesis, a theory) but leaves the level of abstraction untouched. This is because clauses sum up the variables to be set at a limiting value. Conversely, abstraction is a particular way of generalizing; to be sure, it is an instance of existential generalization. The full description of a concrete phenomenon includes the details taken to be fundamental of it (although the judgement about what is fundamental or not is meaningful only relative to some context, such as the research interests of the theorizer).

5. Abstraction and idealization in Robbins and Wicksell

In the *Lectures on Political Economy*, Kurz says, Wicksell gave rise to later misunderstandings due to his ambiguous treatment of equilibrium. At one time Wicksell said that (1)

we shall content ourselves with what has been called the static aspect of the problem of equilibrium, i.e. the *conditions necessary* for the main-

tenance, or the periodic renewal, of a stationary state of economic relations.⁹

But at another time he referred to (2)

a fundamental – and simplest – hypothesis [of] the stationary economy in which capital and the other economic factors can be thought of as an approximately unalterable sum¹⁰

The *first quotation* talks of the conditions for the possibility of a stationary state. The *second quotation* involves a stationary economy as the simplest hypothesis and as a mode of thought. In fact, however, in his second phase Wicksell looked for a way to treat growing economies by a comparative static *method*, although he alluded to the ‘stationary state’ to refer to that method.

The stationary state analysis assumes stable endowments, reflected by fixed factor supply. But no such long run standstill economy was like what Böhm-Bawerk and Wicksell observed. In his VCR, Wicksell introduced the assumption of a stationary state as an isolative strategy. He studied a hypothetical economy. This model economy in perpetual stationary state equilibrium, gave him an interesting research object. It allowed him to learn about the sources and level of interest and about the determinants of income distribution, without any form of capital growth. But it did not take him to his final goal. Kurz’ main point shows that it is wrong to take Wicksell as trying to present a stationary state analysis ‘*stricto sensu*’. Indeed, for a theory of capital and distribution in the long run Wicksell dropped the assumption of a stationary state, changed his strategy, so to say, and started of with a static economic model of what actually is a long run dynamic situation.

Robbins (1930) locates the view on an economy as stationary in Mill’s *Principles of Economics*. He paraphrases Mill by the interpretation that “[w]e have studied [in the preceding part of the *Principles*] what happens when the factors of production are constant. Now we must proceed to ask what causes their numbers to change”¹¹ and, next, quotes him: ‘we have still to consider what these changes are and what are their laws’.¹² In conclusion:

[t]he statics should deal with what happens when the factors are given. the dynamics, with the laws of change in the quantity of the factors.¹³

⁹ Wicksell (1951 [1901]), p.105. Italics are mine.

¹⁰ Wicksell (1954 [1883]), p.22

¹¹ Robbins (1930), p.202.

¹² *Ibidem*, p.203.

¹³ *Ibidem*.

Robbins then observes that Clark indebted to Mill in his conception of ('what he calls') a static state by abstracting the forces of social progress. Clearly, here it is worth while to consider my distinction between abstraction and idealization to assess this paraphrasing. 'To study this state', Robbins says, 'we must consider changes of this sort absent.'¹⁴ To consider changes absent, I proposed above, is best rendered idealization, not abstraction. We first consider them, hence we do not abstract from them, and next we give them a limiting value. This limiting value does not accord to the facts, so we idealize by the insertion of a false clause. If it were the case that capital does not accumulate, that populations do not grow, etcetera, then we will have the following stationary state results. That is what the theory can teach us.

Robbins does not marquee the epistemic labels this way, but he does distinguish the two very different modes of analysis.

this is the fundamental difference which is desired here to exhibit – in the one, this constancy is the condition of equilibrium; in the other, it is simply one of the resultants of the equilibrating process. In the Clarkian state, population and capital arte to be constant – they are *not allowed* to vary. In the classical constructions, population and capital are constant, but this is because , together with wages and interest, etc., they have reached a position of rest.¹⁵

Let us express all this in terms of the conceptual apparatus briefly developed in the previous section. In his research after VCR, Wicksell dropped the clause of vertical supply curves so as to further approach what he thought was the kernel of actual economies: growth. Dropping the clause about vertical supply curves is an act of de-idealization: the false clause of a stationary state was relaxed.

But the more sophisticated model was not strictly dynamic, it was an instance of comparative statics. Each step of the analysis was studied under the assumption of an already accumulated stock of capital. The result of every such step was an abstraction in the sense that Wicksell disregarded the flow of phenomena over time. Comparative static analysis is a repeated static analysis. The use of the instrument of successive synchronic views, however, does not imply the assumption of a world in long run arrest. There is no clause involved that does a claim about a hypothetical world without growth. Instead, each synchronic slice of economic reality from its diachronic flux is an abstraction in the sense that the dynamics of the system is ignored, not denied. Nevertheless, the aim of it all is not to ignore but to study it. That is why the mode of analysis is repeated. Wicksell ignored the dynamics but was nevertheless interested in (long

¹⁴ *Ibidem*.

¹⁵ *Ibidem*, p.204. Italics by Robbins.

run) economic development: comparative statics came in the place of real dynamics.

6. Conclusion

When Robbins said about the classical and the Clarkian ways of talking about the absence of long run growth that 'the one admits the possibility of variations of labour and capital, the other excludes these by definition', he traced a difference in a mode of analysis that seems to have puzzled many commentators of Wicksell. When Heinz Kurz notes that historians, who tried to find a missing equation in Wicksell's work, were mistaken because of their failure to distinguish two modes of analysis, he points to the distinction Robbins warned us to observe: stable equilibrium versus stationary capital endowments.

My interpretation is that the first requires abstraction, the second is an act of idealization. I disagree with Kurz that Wicksell did not analyse the stationary state (in my terms: tried to abstract and did not idealize as well). He did, be it only in his first phase; he later used a more classical mode of analysis. What is of much more importance, however, is that *idealization* characterizes Robbins' idea of given endowments and *abstraction* characterizes the idea of ignoring the possibility of fluctuations affecting the state of equilibrium.

As idealization and abstraction are recurrent and well respected modes of analysis in modern economic thought, Robbins' distinction has value for the interpretation of what economists are doing today.

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